# Essays on the Scale-up of Extensions to Cash Transfers

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**Abstract**

This thesis contributes to the scientific and policy debate surrounding the so-called graduation approach – an integrated development intervention that directly targets the poorest households in low-income countries and provides them with a combination of assets at no cost. Though the components vary, they always involve transfers (of a productive and/or monetary asset) alongside extended training and coaching activities (intended to cultivate some intangible asset).

Randomized evaluations of the graduation approach have demonstrated poverty reduction across contexts and in the long run. This has attracted policy attention, as training and coaching interventions could be expanded in widespread social protection schemes that already identify low-income households and provide them with cash transfers. The case for scaling up these intangible extensions could be strengthened through a better understanding of whether and why the integrated approach is more cost-effective than its constituent components, such as transfers alone.

Gaps also remain in the science of implementation. Past evaluations have been conducted mainly in nonprofit pilot settings and may not be predictive of the at-scale effects that will materialize in the institutional contexts of governments. Anticipating that the fidelity and impact of the intangible extensions will tend to suffer with scale, we evaluate two innovations to protect implementation quality. One involves the attempt to identify the most critical ingredients: all else equal, a simpler approach will be more scalable. The other innovation involves the design of the funding agreements: when funders pay implementers as a function of demonstrated impact, scale-up efforts will tend to accelerate where the critical success factors happen to be in place.

We add to recent evidence underlining the potential of the integrated graduation approach. We find that it tends to resist simplification attempts. Paying implementers as a function of demonstrated poverty impacts comes with complications, but some of these are mitigated with scale.

**Notes for the Reader**

The thesis is presented in a three-paper format, with each of three chapters corresponding to an individual research paper. Preceding these chapters is an introduction that grounds the work in contemporary science and policy and motivates the key research questions. Selected elements of the literature review from the introduction re-appear in each of the chapters, which are meant to stand on their own.

Chapter 1 is published in the working paper series by the Centre for the Study of African Economies, co-authored with Munshi Sulaiman and Anuj Shah.[1] As described in the (publicly available) discussion of authorship, I have been solely responsible for all analysis and writing. A revision has been requested by the *Journal of Development Economics*.

Chapter 2 is single-authored and not published, but a detailed pre-analysis plan is public.[2] The data are associated with another trial involving Orkin, Garlick, Mahmud, Haushofer, and Dercon;[3] findings from this trial are not ready at the time this thesis is submitted and data continue to be cleaned. As the data are meant to remain consistent between the two projects, there is a chance that some findings could change.

Chapter 3 is single-authored and published as a working paper on *SocArxiv*.[4] Data from Chapter 1 are used.

Following the three main chapters is a conclusion that summarizes and qualifies the findings, presents policy recommendations, and lays out areas for future research.

The thesis has 186 pages and 56,172 words (including in figures, tables, and footnotes, but excluding references).[5] These numbers include two elements of Chapter 1 (i.e., Appendix III: Specification Curves and Appendix IV: Tables) that are not suited for publication in a journal, but as an online appendix.

[1] poverty-action.org/sites/default/files/publications/csae-wps.pdf
[2] socialscienceregistry.org/docs/analysisplan/2174/document
[3] socialscienceregistry.org/trials/996
[4] osf.io/preprints/socarxiv/hdr78
[5] To estimate the word count inclusive of figures, tables, and footnotes, we multiply the number of words on a representative page (302 words on page 1) by the number of pages.

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Apologies to anybody I may have missed. Any errors are my own.

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## *INTRODUCTION AND LITERATURE REVIEW*

### *POLICY CONTEXT ON EXTENSIONS TO CASH TRANSFERS*

In 2002, one of the world's largest microfinance organizations started implementing a program called *Challenging the Frontiers of Poverty Reduction / Targeting the Ultra-Poor*, or CFPR/TUP (BRAC, 2017). The program had the objective of "graduating" Bangladeshi households out of conditions of extreme poverty and into ones that were thought to allow them to benefit from microcredit (Ahmed, 2009). Other organizations have since adopted similar language to describe comparable programs, including ones that had predated the introduction of CFPR/TUP. Today the term "graduation" is broadly applied to programs that pursue sustained income gains among the poorest households in low- and middle-income countries by providing these with an integrated combination of a transfer (in the form of cash and/or productive assets) as well as intangible extensions in the form of extensive coaching and training (typically with a primary emphasis on microenterprise development and the creation of savings devices) at no cost. Secondary training components vary widely, and programs of different intensities and with different targeting criteria have been filed under the graduation label (UNHCR, 2014).

There is momentum to advance the approach to greater scale, with multiple governments having initiated policy changes to implement it and millions of households reached to date (Partnership for Economic Inclusion, 2018). One plausible avenue involves amendments to existing social protection programs, and unconditional cash transfer programs in particular. Of 124 developing countries currently tracked by the World Bank, 89 implement unconditional cash transfers at more than negligible scale. Average spending is estimated at 0.53% of GDP (World Bank, 2019), amounting to over one third of total social protection spending (World Bank, 2018). Rates of spending in region with the lowest GDP – Sub-Saharan Africa – are estimated to be slightly lower, at 0.46% of GDP; nonetheless, this alone amounts to over US$ 25M daily. By adding intangible (training and mentorship) extensions to the existing targeting and transfer infrastructure, the composition of many cash transfer programs could be made to more closely resemble that of the graduation approach.

This emerging policy shift is grounded in empirical insights. The graduation approach gained major attention in the development research community following an experimental impact evaluation of BRAC's CFPR/TUP program by Bandiera et al. (2013), which demonstrated remarkable short- and medium-term poverty alleviation effects. Replication research in other organizational and geographic contexts yielded substantial heterogeneity across sites (and outright failure in some), but encouraging results on average (Banerjee, Duflo, Goldberg, et al., 2015; Bauchet, Morduch, & Ravi, 2015). The median social internal rate of return has been estimated at 8.5% annualized in real terms (Banerjee, Duflo, Goldberg, et al., 2015). While this number hinges critically on assumptions about long-term effects, available data from two sites collected 7-8 years after implementation indicate that impacts in fact persisted (Bandiera et al., 2017; Banerjee, Duflo, Chattopadhyay, & Shapiro, 2016). Graduation programs are unusual in terms of the sustainability of demonstrated poverty reduction, the high scientific standard with which the evidence was generated, and the breadth of this evidence.

That said, it should not be surprising that significant impacts on economic outcomes were detected, given a unit cost that often lies in the thousands of dollars per household (Banerjee, Duflo, Goldberg, et al., 2015; Partnership for Economic Inclusion, 2018). It is unclear what the required social rate of return to social protection and poverty alleviation activities – i.e., the opportunity cost of capital – might be. The estimated return of 8.5% lies above the discount rate used in the analysis of debt sustainability in low-income countries (International Monetary Fund, 2013) but below the estimated economic rates of return on World Bank projects (Herrera, 2005; The World Bank, 2010).

Capital costs might alternatively be defined as the expected returns from the single most realistic alternative use of funds. It has been argued that unconditional cash transfers are a sensible alternative to most poverty alleviation interventions (Blattman & Niehaus, 2014), and there is substantial momentum to use the impacts of cash as a benchmark (Center for Global Development, 2018). While the appropriate scope for cash benchmarking is debatable, subjecting the graduation approach to it appears sensible. Graduation already includes a transfer element, and from a short-run budgeting perspective any increases in the funding of training and mentorship extensions would proportionally reduce the volume of resources available for transfers. Substantial research has been conducted on unconditional cash transfers and impacts have been

detected on many dimensions of human development (Bastagli et al., 2016). Long-term data on economic impacts are thinner and rather mixed, and do not indicate that impacts universally persist (Blattman, Fiala, & Martinez, 2018; De Mel, McKenzie, & Woodruff, 2012; Handa, Natali, Seidenfeld, Tembo, & Davis, 2018; Haushofer & Shapiro, 2016, 2018; Wydick, 2018).

### *THE CHALLENGE OF PROJECTING AND MAINTAINING IMPACT AT GREATER SCALE*

As discussed, the addition of coaching and training extensions could make cash transfer programs more closely resemble graduation programs. But similarity in form must not mean similarity in function: the impact of an intervention depend not only on its structure but also on the context in which it is implemented. Context-specific moderators can differ across settings, so the same intervention can function differently in a new setting (Cartwright & Hardie, 2012; Deaton, 2010). The more complex an intervention and the more nuanced its rationale, the more difficult it is to understand under what conditions it works. The multifaceted structure of the graduation approach makes its critical success factors particularly difficult to define.

Of additional concern, some contextual moderators are likely a function of scale. Programs that worked at small scale may fail at large scale because the fidelity of implementation can suffer (Pritchett & Sandefur, 2014). All available randomized evaluations of graduation programs were implemented by nonprofit service providers, and all but one (i.e., Bandiera et al., 2017) in pilot settings; there are grounds for concern that diligence and quality may fall as scale expands. This concern is especially pressing in the context of interventions that aim to cultivate human capital (Bold, Kimenyi, Mwabu, Ng'ang'a, & Sandefur 2013).

### *OBJECTIVE AND APPROACH*

A central objective of this work is to anticipate and mitigate the effect of fading implementation quality, and therefore fading impact, as insights from the graduation agenda are promoted to greater scale. We pursue two types of ideas: one relates to the structure of the program, the other to the institutional rules that govern its implementation.

#### *Innovation in Program Structure*

By gaining a better theoretical grasp of the fundamental justifications for the graduation approach, we can arrive at hypotheses about how it functions. This may allow us to better predict under what conditions graduation will succeed (Bates & Glennerster, 2017; Williams, 2017). It could also highlight potential opportunities to simplify the approach, which would help address the challenge of implementing at scale: all else equal, a simpler approach – one with fewer interactions between implementer and beneficiary, and more templatized interactions where they do materialize – will tend to make the approach more scalable.[6]

The fundamental objective of the training and coaching components is clearly to build some intangible human capital asset that delivers sustained economic benefits to participants. These components usually account for a substantial portion of graduation program costs (Banerjee, Duflo, Goldberg, et al., 2015). Why the paternalistic decision to invest in them presumptively? Why not expand the transfer portion of the resource envelope and let participants allocate resources as they see fit, procuring goods and services that allow them to build whatever form of human or other capital they feel they require?

One possible explanation is that some goods and services are simply not available to the poor. For instance, one study found that aid program recipients routinely reported a valuation for vocational training services that far exceeded the cost of provision (Shapiro, 2017). Why might a functional market for coaching and training fail to independently materialize? Among several other reasons, one may be that human capital markets are not seamlessly linked to credit markets, as collateralizing (or otherwise transferring ownership over) human capital would be in conflict with legal and social norms related to the freedom of labor (Friedman, 1962). This suggests that people may find it more difficult to independently obtain financing for human capital investments (such as training or coaching) than for investments in tangible assets (Ljungqvist, 1993). It could explain why graduation providers promote the creation of savings groups – a financial device that we will investigate more closely. But it does not explain why training and mentorship must be added to a transfer, especially if the transfer comes in the form of cash; after all, this temporarily relieves credit constraints.

[6] That said, simplicity does not guarantee scalability, as evidenced by recent difficulties in promoting a light-touch migration subsidy intervention to greater scale; see Gharad, Chowdhury, & Mobarak (2014) and Levy & Raman (2018).

The design of the graduation approach therefore seems to presume that the poor would under-invest in human capital even if they had the necessary funds. It is widely accepted that human capital leads to the creation of sharable knowledge, a nonrival good with positive spillover effects (Romer, 1990); and such spillovers imply that when left to themselves, markets tend to under-invest in human capital relative to the social optimum (Stiglitz, 1989). But unlike other widespread transfer-based interventions such as community-driven development programs (which use cash transfers explicitly towards the creation of public goods) or conditional cash transfer programs (which often take an intergenerational perspective by promoting investments in children), the intended beneficiaries of graduation programs are equivalent to the participant households themselves. The design of graduation therefore seems to presume not externalities but *internalities*: that if participants were left to their own devices, they would take capital allocation decisions that are misaligned with their own best interests. This could be grounded in diverse heuristics and biases whose relevance has been repeatedly demonstrated in empirical field research (DellaVigna, 2009). In a clear illustration of their role in the design of cash transfer programs, Benhassine, Devoto, Duflo, Dupas, & Pouliquen (2015) introduced randomized variation in their delivery by labeling some as education transfers; though no conditionality was implied, recipients who received transfers under the education label ended up spending more on education on average. Such demonstrations of framing and mental accounting effects clearly challenge the notion that the investment decisions of cash transfer recipients necessarily reveal their deeply held preferences. But the insight that choices are malleable does not illuminate when they are suboptimal, nor does it guide how they might be improved.

To gain confidence in an agenda that makes human capital investment decisions on people's behalf, let us explore one way in which bounded rationality could lead people to systematically *under-invest* in human capital when left to their own devices. Human capital investments can only be made on the basis of expected returns (Manski, 1993). Individuals' appraisals can be systematically incorrect (differing from actual returns by more than a random stochastic element), so third parties with access to objective data may be able to appraise returns to investment in human capital more accurately than the individuals in question can (Jensen, 2010). One possible explanation is that the optimization problem of allocating resources to one's own human capital is a particularly insidious one, as it involves not only risk, but Knightian

uncertainty (Knight, 1921). Meta-knowledge (i.e., knowledge about the returns to knowledge) is difficult to acquire if the nature of the yet-unacquired primary knowledge is itself unclear: as the adage goes, "you don't know what you don't know". North (2010) argues that people routinely face such uncertainty and that a coping strategy is to rely on a stock of mental assets: beliefs, ideologies, half-baked theories, and similar schematic cognitions. These allow them to take decisions on complex matters for which they lack an adequate basis upon which to form probability-weighted expectations. Selten (1998) argues that aspirations serve a similar purpose: by setting goals and sticking to them, people can avoid the cognitively demanding process of constantly re-optimizing. So cognitions such as beliefs and aspirations are not grounded exclusively in contemporary data and profound analysis. Meanwhile, they are heavily influenced by social processes and cultural traditions (Hoff & Stiglitz, 2015). Even if individuals may be able to hone their cognitions through deliberation, gaps will remain (Gabaix, 2014; Hanna, Mullainathan, & Schwartzstein, 2014). Consequently, some opportunities that are objectively available may simply not feature in people's minds (Bernard, Dercon, Orkin, & Taffesse, 2014). The poor may be especially neglectful of opportunities that require a heightened sense of confidence if their cognitions have be shaped in part by stigma and social exclusion (Ghosal, Jana, Mani, Mitra, & Roy, 2016; Hoff & Pandey, 2014; Hoff & Walsh, 2018). Summing up, people may fail to pursue certain opportunities because the *homo oeconomicus* in them lacks a basis for appraising their returns and the *homo sociologicus* provides no impetus to even consider them. Meanwhile the implementer of a graduation program – having defined a specific curriculum and gained experience using it in training – may have an empirical basis upon which to form return expectations. This helps justify the implementer's decision to make presumptive investments in human capital on people's behalf.

It also points to a possible avenue for simplifying the approach. In the above framework, one constraint that impedes the poor from independently making profitable investments in their own capabilities is their psychological outlook. (This can be thought of as a capability in itself: Appadurai, 2004, writes of the "capacity to aspire".) The graduation approach may function in part by addressing this constraint: markers of psychological empowerment and well-being have been shown to improve markedly among participants (Banerjee, Duflo, Goldberg, et al., 2015; Duflo, 2012). But it is possible to address psychological

constraints more directly than through participation in extended training and coaching activities. For instance, there is substantial academic evidence that quick and inexpensive interventions can help people define aspirations and make plans to pursue them (Duckworth, Grant, Loew, Oettingen, & Gollwitzer, 2011; Duckworth, Kirby, Gollwitzer, & Oettingen, 2013; Gollwitzer & Sheeran, 2006). Related cognitions can be influenced through similarly light-touch experiences: for instance, research has repeatedly demonstrated the developmental impacts of media and communication interventions involving persuasion (C. Dweck, 1999; Paunesku et al., 2015; Walton, 2014) and role models (Beaman, Duflo, Pande, & Topalova, 2012; Bernard et al., 2014; Kearney & Levine, 2015; Riley, 2018). We refer to "related cognitions" because our interest is not limited to one isolated belief or goal. Diverse forces – ranging from an intrinsic aversion to cognitive dissonance (Festinger, 1957; Mahaffy, 1996) to social processes such as information cascades (Baddeley, 2010; Bikhchandani, Hirshleifer, & Welch, 1992; Hirshleifer & Welch, 1992) – tend to align cognitions with some coherence. Beliefs therefore cluster into belief sets: people who believe that human intelligence is malleable tend to also subscribe to the notion that one's conditions can be improved through will and effort; meanwhile, people who report low self-efficacy tend to also have deterministic convictions (Baumeister & Brewer, 2012; Plaks, Levy, & Dweck, 2009). One possible object of intervention, then, is a broader constellation of cognitions that reflects positively upon one's sense capabilities and future. Empirically, this may be indicated by a set of correlated measures (Orkin et al., forthcoming), including the belief in control over one's future ("locus of control"; Rotter, 1966); the belief one's ability to achieve goals ("self-efficacy"; Bandura, 1982); the belief that capacity is malleable through effort ("growth mindset"; Dweck, 1986), and diverse measures of aspirations and expectations.

A potentially effective and yet scalable variant of the graduation approach could therefore combine plain transfers with a psychological intervention that aims to foster a constellation of empowering cognitions. The transfers would overcome what in the words of Lybbert & Wydick (2018) is an "external" constraint to prosperity (i.e., a lack of objective opportunities), while the psychological intervention would aim to overcome an "internal" one (as manifested in a lack of perceived opportunities). This could conceivably salvage the critical active ingredients of graduation while avoiding the need to invest in extensive training and coaching on participants' behalf. While we hypothesize that the psychological intervention removes

the barriers that had prevented individuals from investing in training and coaching independently, the set of available investments is much broader and the psychological intervention may provide a mental channel to any number of potential pursuits that call for a heightened sense of confidence. The agency to select among these pursuits would be left to beneficiaries.

***Innovation in the Governance of Implementation***

Above we hypothesized that some program ingredients may be more central than others and explored how to potentially simplify and templatize the approach. But it is possible that such simplification attempts miss the mark because the critical ingredients of graduation are too subtle to be fully understood, codified and disseminated; that they are tacit as opposed to explicit (Polanyi, 1966). For instance, it may turn out that the human touch of personalized coaching is irreplaceable, but that it requires a dash of secret sauce and a pinch of magic dust to succeed. If we end up unable to define the key success factors, then we cannot ascertain if they are present in new settings, and we can neither use evidence to make informed projections of whether graduation will function in a new setting, nor whether its impacts will be robust to a scale-up.

We could still consider promoting evidence-based scale-up efforts that allocate resources retrospectively as a function of demonstrated impact: in other words, that pay for results. Existing academic literature on Payment-by-Results (PbR) is predominantly skeptical (Clist, 2016; Paul, 2015), emphasizing pitfalls such as gaming (Boyle, 2011) and motivation crowding (Frey, 2017). It is widely assumed that the central objective of PbR is to align implementers' incentives with those of donors so as to overcome principal-agent issues such as moral hazard. It is less widely appreciated that PbR could also *select for* implementers and approaches that happen to be impactful (Barder, 2012). Put differently, it has the potential to introduce evolutionary forces that push development practice to learn by reinforcement. Applied to our policy challenge: if PbR differentially expands resource allocation in those settings where evidence of impact happens to materialize, then this accelerates scale-up where emerging evidence suggests that the necessary conditions for cost-effective implementation are present. This mechanism may work even where other forms of learning fail, e.g. because the critical success factors cannot be fully understood and codified into generalizable evidence. While this justification is rarely invoked to motivate PbR, the gale of evolutionary

forces is central to the explanation of economic progress in the private sector (Beinhocker, 2007; Schumpeter, 1942) and machine learning and artificial intelligence routinely deploy reward functions to overcome epistemic challenges (Barto & Sutton, 1998). Though the rationale for PbR under an evolutionary framework is quite different from the principal-agent framework, the practical prescription is similar: payments should be tied to impacts achieved, "as if" to incentivize implementers to perform.

### *GAPS AND CONTRIBUTIONS*

Learning how to achieve sustained poverty relief through the design of existing social protection systems is a major challenge that calls for many complementary and sustained research efforts across different contexts. We will aim to contribute insights that speak to three questions:

**Question 1. Can the graduation approach be simplified? More specifically, can the intangible component be cut, distilled, or eliminated without adverse consequences?**

Some recent multi-arm studies suggest that combining transfers with human capital extensions can expand poverty reduction (Banerjee, Karlan, Osei, Trachtman, & Udry, 2018; Berge, Bjorvatn, & Tungodden, 2015; Chowdhury, Collins, Ligon, & Sulaiman, 2017). But this is not universally so (Fiala, 2018; Karlan, Knight, & Udry, 2015). It will be useful to better establish the impacts of the full approach versus plain cash transfers within given study contexts and ask if adding intangible extensions to transfers can reduce poverty not just detectably, but cost-effectively - so by more than the cost-equivalent top-up of transfers would.

It would also be sensible to evaluate alternative intervention packages that are grounded in alternative theories of change. This could involve subtracting individual intangibles, e.g., evaluating the approach without training on savings group formation, as we will aim to do. It could also involve an attempt to distill the presumed active ingredients. For instance, if the central function of the intangible component was not in fact to build specific skills or knowledge but to alter more generalized mental constructs, this insight would be highly actionable: psychological science points to very light-touch interventions that manipulate such constructs directly (Duckworth et al., 2013; Walton, 2014). Behavioral features could substitute extensive training and coaching in principle, or at least enhance the impact of cash transfer programs

(Wydick, 2018). We will therefore aim to expand evidence on psychological engagement with cash transfer recipients.[7]

**Question 2. How might transfers and the intangible components interact?**

Implicit in the integrated design of the graduation approach is the assumption that combining the transfers with intangible assets (cultivated in the form of training and mentorship) is at least as cost-effective as delivering either constituent component in isolation. Why might they be complementary? One obvious answer is that the intangible assets created through training and coaching could help participants to better maintain, or derive more value from, the tangible wealth created by the transfer. A less obvious one is that the poor – who need to juggle erratic income and pressing expenses (Mani, Mullainathan, Shafir, & Zhao, 2013) – may experience temporary relief from the cognitive burdens of poverty when they receive a transfer; in other words, the cash inflow may "cognitively prepare" them for the cultivation of the intangible. We will investigate this later conjecture, as it has a very direct policy repercussion: it suggests that when a cash transfer is combined with the cognitively demanding cultivation of an intangible, the transfer should come first.

**Question 3. When PbR is used to scale up integrated poverty alleviation programs, what features should the grant agreements have?**

In one possible future scenario, the intangible component is established to be critical and cannot be eliminated, simplified, or cut without major adverse effects on cost-effectiveness; but meanwhile, research on the scale-up of the integrated approach fail to achieve sustained impact and no consensus about the reasons and remedies emerges. PbR might play a promising role in this scenario by accelerating scale-up in those conditions where the critical success factors happen to be in place, even if the nature of these

[7] Relevant literature includes Blattman, Jamison, & Sheridan (2017), who demonstrate that augmenting cash transfers with cognitive behavioral therapy reduced crime and violence among male adolescents in Liberia; and Benhassine et al. (2015), who explore the impacts of changing the framing of cash transfers. Much further work is ongoing: Orkin et al. (forthcoming) are investigating the interaction of aspirational interventions and cash transfers, and Datta, Martin, & Rawlings (forthcoming) are experimenting with psychological extensions to improve child development outcomes.

factors remains poorly understood. We will describe the initiation, design, and implementation of an attempt to fund the scale-up of a nonprofit implementer's graduation-type poverty reduction program based on demonstrated outcomes. Using available data from other parts of the dissertation, we will analyze to what degree this PbR arrangement achieves the objective of linking the implementer's poverty alleviation performance to its financial performance; this linkage appears critical for the ability of PbR to "select for success". We will also discuss how the project navigates diverse well-known pitfalls of PbR (as highlighted by Boyle, 2011, Clist, 2016, and Frey, 2017).

## *CHAPTER 1. CASH-PLUS: POVERTY IMPACTS OF ALTERNATIVE TRANSFER-BASED APPROACHES*


### *ABSTRACT*

Can training and mentorship extensions enhance the economic impact of cash transfers, or do they needlessly absorb resources that recipients could allocate more impactfully by themselves? A poverty alleviation program in rural Uganda targets poor households and offers them an integrated package comprised of lump sum cash transfers as well as training and coaching on microenterprise development and savings group formation. Using a randomized trial, we evaluate the impacts of this program and of simplified variants. One program variant subtracts the savings component; another subtracts all training and coaching, providing only lump sum cash transfers. A third variant adds only a light-touch behavioral extension (involving goal-setting and plan-making) to the cash transfers. We find that simplifying the integrated program tends to erode its impact and cost-effectiveness.


### *DISCLOSURES*

The study has been registered[8] and de-identified data, cleaning code, analysis code, scripts, surveys, and other supplementary materials have been archived.[9]

### *MOTIVATION*

So-called graduation programs provide an integrated intervention package at no cost to exceptionally poor households in low-income countries. The package is comprised of both a transfer (in the form of productive and/or monetary assets) and a series of training and coaching activities (that aim to cultivate some intangible asset). Randomized evaluations have found cost-effectiveness on average, though there is considerable heterogeneity across contexts (Banerjee, Duflo, et al., 2015). Long-term (7-8 year) effects appear highly encouraging (Bandiera et al., 2017; Banerjee, Duflo, Chattopadhyay, & Shapiro, 2016).

The intangible (training and mentorship) component usually accounts for a substantial portion of program costs. Skeptics may argue that development practice has a long history of paternalistically misallocating

[8] Registry for International Impact Evaluations ID 52bb3799ccf6a
[9] osf.io/mzrkx

resources (Bauer, 1972; Easterly, 2007; Moyo, 2009; Shapiro, 2017). Why invest in training and mentorship on participants' behalf? Why not give beneficiaries expanded agency over program resources by expanding the monetary transfer portion of the program (effectively providing unconditional cash transfers instead of the integrated graduation package) and allow beneficiaries to invest for themselves?

One justification could be that human capital markets are inefficient or inaccessible (Stiglitz, 1989). For example, they may be disjoined from credit markets because human capital cannot easily be collateralized (Ljungqvist, 1993). This could justify why the graduation approach invests in the creation of savings groups – an alternative financial instrument. But it goes much further, providing extensive training and coaching on microenterprise development and often beyond. The design appears to presume that if participants were left to their own devices – say, if they were provided only with unconditional cash transfers – they would choose to under-invest in their human capital against their own best interests. Of course, the choices of cash transfer recipients may simply reveal their preferences (Das, Do, & Ozler, 2005). That said, it has been demonstrated that their investment decisions can be highly sensitive to seemingly trivial design factors, such as the labeling of the transfer (Benhassine et al., 2015). It is therefore sensible to account for behavioral factors that might cause cash transfer recipients to forgo profitable investment opportunities. Many such factors have been established, ranging from time inconsistency (O'Donoghue & Rabin, 1999) to social preferences (DellaVigna, 2009). People's beliefs may also be flawed: for boundedly rational agents who lack perfect information and perfect attention (Simon, 1971), it can be a tall challenge to accurately perceive the returns to available investment options. Program implementers with access to objective data may be in a position to appraise returns to human capital more accurately than the individuals in question can (Jensen, 2010).

But expanding transfers with extensive training and mentorship warrants more than a theoretical justification; it calls for empirical scrutiny. If integrated microenterprise development is in fact a more cost-effective poverty alleviation approach than unconditional cash transfers, this could have significant policy repercussions. In Sub-Saharan Africa alone, over US$ 25M are disbursed in the form of unconditional cash transfers on a daily basis (World Bank, 2019). The composition of transfer programs could be altered by adding intangible (training and mentorship) extensions to the existing targeting and disbursement

infrastructure.[10] Evidence on the relative impacts of integrated microenterprise programs and pure cash therefore appears highly policy relevant. A broad literature on the impacts of unconditional cash transfers exists (Bastagli et al., 2016), but longer-term economic impacts are less well established (Wydick, 2018). While highly encouraging impacts have been found in contexts involving middle-income entrepreneurs (De Mel et al., 2012; McKenzie, 2017), the long-term impacts on the very poor appear more muted (Blattman et al., 2018; Haushofer & Shapiro, 2018). Some recent research suggests that human capital extensions can help the poor derive more sustained value from cash or asset transfers (Banerjee et al., 2018; Berge et al., 2015; Chowdhury et al., 2017) though not all research does (Fiala, 2018; Karlan et al., 2015).

The high degree of heterogeneity in available evidence suggests that impacts are moderated by important unknown factors (Cartwright & Hardie, 2012; Deaton, 2010). If some of these include tacit ones that can neither be well defined nor monitored (say, some aspect of "implementation quality"), and these factors are negatively associated with scale, then pilot studies may have yielded exceedingly optimistic policy predictions (Bold et al., 2013; Pritchett & Sandefur, 2014).

### *RESEARCH QUESTIONS*

One of our objectives is to generate evidence on the relative impacts of a (graduation-style) integrated microenterprise development program versus pure unconditional cash transfers. We compare these approaches within the same setting and at approximately the same cost per household. Based on the available body of evidence, we expected that the integrated program would direct households towards microenterprise administration and lead to sustained improvements in economic as well as subjective well-being. Meanwhile, based on work by Fafchamps, McKenzie, Quinn, & Woodruff (2012), we expected that unconditional cash transfers would result in relatively lower initial investment in productive assets, leading to higher short-term but lower medium-term consumption impacts compared to the integrated microenterprise development approach.

[10] For the programs to fully resemble the so-called graduation approach, many would further require a change in the payout pattern of transfers – specifically, the inclusion of larger and less frequent lump sum payments.

Another objective is to evaluate the simplification of the integrated graduation approach. All else equal, an alternative with fewer and more templatized interactions between program implementers and beneficiaries would be more scalable. We can simplify the integrated approach by subtracting individual components. The theoretical justification for including savings devices in the integrated approach is somewhat ambiguous, and available evidence on savings groups as a stand-alone intervention is mixed (Gash & Odell, 2013; Karlan, Savonitto, Thuysbaert, & Udry, 2017). The savings group component is therefore one potential candidate for elimination.

We can also simplify the integrated approach by distilling its hypothesized active ingredients and delivering them in the form of a light-touch intervention on top of pure cash transfers. Improvements in markers of subjective well-being have been established across evaluations of the graduation approach (Banerjee, Duflo, et al., 2015), and it has been argued that these improvements may have mediated the observed poverty reduction by promoting more optimistic beliefs (Duflo, 2012). The graduation approach is easily reconciled with psychological insights, e.g., on goal-setting and plan-making (Gollwitzer & Sheeran, 2006; Oettingen & Gollwitzer, 2010). This raises the question if a light-touch intervention that directly targets psychological mechanisms could yield similar benefits to the integrated program at a fraction of the cost, or at least expand the benefits of pure cash transfers.

### *PROGRAMMATIC CONTEXT*

Village Enterprise is a nonprofit organization that implements microenterprise programs in Uganda and Western Kenya. Like most other graduation programs, it uses a participatory targeting process as well as a proxy means test to identify the poorest households and then provides one of the household members with a combination of transfers, mentorship, and training. The program is relatively short for a graduation program: training sessions take four months, mentorship engagement takes nine months, and the overall program concludes within a year. A substantial part of the training is focused on microenterprise administration (e.g., business selection, business planning, record-keeping, and livestock management). The program encourages participants to establish business activities as partnerships with other households (target size: three households). The program also establishes village-level savings groups with about thirty

members that provide basic savings and loan functions and train participants on the formation, functioning, and governance of these groups. There is little training beyond microenterprise and savings group formation; the program does not include modules included in diverse other integrated development programs, such as nutrition, hygiene, family planning, child rearing, or literacy. However, the program does include a training session on environmental conservation that is not otherwise widespread. Coaching is run by designated business mentors and focuses specifically on matters of micro-enterprise management. The transfer component of the program is delivered not in the form of physical assets, but cash. Transfers are made to the business partnership, not to individuals or households, on the presumption that this will encourage productive investment. Indeed, the second of the two transfer instalments is made conditional on having invested the first instalment in the group business. Unlike in some comparable programs, no consumption stipend is provided. Being less comprehensive and shorter in duration, the Village Enterprise program comes at roughly a third of the cost (in USD PPP terms) of the least costly graduation program included in the meta-study of Banerjee et al. (2015).[11]

## *STUDY DESIGN*

### *Sampling and Eligibility*

Two regions were selected for the study – one in Western Uganda (Hoima district) and another in Eastern Uganda (Amuria, Katakwi, and Ngora districts). In each region, 69 villages were identified that qualified as large enough for the study, meaning that an initial mapping exercise indicated that at least 70 participant households would qualify for the Village Enterprise program.

In these villages, Village Enterprise implemented a participatory wealth ranking similar to the participatory rural appraisals described by Banerjee, Duflo, Chattopadhyay, & Shapiro (2009), involving the active

[11] If one was to interpret the programmatic imperative underlying graduation to involve "big push" investments, the low relative cost of the Village Enterprise program limits the generalizability of results to other graduation programs. Meanwhile, if one interprets the programmatic imperative to involve "multi pronged" investments, comprised of both tangible and intangible components; and if one does not assume economies of scale; then it is warranted to generalize rates of cost-effectiveness from the Village Enterprise program to costlier graduation programs.

participation of village residents in the identification of ultra-poor households. Households ranked in the poorer two of four possible wealth categories were slated for further screening.

A basic household survey was then administered to these households with 10 survey questions (see Schreiner, 2011) that were used to calculate a Poverty Probability Index[12] (PPI). Households with poverty scores at or below 39 out of 100 were considered eligible, except if any of the following exclusion criteria applied: a) there is a teacher, salaried worker or pension recipient in the household; b) the household owns more than two cows; or c) the dwelling has a cement floor, brick wall, and metal roof. Households with poverty scores above 39 were considered ineligible, except if two or more of the following inclusion criteria applied: a) the household head is unemployed and not pensionable; b) the household has 8 or more children aged under 18; c) the household head is disabled, widowed, or an orphan under the age of 18; d) the household has suffered directly from a natural catastrophe; or e) the household head is living with a chronic illness.

Table 1 provides a sense of the economic status of eligible households at baseline. Per capita consumption averages UGX 617k annually, amounting to 1.59 USD PPP (2016) per day (for applicable rates, see "Appendix I: Exchange Rates"). It appears that the implementer successfully targets people whose consumption lies below the international poverty line of USD PPP 1.90 per capita per day. Our measures indicate that most consumption is not derived from income earned in the form of cash inflows from productive activities, which suggests that households derive a large share of consumption from subsistence or assistance. That said, income measures are notoriously difficult to measure in low-income contexts and especially prone to under-reporting (see Deaton, 1997, and Meyer & Sullivan, 2003).

[12] At the time of its administration, the PPI was defined as the Progress-out-of-Poverty Index.

**Table 1: Economic Status of Eligible Households at Baseline (UGX per Capita)**

| | Western Region | | Eastern Region | |
|---|---|---|---|---|
| | Mean | Std Dev | Mean | Std Dev |
| **Total Consumption (Annual)** | **747,387** | **399,386** | **505,844** | **311,176** |
| Food & Beverage Consumption | 586,012 | 316,141 | 378,748 | 252,055 |
| Recurring Consumption | 87,630 | 83,409 | 59,573 | 59,316 |
| Infrequent Consumption | 61,815 | 71,185 | 60,435 | 66,710 |
| **Total Net Assets** | **102,197** | **114,654** | **95,196** | **111,343** |
| Livestock Assets | 32,220 | 58,211 | 60,752 | 82,027 |
| Durable Assets | 62,752 | 64,674 | 30,869 | 39,711 |
| Net Financial Position | 1,830 | 8,187 | 833 | 6,557 |
| **Total Productive Cash Inflows (Annual)** | **268,442** | **326,199** | **104,266** | **164,515** |
| Net Cash Inflows from Farming | 11,128 | 67,611 | -9,024 | 49,288 |
| Income from Other Self-Employment | 94,324 | 169,463 | 39,482 | 91,791 |
| Income from Paid Employment | 124,588 | 176,312 | 66,532 | 89,469 |

Notes: As data are derived from baseline survey, they are contingent on study recruitment and survey consent. All items are calculated in accordance with the preferred operationalization presented below, meaning that the top and bottom 2.5% of observations are coded to the respective cutoff levels (95% winsorization). As each measure is winsorized separately, the sum of the sub-aggregates may not equal the aggregates.

During the PPI survey process, Village Enterprise also identified a representative for each household who might be invited to receive a development intervention. The resulting list was shared with the research team for randomization.

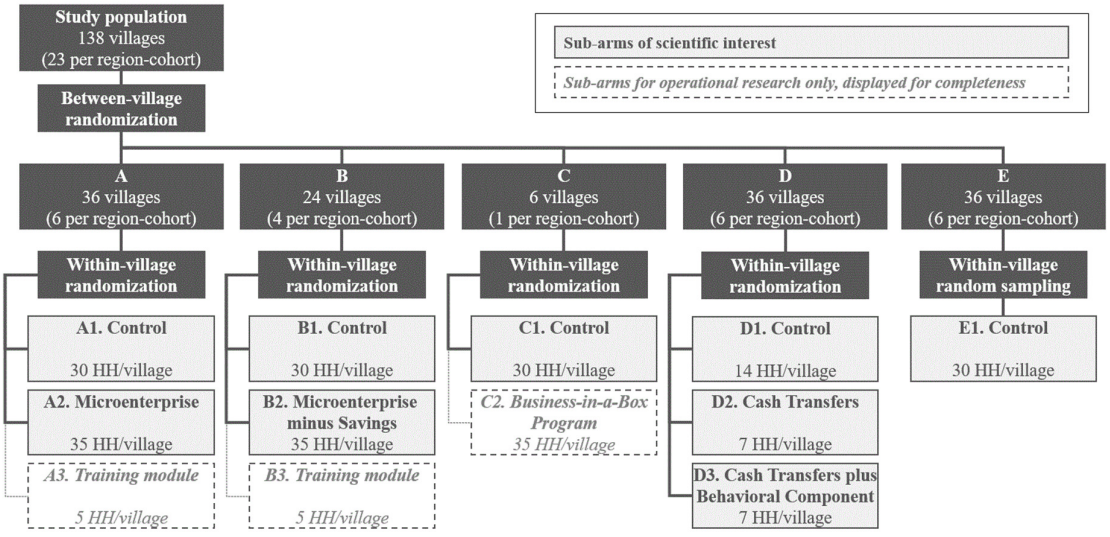
***Randomized Assignment***

The chosen randomization protocol was an effort to optimize the parallel objectives of different parties - to implement impactfully, improve future operations, and expand the body of knowledge. The allocation of the study population to different arms and sub-arms was therefore determined not only by the scientific considerations of the research team but also by accommodating the needs and constraints of the implementer. The implementer was largely unconstrained in the availability of resources for the implementation of its standard program. The further removed a program variant, the harder it was for the implementer to justify the use of charitable resources. As a consequence, the program variant without the savings group component was assigned a smaller sample than the standard program. Pure cash transfers (which required dedicated evaluation resources) were implemented on an even smaller scale. Meanwhile, we included some variants that were closely associated with standard operations, and could be funded by

the implementer and assessed for operational research purposes, but were implemented at too small a scale to warrant scientific attention. The analyses presented in this paper are those from which some positive scientific contribution was expected – but as the sample sizes allocated to different tests vary, so does the probability of false negatives. This has some repercussions for the application of statistical standards in the interpretation of results. A reader who seeks to assess the impacts of a program variant relative to a pure control group should carefully consider the precision of the estimate. Meanwhile, a reader who directly compares two program variants in the spirit of an A/B test may be less concerned with the thresholds of detectability: if the interventions are cost-equivalent and impacts are a priori expected to be equal, even imprecisely estimated effects can have some epistemic value.

Within each region, three equally sized region-cohorts of 23 villages each were formed, resulting in six region-cohorts. As displayed in Figure 1, the randomization was stratified by region-cohort and assigned villages at random to one of five arms, labeled A-E. Eligible participants within each village were further randomly allocated to sub-arms.

**Figure 1: Assignment of Villages to Study Arms (and Households to Sub-Arms)**

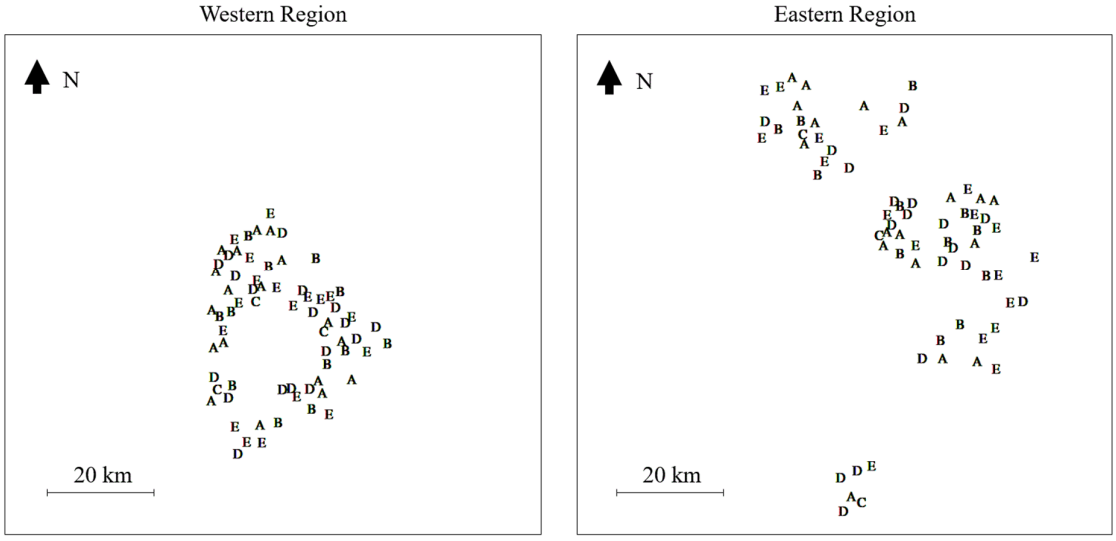



Note: HH/village denotes the number of households per village.

In 36 A-type villages, 30 households were assigned to controls (sub-arm A1) and 35 to the microenterprise program (A2). A further 5 households were assigned to a training module designated ex ante to be used for

operational research purposes only. In 24 B-type villages, 30 households were assigned to controls (B1) and 35 to a variant of the microenterprise program excluding the savings group components (B2). Here too, a further 5 households were assigned to operational research. In 6 C-type villages, 30 households were assigned to controls (C1) and 35 to a variant of the microenterprise program called business-in-a-box that Village Enterprise opted to evaluate for operational research purposes (C2). In 36 D-type villages, 14 households were assigned to within-village controls (D1); 7 were to plain cash transfers (D2); and 7 were to behaviorally designed cash transfers (D3). In 36 E-type villages, 30 households were assigned to controls (E1). Figure 2 displays the geographic distribution of villages by arm and region.

**Figure 2: Map of Villages by Region and Arm**




Note: Each letter corresponds to a village; the labels (A-E) define the study arm that was randomly assigned to each village. The position of villages describes their spatial location, so the charts can be interpreted as maps. As only the relative location is of interest here, other geographic features are omitted.

Following the randomization, a baseline survey team was provided with a list of intended study invitees. Neither enumerators nor invited respondents were acquainted with the intended treatment assignment, so the decisions to accept the invitation and participate in the research study were independent of the randomization. Participants who opted to participate in the baseline survey were formally recruited into the study.

***Intervention Design***

The standard microenterprise program (sub-arm A2) was the routine program of Village Enterprise, comprising training, transfers, and mentorship. The training component involved sixteen weekly sessions, each of which took one to three hours (excluding travel time). Of these, the first was an introduction to the program; another session involved the formation of microenterprises; six dealt with savings and the formation, functioning, and governance of savings groups; seven with microenterprise administration; and one with environmental conservation. The total duration of the training was approximately 4 months. Several training sessions into the program, a nominal lump sum cash transfer of 240k Ugandan Shillings (UGX) was made to each business (amounting to UGX 80k per household), contingent upon approval of a business plan. The second transfer (at half the initial amount) was made upon a progress report approximately seven months later, contingent on a review that investments of the initial seed capital had been invested in business activities and that the group was still operating. The average transfer date, weighted by the transfer amounts, was August 2014 (i.e., 15 months before the first and 27 months before the second follow-up survey). Mentorship visits started after the first transfer and continued at monthly intervals.

Sub-arm B2 was a variant of the microenterprise program that excluded the six training sessions on savings group formation, as well as associated coaching visits. Village-level groups with a representative were still formed for the purpose of establishing an administrative counterpart for the implementer.

Sub-arm C2 was a variant of the training program involving the delivery of a pre-selected (typically livestock) asset instead of cash transfers, along with some training on the management of the asset. As discussed above, this arm was excluded from the scientific evaluation at the outset and used only for operational purposes; we discuss it here because the control group in C-type villages (i.e., sub-arm C1) can prove useful in the analysis, and because the activities in these villages will need to be considered in the costing exercise. (Operational research was also conducted in sub-arms A3 and B3; the incremental cost of delivery was however negligible, so these sub-arms will not be revisited in the costing section.)

Sub-arm D2 involved only unconditional cash transfers. Unlike in the microenterprise program variants, payments were provided not to three-member businesses but to households directly. Eligible ones were

presented with a voucher and given a time and date when they could expect initial cash disbursements. Intervention leaders explained that a nonprofit had decided to disburse cash for people in the region that they could use as they pleased. The cash disbursement was made in a central village location, with an initial lump sum transfer of UGX 208k per household, followed by a second transfer at half the initial amount. The timing of the two payments mirrored that of the microenterprise program variant. The amounts were budgeted in the planning stage to equate 92.6% of the total expected cost of the microenterprise program. In other words, 7.4% were subtracted to account for targeting, delivery logistics, and overhead; this was the lowest rate of non-cash costs that was identified in a review of cash transfer delivery initiatives.[13]

Sub-arm D3 expanded upon the cash transfers described in sub-arm D2 using a light-touch behavioral intervention that attempted to distill, with the help of a psychologist, the existing training and mentorship activities into their presumed psychological elements. The intervention was comprised of three sessions, including (a) an introductory discussion alongside the provision of the cash vouchers (35 minutes); (b) a workshop surrounding the first cash disbursement (145 minutes); and a meeting surrounding the second disbursement (30 minutes). Goal setting and plan-making activities were designed on the basis of literature on mental contrasting and implementation intentions (Gollwitzer, 1999; Oettingen, 2000). Participants also completed self-affirmation exercises intended to address the stigma of poverty and to promote a sense of adequacy and pride (Cohen & Sherman, 2014; Hall, Zhao, & Shafir, 2014) Participants were asked to think about peers who had been successful and about ways they could follow their peers' examples. This was motivated by work on role models (Lockwood & Kunda, 1997), on social norms (Cialdini & Trost, 1998), and on social comparison processes (Festinger, 1954). Participants also completed drawings and created slogans to help remind them of their goals (Karlan, McConnell, Mullainathan, & Zinman, 2010; Rogers & Milkman, 2016). Finally, the program included a mental accounting exercise (Thaler, 1999). The first transfer was provided in two envelopes, with one (amounting to UGX 188k) labeled as intended to support

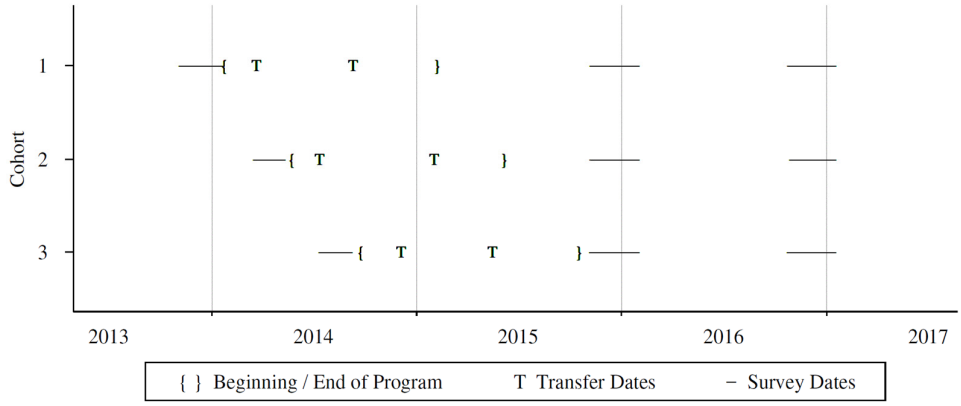
[13] The materials used for this analysis are outdated and no longer public on givedirectly.org. The following website, which made a copy of them, has been archived by the author:
http://web.archive.org/web/20190807141026/http://givedirectly12.blogspot.com/p/analista.html?m=0

the goal, and the other (UGX 20k) labeled as intended for personal incidentals. This was meant to encourage participants to draw a clear line between consumption and investment.

***Data Collection***

As displayed in Figure 3, the study builds on three household surveys: one baseline and two follow-up surveys (labeled midline and endline). The baseline survey (like the program implementation) were staggered by cohort, but the two follow-up surveys were conducted in the same time period for all study participants.

**Figure 3: Intervention and Survey Timelines by Study Cohort**




At the outset of the study, the outcome variables perceived as most central to the theory of change were key poverty indicators (i.e., per-capita consumption, income, and assets). The structure of financial positions (i.e., savings and debt), the employment status of household members, and the subjective well-being of the respondent were also of elevated interest. Further measures on nutrition, education, health, decision-making, cognitive performance, and community life were also of collected.

Over the course of the evaluation, some measurement decisions were updated. Diverse psychological and community related measures (e.g., self-control, pride, aspirations, expectations, trust, intimate partner violence) were added to the follow-up surveys. In these follow-up surveys, income and asset measures were collected in updated manner (specifically, collected separately for households and businesses, whereas previously they had been pooled). The collection of cognitive performance data was piloted but

abandoned, and no cognitive outcomes were analyzed. Survey forms, data sets, and code are publicly archived in de-identified form.

### *Empirical Strategy: Economic Outcomes*

The multifaceted research design and the richness of the data set provide opportunities for many different analyses. We first generate a universe of plausible analyses. Then, building on literature on model selection (Burnham & Anderson, 2004, 2010; Clyde, 2003; Hoeting, 2002; Hoeting, Madigan, Raftery, & Volinsky, 1999) and on so-called specification curves (Simonsohn, Simmons, & Nelson, 2015), we choose a preferred specification. The rest of this section describes the selection process; the results-oriented reader may want to jump straight to the "Results", where the preferred analytical specification is shown.

#### *Universe of Plausible Analyses*

To create a universe of plausible analyses, we first identify central dimensions along which different sensible analyses can differ. We distinguish three classes of "choice dimensions": tests, models, and operationalizations. Tests are defined as alternative combinations of follow-up outcomes $y_F$ and intent-to-treat assignments $T$. Each test has a different substantive interpretation. Alternative models and operationalizations do not have a different substantive interpretation, though their choice can influence the results of any given test.

The dimensions along which "tests" can differ include the following:

(1) **Definition of outcomes**. As our central poverty indicators, we focus on economic outcomes in the narrow sense. In defining outcome $y$, we follow the definitions in Table 1, separating three sub-composites for each of consumption, assets, and cash inflows. We also create an aggregate composite for each. (Total: 12 choices)

(2) **Definition of outcome rounds**. Each poverty outcome can be defined in terms of follow-up round $F$: the first follow-up (midline); the second follow-up (endline); and a pooled value averaging the two. (Total: 3 choices)

(3) **Definition of comparisons**. In defining $T$, we choose to pursue 6 types of impacts:

[a] those of spillovers by comparing the set of sub-arms A1, B1, C1, and D1 to E1 (which may help in the selection of appropriate counterfactuals where alternative choices are available);

[b] those of the microenterprise program variants by comparing the set of sub-arms A2 and B2 to untreated controls (though alternative choices of appropriate control counterfactuals are available, as described below);

[c] those of the cash transfer program variants by comparing the set of sub-arms D2 and D3 to untreated controls (though alternative choices of appropriate control counterfactuals are available, described below);

[d] those of the savings group component, conditional on the microenterprise program, by comparing sub-arm A2 to B2;

[e] those of the behavioral intervention component, conditional on the cash transfer program, by comparing sub-arm D3 to D2; and

[f] the incremental impacts of the microenterprise program over those of the cash transfer program by comparing the set of sub-arms A2 and B2 to the set of sub-arms D2 and D3.

We define "models" as alternative forms of specifying the econometric analysis for any given test. Choice dimensions include the following:

(1) **Use of baseline values**. We can control for the baseline value of the outcome $y_B$; subtract it from outcome data for differences in differences; and leave it out of the estimation process altogether. (Total: 3 choices)

(2) **Use of socioeconomic covariates.** Many different socioeconomic baseline covariates $X_B$, and many more combinations, are available. We can reduce this complexity by pursuing 2 choices: 'selecting none' or 'selecting some set'. Where 'selecting some set', we cede the choice to an algorithm to minimize researcher discretion; specifically, we use least angle regression (Efron, Hastie, Johnstone, & Tibshirani, 2004) to identify five among those candidates listed in Table 2.

(3) **Use of fixed effects**. The model can use intercept $\alpha$ or control for $\alpha_j$, where $j$ is the village; the latter implies the use of cluster fixed effects. (Total: 2 choices)

By "operationalization", we mean the coding of variables. We define the following choice dimensions:

(1) **Outlier adjustment**. As poverty measures are sensitive to outliers, some adjustment is required. To avoid introducing attenuation bias, it is most sensible to adjust each combination of y and $T$ separately. But the appropriate level is not certain ex ante. We recode the highest and lowest 0.5%, 2.5%, or 5% of observations to the cutoff value; i.e., winsorize at the 99%, 95%, or 90% level. (Total: 3 choices)

(2) **Definition of the counterfactual in controlled comparisons.** As defined above, comparison sets [b] and [c] compare a treatment group with a counterfactual group comprised of "pure controls" – i.e., one where nobody is treated. There are different ways to operationalize these controls: we code treatment assignment $T$ to the value zero [i] for controls within villages (within-village controls); [ii] for controls in pure control villages (between-village controls); or [iii] for all available controls ranging from A1 to E1. These choices come with different merits: electing between-village controls circumvents adjustments for cluster robustness, with benefits for statistical power, while selecting only control villages would minimize susceptibility to bias emerging from within-village spillovers. The third option is a compromise between power and unbiasedness. (Total: 3 choices)

(3) **Valuation approach**. Where the computation of $y$ involves calculating the value of goods, we use the price estimates reported by respondents; the median prices in a regional geographic unit; and a combination that uses the former where available and the latter where respondents are unsure. (Total: 3 choices)

*Distillation of Analyses*

Multiplying each of the 12×3×6 tests with 2×3×2 alternative models and 3×3×3 alternative operationalizations would yield a total of 69,984 impact coefficients. But not every specification is applicable for every test. First, a choice of three alternative counterfactuals is only available for comparison sets [b] and [c], but not for comparison sets [a], [d], [e], and [f]. Second, a choice of cluster fixed effects is only available for comparisons within arms, where the unit of randomization as well as the unit of observation is the household (we label "non-clustered comparisons"), because cluster fixed effects would be collinear with the unit of randomization this is itself the cluster (we label these "clustered comparisons").

Third, the use of any valuation other than the respondent's is only appropriate for measures with commodity character. This leaves the number of actual estimates at 16,848, i.e., an average of 78 specifications for each of the 216 tests on average.

We rely on selection algorithms to identify that model which has the strongest support from the data (Clyde, 2003; Hoeting et al., 1999; Raftery, 1995). We initially want to end up with a minimum number of models; we seek one for all clustered and another for all non-clustered comparisons. So-called Akaike weights provide proportional support for a given specification within a set of specifications *K*. To calculate them, we build on the insight that the Akaike Information Criterion AIC is an unbiased estimator of twice minus the log likelihood of a model, and that $e^{-0.5AIC}$ estimates the likelihood of a model conditional on the data (Burnham & Anderson, 2004; Wagenmakers & Farrell, 2004). The proportional support for model *x* within set *K* is therefore defined as

(I) $$w_x = \frac{e^{-0.5AIC_x}}{\sum_{k=1}^{K} e^{-0.5AIC_k}}$$

We define $K$ as the set of specifications within a given test and will conduct this procedure for all 216 tests, then average model weights across tests. A Bayesian interpretation is that model weights are posterior probabilities conditional on the data, assuming that prior probabilities had been equally distributed across all models. The model with the highest weight is most likely to be the best model, given the available data.

Both numerator and denominator can take on explosive values, making (I) impossible to compute (Burnham & Anderson, 2004). In practice, we therefore compute Akaike weights as

(II) $$w_x = \frac{e^{-0.5(AIC_x - AIC_{\min})}}{\sum_{k=1}^{K} e^{-0.5(AIC_k - AIC_{\min})}}$$

Here $AIC_{\min}$ is defined as the minimum Akaike Information Criterion in set K.

It is not standard to extend model selection processes to the challenge of variable operationalization; this step therefore involves elevated discretion. To ground it in an informed and transparent process, we build on Simonsohn, Simmons, & Nelson (2015) by visually plotting the results of a universe of plausible operationalizations behind a given test. For this purpose, we will visually identify every combination of

operationalizations that are under consideration, as well as each corresponding intent-to-treat effect and p value. This allows us to learn how sensitive results are to alternative specification choices.

We will be left with 216 preferred estimates: 36 intent-to-treat coefficients and associated p values (i.e., one for each of the 12 outcomes and three survey rounds) across six comparison groups. To account for multiple inference among these residual estimates, we control for the false discovery rate (Benjamini & Hochberg, 1995), reporting sharpened q values following the two-stage method coded and described by Anderson (2008). We apply these adjustments across all estimates within a given comparison group and outcome class. Put differently, we apply it separately to each set of p values that is displayed in each individual table of Appendix IV: Tables.[14] Each of these tables corresponds to our definition of the individual hypothesis.

***Empirical Strategy: Other Outcomes***

For all outcomes other than the set of main economic poverty indicators, we present two specifications. The first will use simple linear regression and the second will use the preferred model as defined by the aforementioned model selection process. We apply both specifications to all measures. Estimates for non-economic outcomes do not feed back into the model selection process; we wish to avoid a scenario where the main analyses might be tipped by more exploratory ones. Standard errors are clustered in so-called clustered comparisons (Abadie, Athey, Imbens, & Woolridge, 2017). When outcome data are available at the individual level, our unit of analysis will correspond to the individual as opposed to the household. Logistic regression is applied whenever outcomes are binary, and estimates presented as odds ratios. As before, we apply false discovery adjustments for each comparison group and outcome group separately, meaning that each table presented in Appendix IV: Tables is subject to a dedicated false discovery rate adjustment.

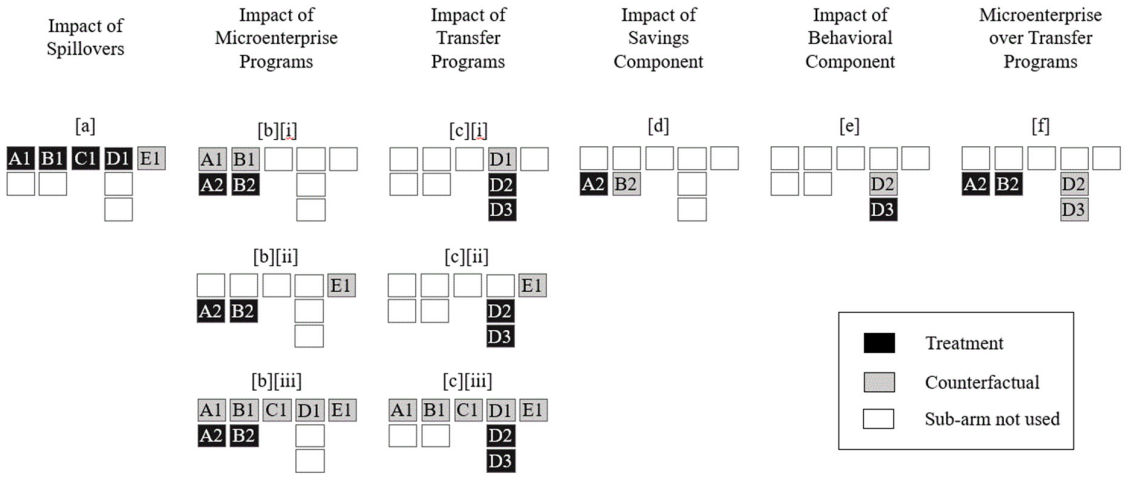
***RESULTS***

[14] The one exception is Table 18, which aggregates selected q values from Table 19 through Table 24.

### *Balance Checks*

As discussed, we are evaluating six comparisons; for two of them (the ones for which the counterfactual is an untreated control group), three sets of counterfactuals are available. This initially brings us to nine available comparison sets; for ease of reference, they are illustrated in Figure 4.

**Figure 4: Available Comparison Sets**




Notes: For the definition of sub-arms A1, A2, etc, consult Figure 1. For more detailed definitions of comparisons [a]-[f] and alternative counterfactuals [i]-[iii], consult the section "Universe of Plausible Analyses".

We first test for covariate balance in each of these comparison sets. As illustrated in Table 2, all comparison sets are well balanced at baseline.

**Table 2: Covariate Balance by Comparison Set**

| | Comparison set: | [a] | [b][i] | [b][ii] | [b][iii] | [c][i] | [c][ii] | [c][iii] | [d] | [e] | [f] |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Average value | p values | | | | | | | | | |
| HH size | 5.91 | 0.21 | 0.62 | 0.38 | 0.85 | 0.26 | 0.32 | 0.67 | 0.66 | 0.50 | 0.71 |
| Age of HH Head | 43.1 | 0.58 | 0.19 | 0.46 | 0.08 | 0.54 | 0.13 | 0.16 | 0.58 | 0.94 | 0.04 |
| HH Head's years of schooling | 5.32 | 0.97 | 0.44 | 0.98 | 0.81 | 0.56 | 0.97 | 0.91 | 0.37 | 0.25 | 0.99 |
| HH Head is female | 0.29 | 0.67 | 0.44 | 0.75 | 0.97 | 0.63 | 0.96 | 0.76 | 0.59 | 0.67 | 0.70 |
| HH Head is monogamously married | 0.56 | 0.77 | 0.88 | 0.97 | 0.80 | 0.14 | 0.13 | 0.07 | 0.41 | 0.24 | 0.09 |
| HH Head is literate | 0.47 | 0.81 | 0.25 | 0.81 | 0.95 | 0.31 | 0.74 | 0.86 | 0.91 | 0.28 | 0.98 |
| HH has iron roof | 0.26 | 0.59 | 0.02 | 0.99 | 0.47 | 0.62 | 0.98 | 0.60 | 0.86 | 0.76 | 0.91 |
| HH has mud walls | 0.40 | 0.99 | 0.90 | 1.00 | 0.98 | 0.60 | 0.88 | 0.84 | 0.80 | 0.90 | 0.89 |
| HH has earth floor | 0.97 | 0.16 | 0.53 | 0.35 | 0.91 | 0.48 | 0.11 | 0.26 | 0.41 | 0.64 | 0.31 |
| HH has sanitary toilet / latrine | 0.41 | 0.54 | 0.33 | 0.72 | 0.97 | 0.34 | 0.60 | 0.78 | 0.34 | 0.21 | 0.87 |
| HH uses wood as main cooking fuel | 0.98 | 0.90 | 0.03 | 0.13 | 0.02 | 0.51 | 0.47 | 0.32 | 0.54 | 0.41 | 0.62 |
| HH uses electric light | 0.02 | 0.64 | 0.55 | 0.97 | 0.72 | 0.22 | 0.14 | 0.19 | 0.26 | 0.27 | 0.16 |
| HH owns its home | 0.88 | 0.61 | 0.29 | 0.97 | 0.61 | 0.87 | 0.88 | 0.85 | 0.07 | 0.30 | 0.92 |
| All HH members have two sets of clothes | 0.62 | 0.87 | 0.18 | 0.46 | 0.27 | 0.11 | 0.39 | 0.29 | 0.39 | 0.62 | 0.13 |
| All HH members have a pair of shoes | 0.23 | 0.36 | 0.96 | 0.29 | 0.38 | 0.24 | 0.53 | 0.17 | 0.97 | 0.93 | 0.12 |
| **Test of joint orthogonality** | | **0.98** | **0.29** | **0.97** | **0.57** | **0.76** | **0.66** | **0.61** | **0.86** | **0.89** | **0.42** |

Notes: Data are derived from the baseline. The first column lists the covariates. The first three are continuous variables; and the others are binary. The second column shows the average value of the covariate across all observations. In the case of binary covariates, this represents the proportion of households for which the dummy is coded to 1 ("yes"). The subsequent columns are derived from nine linear regressions (one per comparison set) of the form $T_{ij} = \alpha + \beta X_{ij} + \varepsilon_{ij}$, where $T$ is the applicable intent-to-treat assignment dummy of household $i$ in village $j$. The p values correspond to each coefficient in the vector of covariates $X$. Standard errors are adjusted for cluster robustness in so-called clustered comparison sets, i.e.: [a], [b][ii], [b][iii], [c][ii], [c][iii], [d], and [f]. The test of joint orthogonality reports the p value of the $F$ statistic for each of the nine regressions.

As some indications of differential attrition are discernible among the sub-arms in Table 11 (see "Appendix II: Participant Flow"), we test for differential attrition in each of the comparison groups. The results are presented in Table 3. Comparison sets [c] and [f] are indeed afflicted by differential attrition in both the mid- and endline. We assess the robustness of these comparisons to alternative assumptions about attritors, follow the trimming procedures proposed by Lee (2009) to put bounds on the treatment effects. We limit this procedure to poverty outcomes.

**Table 3: Tests of Differential Attrition by Comparison Set**

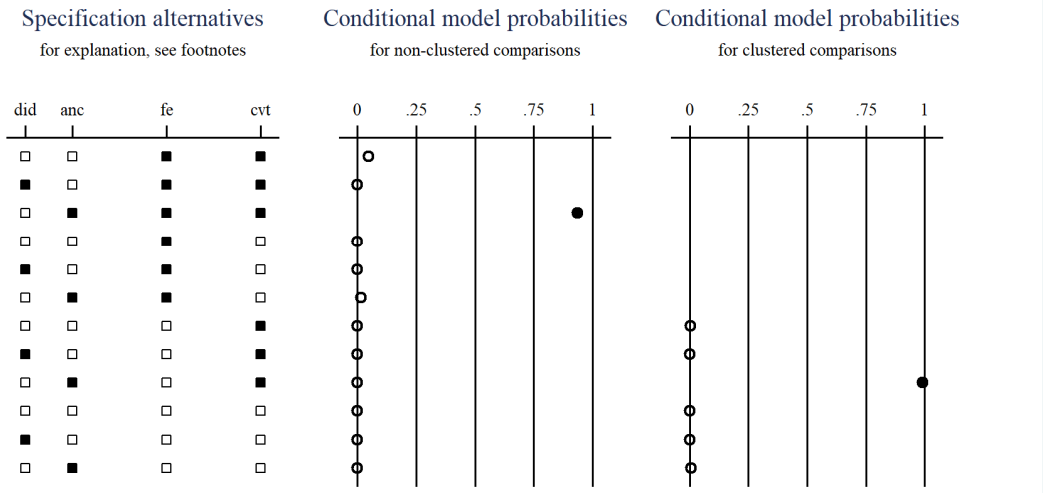
| Comparison set | | Midline | | | | | | | Endline | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Treatment | | | Counterfactual | | | | Treatment | | | Counterfactual | | | |
| | | Surveyed | Attrited | Odds | Surveyed | Attrited | Odds | p value | Surveyed | Attrited | Odds | Surveyed | Attrited | Odds | p value |
| [a] | | 2,164 | 165 | 0.076 | 933 | 62 | 0.066 | 0.530 | 2,113 | 216 | 0.102 | 915 | 80 | 0.087 | 0.386 |
| [b] | [i] | 1,819 | 151 | 0.083 | 1,584 | 115 | 0.073 | 0.297 | 1,783 | 187 | 0.105 | 1,543 | 156 | 0.101 | 0.747 |
| [b] | [ii] | 1,819 | 151 | 0.083 | 933 | 62 | 0.066 | 0.322 | 1,783 | 187 | 0.105 | 915 | 80 | 0.087 | 0.332 |
| [b] | [iii] | 1,819 | 151 | 0.083 | 3,097 | 227 | 0.073 | 0.348 | 1,783 | 187 | 0.105 | 3,028 | 296 | 0.098 | 0.530 |
| [c] | [i] | 463 | 17 | 0.037 | 430 | 33 | 0.077 | 0.016 ** | 453 | 27 | 0.060 | 419 | 44 | 0.105 | 0.026 ** |
| [c] | [ii] | 463 | 17 | 0.037 | 933 | 62 | 0.066 | 0.092 * | 453 | 27 | 0.060 | 915 | 80 | 0.087 | 0.227 |
| [c] | [iii] | 463 | 17 | 0.037 | 3,097 | 227 | 0.073 | 0.020 ** | 453 | 27 | 0.060 | 3,028 | 296 | 0.098 | 0.076 * |
| [d] | | 1,073 | 106 | 0.099 | 746 | 45 | 0.060 | 0.027 ** | 1,059 | 120 | 0.113 | 724 | 67 | 0.093 | 0.340 |
| [e] | | 229 | 8 | 0.035 | 234 | 9 | 0.038 | 0.846 | 223 | 14 | 0.063 | 230 | 13 | 0.057 | 0.791 |
| [f] | | 1,819 | 151 | 0.083 | 463 | 17 | 0.037 | 0.010 ** | 1,783 | 187 | 0.105 | 453 | 27 | 0.060 | 0.057 * |

Notes: p values are derived from logistic regression without covariates. Standard errors are adjusted for cluster robustness in so-called clustered comparisons, i,e: [a], [b][ii], [b][iii], [c][ii], [c][iii], [d], and [f]. For the definition of comparison sets, consult Figure 4.

### *Model Selection*

As discussed in the section on *Empirical Strategy*, we start the model selection process by extracting AIC for each of the 16,848 estimates and calculating Akaike weight $w$ for each estimate. Each weight lies between zero and one; the higher it is, the better the fit of the underlying model. Averaging Akaike weights $w$, we find the model probabilities presented in Figure 5. This clearly prescribes the use of the baseline measure of the outcome in question as a covariate, alongside a set of socioeconomic baseline covariates. In non-clustered comparison sets, it additionally prescribes the use of cluster fixed effects.

**Figure 5: Model Selection**




Notes: Each row in the chart on the left describes one specification. In the same row, the charts on the right show the corresponding Akaike weights. More specifically, read the three charts as follows:

- **"Specification alternatives"**: Columns define specification features. A black square ■ indicates that a choice dimension applies, and a blank square □ that it doesn't. Where two columns are adjacent, three alternatives are available; the third column is not displayed, as it can be inferred that it applies whenever the other two do not apply. The first two columns define the use of baseline data. **did** implies differences in differences, i.e., that baseline data are subtracted from outcome data. **anc** implies an ANCOVA specification where the baseline value of the outcome serves as a covariate. A third choice applies when symbols in both columns are blank: in that case, baseline data is not used. Column **fe** defines if cluster fixed effects are used. This is only an option for so-called non-clustered comparisons. Column **cvt** defines if socioeconomic baseline characteristics are used as covariates. Where this is the case, the least angle regression algorithm proposed by Efron, Hastie, Johnstone and Tibshirani (2004) is applied to the applicable outcome and comparison group data model building purposes and selects five covariates from all those listed in Table 2. The selection process is repeated for each test.
- **"Conditional model probabilities for non-clustered comparisons":** Shows the Akaike weight for each specification; across specifications, these add to 1. The preferred specification is defined as the one with the highest conditional probability and is highlighted through symbol ●.
- **"Conditional model probabilities for clustered comparisons":** Does not compute the weights of specifications using village fixed effects, which would lead to collinearity in clustered comparisons.

We therefore arrive at the following preferred model specification for clustered comparison sets:

(IV) $$y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$$

Here, $y_{ijF}$ is the per-capita outcome in household $i$ in village $j$ at the time of follow-up $F$; $T$ is the randomized assignment, coded to 1 for intent-to-treat and to 0 for the counterfactual; $y_{ijB}$ is the baseline observation of the outcome; and $X_{ijB}$ is a set of socioeconomic baseline covariates. The coefficient for the intent-to-treat estimate is $\beta$. Standard errors are clustered at the village level.

Standard errors are not clustered in non-clustered comparison sets (Abadie, Athey, Imbens, & Woolridge (2017), and the specification includes village fixed effects:

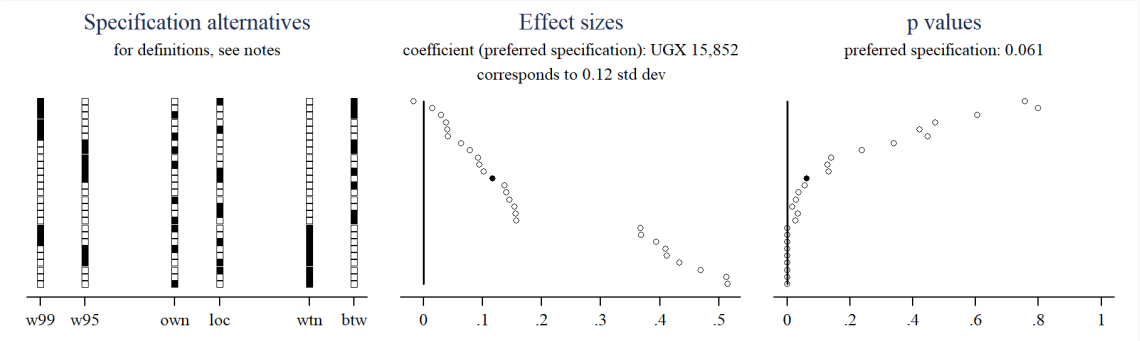
(V) $$y_{ijF} = \alpha_j + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$$

We refer to IV and V jointly as "the preferred specification". It appears consistent with the norms of contemporary economic development research (McKenzie, 2012).

***Selection of Operationalization Rule***

To decide how to operationalize variables, we consult Appendix III: Specification Curves, where effects and p values are plotted as a function of alternative choices. To illustrate the learning and decision process, Figure 6 extracts one example that plots alternative impact estimates of the cash transfer program variants – i.e., comparison [c] – on assets.

**Figure 6: Specification Curve - Impact of Transfer Programs on Net Assets**




Notes: Each row in the chart on the left describes one specification. In the same row, the charts on the right show the corresponding effect size and p value. More specifically, read the three charts as follows:

- **"Specification alternatives"**: Columns define specification features. A black square ■ indicates that a choice dimension applies, and a blank square □ that it doesn't. Where two columns are adjacent, three alternatives are available; the third column is not displayed, as it can be inferred that it applies whenever the other two do not apply. The first two columns define the choice dimension of outlier adjustment. **w99** implies that 0.5% of highest and 0.5% lowest per capita outcomes are recoded to the cutoff value, and **w95** implies that 2.5% of highest and 2.5% lowest per capita outcomes are recoded to the cutoff value. Where symbols in both columns are blank, a third choice (90% winsorization) is applied. The next two columns define the valuation approach that is used. **own** implies that only the respondent's valuation is used; **loc** implies that regional prices (specific to the survey round) are used. Where symbols in both columns are blank, a third option is applied that uses own values except where these are unavailable, in which case loc values are used. (Some goods, such as jewelry assets, are too heterogeneous to allow for a sensible unit valuation across households; for such categories, only the respondent's own valuation is always used. When aggregated with measures that use another valuation rule, the latter valuation rule is displayed.) The final two columns define the choice dimension pertaining to the counterfactual selection. Alternative choices are only applicable in comparison sets [b] ("Impact of Microenterprise Programs") and [c] ("Impact of Cash Transfer Programs). **wtn** implies a comparison within villages, and **btw** implies a between-village comparison. Where symbols in both columns are blank, a third choice applies, and all control groups (A1, B1, C1, D1, and E1) are used as the counterfactual. Note that the paper refers to the first choice as a clustered comparison, and to the latter two as a non-clustered comparison.
- **"Effect sizes":** This displays the intent-to-treat estimates that correspond to the specification choice, presented in terms of standard deviations of the counterfactual. All numbers are per capita, The preferred specification, identified in the paper, is highlighted through a black marker ●. Treatment effects are sorted in ascending order for visual ease.
- **"p values":** These display the corresponding p values, with the preferred specification again highlighted.

We can see that our estimates are not heavily influenced by the choice of the valuation rule.[15] Meanwhile, they are clearly correlated with the choice of the outlier correction rule: after reviewing all charts in Appendix III: Specification Curves, we opt to winsorize at the 95% level. The choice of how to operationalize the counterfactual also appears important: estimates are much more pronounced in within-village comparisons, indicating negative within-village spillover. Between-village controls and all available controls seem to yield similar results, suggesting that spillovers are specific to cash transfer

[15] We simply select that rule that is most representative of all specifications by minimizing the average squared errors of standardized effect sizes; this suggests valuing commodities using local median prices.

villages (arm D). Indeed, no specification curve investigating comparison set [a] indicates that spillovers are significant in the aggregate (see Appendix III: Specification Curves). We opt to use all available controls as our preferred specification; the section "Robustness Checks" will investigate the potential repercussions of spillovers. We are now left with a single preferred specification rule, which is summarized in Figure 7.

**Figure 7: Summary of Analyses**

| Analytical specification choices | | Comparison set | Impact of Spillovers [a] | Impact of Microenterprise Programs [b][iii] | Impact of Transfer Programs [c][iii] | Impact of Savings Component [d] | Impact of Behavioral Component [e] | Impact of Microenterprise over Transfer Programs [f] |
|---|---|---|---|---|---|---|---|---|
| | Model selection | Difference-in-differences (did) | □ | □ | □ | □ | □ | □ |
| | | ANCOVA (anc) | ■ | ■ | ■ | ■ | ■ | ■ |
| | | Cluster fixed effects (fe) | □ | □ | □ | □ | ■ | □ |
| | | Socioeconomic covariates (cvt) | ■ | ■ | ■ | ■ | ■ | ■ |
| | Operationalization | 95% winsorization (w95) | ■ | ■ | ■ | ■ | ■ | ■ |
| | | 99% winsorization (w99) | □ | □ | □ | □ | □ | □ |
| | | Self-reported unit valuation (own) | □ | □ | □ | □ | □ | □ |
| | | Medial local unit valuation (loc) | ■ | ■ | ■ | ■ | ■ | ■ |
| | | Within-village comparison (wtn) | □ | □ | □ | □ | ■ | □ |
| | | Between-village comparison (btw) | ■ | □ | □ | ■ | □ | ■ |
| | Other | Clustered standard errors | ■ | ■ | ■ | ■ | □ | ■ |
| | | Robustness check: differential attrition | □ | □ | ■ | □ | □ | ■ |

Notes: Each column describes the specification for the corresponding comparison set. A black square ■ indicates that a choice dimension applies, and a blank square □ that it doesn't. Where two rows are adjacent are displayed, three alternatives are available; the third row is not displayed, as it can be inferred that it applies whenever the other two do not. (Specifically, the counterfactual in comparison sets [b][iii] and [c][iii] are neither within-village or between-village controls, but all available controls.) See section "Empirical Strategy" for the definitions and procedures, and Figure 4 for a summary of the comparison sets.

### *Impact Estimates*

Table 4 shows the impact estimates on the main economic aggregates, and Table 5 shows them by sub-aggregate. Composite indices of non-economic outcomes are summarized in Table 6. For further outcomes, disaggregated outcomes, and a separation of results by survey round, consult Appendix IV: Tables.

**Table 4: Intent-to-Treat Estimates, Economic Aggregates (UGX per Capita)**

| Comparison set | Impact of Spillovers [a] | Impact of Microenterprise Programs [b][iii] | Impact of Transfer Programs [c][iii] | Impact of Savings Component [d] | Impact of Behavioral Component [e] | Impact of Microenterprise over Transfer Programs [f] |
|---|---|---|---|---|---|---|
| **Total Consumption** | | | | | | |
| Coefficient | -16,462 | 26,061 | -17,141 | 8,833 | -24,982 | 46,294 |
| Error | 18,915 | 11,248 | 19,679 | 21,944 | 29,279 | 22,429 |
| Effect/mean | -0.02 | 0.04 | -0.03 | 0.01 | -0.04 | 0.07 |
| Effect/sd | -0.04 | 0.07 | -0.04 | 0.02 | -0.07 | 0.13 |
| p value | 0.386 | 0.022 ** | 0.385 | 0.689 | 0.394 | 0.042 ** |
| q value | 1.000 | 0.055 * | 0.580 | 1.000 | 1.000 | 0.193 |
| N | 3,094 | 4,906 | 3,545 | 1,812 | 451 | 2,263 |
| **Total Net Assets** | | | | | | |
| Coefficient | -3,640 | 16,343 | 15,852 | -5,917 | 19,283 | -831 |
| Error | 6,789 | 5,449 | 8,397 | 9,048 | 11,479 | 9,627 |
| Effect/mean | -0.03 | 0.14 | 0.13 | -0.04 | 0.15 | -0.01 |
| Effect/sd | -0.03 | 0.12 | 0.12 | -0.04 | 0.15 | -0.01 |
| p value | 0.593 | 0.003 *** | 0.061 * | 0.516 | 0.094 * | 0.931 |
| q value | 1.000 | 0.021 ** | 0.132 | 1.000 | 0.451 | 1.000 |
| N | 3,004 | 3,796 | 2,773 | 1,746 | 462 | 1,819 |
| **Total Productive Cash Inflows** | | | | | | |
| Coefficient | -8,069 | 13,483 | -8,453 | 20,208 | -5,154 | 12,983 |
| Error | 9,273 | 6,747 | 11,740 | 11,007 | 17,309 | 11,309 |
| Effect/mean | -0.06 | 0.10 | -0.06 | 0.15 | -0.04 | 0.09 |
| Effect/sd | -0.04 | 0.07 | -0.04 | 0.10 | -0.03 | 0.07 |
| p value | 0.386 | 0.048 ** | 0.473 | 0.071 * | 0.766 | 0.254 |
| q value | 1.000 | 0.087 * | 0.599 | 0.506 | 1.000 | 1.000 |
| N | 2,529 | 4,021 | 2,916 | 1,885 | 472 | 2,354 |

Notes: The preferred specification is used. Outcomes are pooled across survey rounds. Coefficients are reported in current Ugandan Shillings (UGX). Effect/mean is the coefficient divided by the mean outcome in the counterfactual. Effect/sd is the coefficient divided by the standard deviation of the outcome in the counterfactual. False discovery rate adjustments that form the basis of q values are calculated on the sets of results as defined in each of the tables in Appendix IV: Tables. Standard errors are adjusted for cluster robustness in all comparison sets except [e]. For more detailed results, consult Table 5 (which breaks out impacts on sub-aggregates) and Table 18 in Appendix IV: Tables (where the economic aggregate outcomes are defined in logarithmic terms).

**Table 5: Intent-to-Treat Estimates, Economic Sub-Aggregates (UGX per Capita)**

| | | Comparison set | Impact of Spillovers [a] | Impact of Microenterprise Programs [b][iii] | Impact of Transfer Programs [c][iii] | Impact of Savings Component [d] | Impact of Behavioral Component [e] | Impact of Microenterprise over Transfer Programs [f] |
|---|---|---|---|---|---|---|---|---|
| Consumption | Food and Beverage | Coefficient | 1,169 | 25,180 | -10,261 | 15,944 | -37,416 | 38,623 |
| | | Error | 13,429 | 9,381 | 15,479 | 18,221 | 23,907 | 18,194 |
| | | p value | 0.931 | 0.008 *** | 0.508 | 0.385 | 0.118 | 0.036 ** |
| | | q value | 1.000 | 0.029 ** | 0.599 | 1.000 | 0.463 | 0.193 |
| | Recurring | Coefficient | -5,188 | -1,402 | -7,690 | -1,069 | -2,127 | 6,039 |
| | | Error | 3,420 | 1,917 | 2,819 | 3,282 | 4,574 | 2,924 |
| | | p value | 0.132 | 0.466 | 0.007 *** | 0.746 | 0.642 | 0.042 ** |
| | | q value | 1.000 | 0.296 | 0.045 ** | 1.000 | 1.000 | 0.193 |
| | Infrequent | Coefficient | -5,437 | 2,839 | 1,171 | -4,145 | 13,121 | 442 |
| | | Error | 4,045 | 2,605 | 4,352 | 4,442 | 6,407 | 4,546 |
| | | p value | 0.181 | 0.278 | 0.788 | 0.355 | 0.041 ** | 0.923 |
| | | q value | 1.000 | 0.218 | 0.713 | 1.000 | 0.335 | 1.000 |
| Net Assets | Livestock | Coefficient | -2,671 | 10,584 | 15,155 | -2,438 | 19,185 | -3,504 |
| | | Error | 3,846 | 2,657 | 4,728 | 4,900 | 7,790 | 5,383 |
| | | p value | 0.489 | 0.000 *** | 0.002 *** | 0.621 | 0.014 ** | 0.517 |
| | | q value | 1.000 | 0.002 *** | 0.032 ** | 1.000 | 0.335 | 1.000 |
| | Durable | Coefficient | 917 | 4,440 | 2,172 | -3,794 | 163 | 1,864 |
| | | Error | 3,007 | 2,452 | 4,416 | 3,830 | 5,684 | 4,664 |
| | | p value | 0.761 | 0.072 * | 0.624 | 0.326 | 0.977 | 0.690 |
| | | q value | 1.000 | 0.099 * | 0.646 | 1.000 | 1.000 | 1.000 |
| | Net Financial | Coefficient | 812 | 1,238 | 3,041 | 996 | 1,101 | -1,749 |
| | | Error | 687 | 572 | 1,059 | 964 | 1,661 | 1,087 |
| | | p value | 0.240 | 0.032 ** | 0.005 *** | 0.306 | 0.508 | 0.111 |
| | | q value | 1.000 | 0.070 * | 0.043 ** | 1.000 | 1.000 | 0.527 |
| Productive Cash Inflows | Net Farming Inflows | Coefficient | -5,638 | 1,409 | -2,612 | -876 | 5,863 | 1,224 |
| | | Error | 3,508 | 2,161 | 5,260 | 4,188 | 7,007 | 5,555 |
| | | p value | 0.110 | 0.515 | 0.620 | 0.835 | 0.403 | 0.826 |
| | | q value | 1.000 | 0.304 | 0.646 | 1.000 | 1.000 | 1.000 |
| | Other Self-Employment | Coefficient | -5,648 | 11,862 | 6,417 | 20,169 | -7,411 | 6,927 |
| | | Error | 5,235 | 4,361 | 6,881 | 6,792 | 10,889 | 6,897 |
| | | p value | 0.283 | 0.007 *** | 0.353 | 0.004 *** | 0.496 | 0.318 |
| | | q value | 1.000 | 0.029 ** | 0.575 | 0.082 * | 1.000 | 1.000 |
| | Paid Employment | Coefficient | 800 | -1,088 | -3,472 | -2,198 | -11,333 | 628 |
| | | Error | 3,529 | 2,681 | 4,784 | 4,005 | 5,581 | 4,445 |
| | | p value | 0.821 | 0.686 | 0.469 | 0.585 | 0.043 ** | 0.888 |
| | | q value | 1.000 | 0.378 | 0.599 | 1.000 | 0.335 | 1.000 |

Notes: The preferred specification is used. Outcomes are pooled across survey rounds. Coefficients are reported in current UGX. For results by survey round, consult Appendix IV: Tables. False discovery rate adjustments that form the basis of q values are calculated on the sets of results from these tables. Standard errors are adjusted for cluster robustness in all comparison sets except [e].

**Table 6: Intent-to-Treat Estimates, Composite Indices**

| Comparison set | Impact of Spillovers [a] | Impact of Microenterprise Programs [b][iii] | Impact of Transfer Programs [c][iii] | Impact of Savings Component [d] | Impact of Behavioral Component [e] | Impact of Microenterprise over Transfer Programs [f] |
|---|---|---|---|---|---|---|
| **Nutrition** | | | | | | |
| Coefficient | 0.012 | 0.135 | 0.021 | -0.019 | 0.066 | 0.083 |
| Error | 0.050 | 0.034 | 0.050 | 0.060 | 0.079 | 0.062 |
| p value | 0.809 | 0.000 *** | 0.670 | 0.757 | 0.399 | 0.183 |
| q value | 1.000 | 0.001 *** | 1.000 | 1.000 | 1.000 | 1.000 |
| **Psychological Outook** | | | | | | |
| Coefficient | -0.126 | 0.143 | 0.107 | -0.060 | 0.178 | 0.033 |
| Error | 0.053 | 0.042 | 0.067 | 0.066 | 0.105 | 0.071 |
| p value | 0.019 ** | 0.001 *** | 0.117 | 0.375 | 0.091 * | 0.644 |
| q value | 0.264 | 0.040 ** | 0.880 | 1.000 | 0.819 | 1.000 |
| **Social Conditions** | | | | | | |
| Coefficient | 0.079 | 0.088 | -0.025 | 0.165 | 0.022 | 0.109 |
| Error | 0.060 | 0.041 | 0.061 | 0.074 | 0.112 | 0.069 |
| p value | 0.190 | 0.032 ** | 0.681 | 0.030 ** | 0.847 | 0.116 |
| q value | 0.939 | 0.309 | 1.000 | 0.258 | 1.000 | 0.534 |

Notes: The preferred specification is used. All outcomes are standardized composite indices, following the method used by Kling, Liebman, & Katz (2007). Outcomes are pooled across survey rounds. The nutrition index is the aggregate of the Household Dietary Diversity Score and the inverse of the Household Food Insecurity Access Score. The psychological outlook index is the aggregate of subjective well-being, aspirations, self-control, sense of control, sense of status, and sense of pride. The social conditions index is the aggregate of sense of community, sense of trust, risk sharing, empowerment of women, and the inverse of intimate partner violence. For more detailed results, consult Appendix IV: Tables. This appendix also shows those outcomes that are not averaged across survey rounds, including all binary outcomes in the domains of labor, schooling, and health.

Numerous positive impacts can be identified in the microenterprise program variant, as defined in the comparison set [b][iii]. Impacts on consumption (Table 4) are driven predominantly by gains in food and beverage consumption (Table 5); this is supported by clear nutritional impacts (Table 6). Half of the consumption gains are explained by gains in cash income. These emanate from self-employment, not paid employment.[16] Gains in asset stock are driven by gains in livestock ownership. While impacts on financial assets are detectable, they are low in absolute terms. Savings groups do not appear to enhance asset accumulation: we fail to detect even an impact on the narrow definition of financial assets.[17] Savings groups

[16] Table 44 shows that day labor tended to fall among microenterprise program participants.

[17] An alternative approach to measuring savings positions might involve consulting administrative data on balances in the savings groups established by Village Enterprise. We do not use these data, as they are only available for the sub-arm A2 where this activity was conducted. However, it should be noted that these yield significantly higher positions than self-reported ones provided by survey respondents, pointing to possible under-reporting in the survey.

do however seem to increase self-employment activities, and there are indications that perceptions of social conditions in the community improved.

Alongside objective improvements in living conditions, microenterprise participants report clear gains in psychological outlook. The light-touch psychological intervention also appeared to manipulate psychological outlook, and it did tend to lead to higher asset stocks. But it did not result in improvements in the standard of living as indicated by the economic flow variables (i.e., cash inflows and consumption): combining this intervention with cash transfers did not appear to effectively substitute the integrated graduation-style approach. The results do not suggest that the impacts of the integrated microenterprise program are mediated exclusively via changes in psychological outlook.

Few positive impacts are discernible from the pure cash transfer program.[18] Recipients do report lower loan balances[19] and net assets tend to rise by a comparable monetary amount as in the microenterprise program. But consider that the incremental asset stock must have been substantially greater at the time of program initiation – akin to UGX 35k per capita on average in the cash transfer program, compared to UGX 20k in the case of the microenterprise program. This suggests assets depreciated or were dissaved at higher rates in the cash transfer arms. Yet surprisingly, no effects are detectable on aggregate consumption flows; and unlike in the microenterprise program, no encouraging signals emerge on psychological, nutritional, or social outcomes. This is not explained by a reduction in labor force participation: in fact, Table 45 suggests that labor inputs increased.

### *ROBUSTNESS CHECKS*

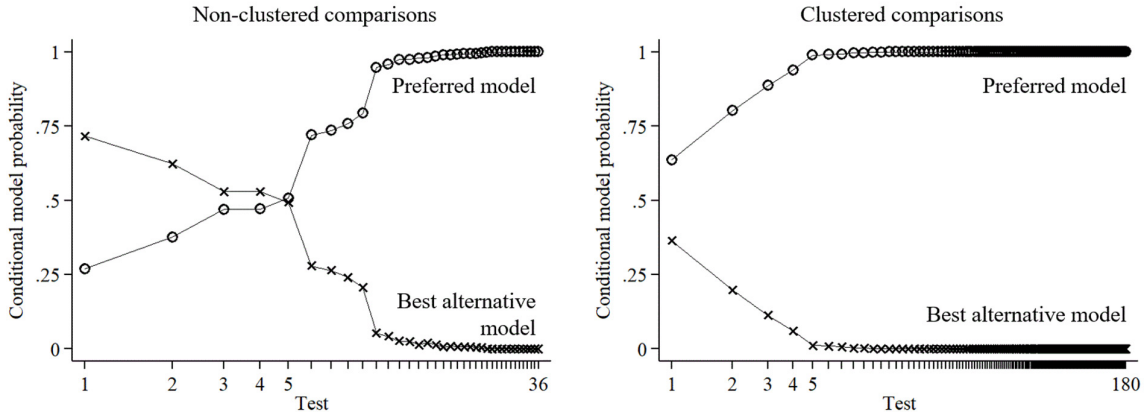
#### *Sensitivity to Model Selection*

By ceding the choice of a "preferred" model to a selection algorithm, we achieved no more than a skilled researcher might have achieved in a pre-analysis plan (Olken, 2015). In doing so, we did not take advantage of the algorithm's full potential to accommodate the collected data.

[18] School attendance and enrolment do tend to increase; see Table 51.
[19] See Table 57.

To explore to what extent the choice of a "preferred" model has restricted our analysis, Figure 8 expands upon Figure 5 by showing the conditional model probabilities of the preferred specification not only on average, but for each one of the 216 tests presented in the "Empirical Strategy" section.

**Figure 8: Conditional Model Probabilities by Test**




Note: Conditional model probabilities are shown for the previously chosen (i.e., "preferred") model as well as the best alternative available model for each of the 216 tests. The scale of the x axis is logarithmic for visual ease.

When the selection algorithm is free to select custom models for each of our 216 tests, it picks a different model than the preferred one in only 4 cases. (This is not surprising: Figure 5 already showed that the data had yielded a dominant model). All four cases relate to comparison set [e], and only one of them relates to outcomes that are pooled across survey rounds. Allowing the model to vary by test does very little to change the main results, as illustrated in Table 7.

The column on the right hand side of Table 7 illustrates the results of a model averaging approach, which takes the Bayesian spirit one step further; rather than using model weights to select a model or multiple models, it uses these to created weighted average coefficients (Burnham & Anderson, 2004). Clearly, choosing one preferred specification had barely restricted our analysis; the main findings remain intact.

**Table 7: Sensitivity of Estimates in Comparison Group [e] to Model Algorithm Alternatives**

| | $\beta_{preferred}$ | $\beta_{testspecific} - \beta_{preferred}$ | $\beta_{averaged} - \beta_{preferred}$ |
|---|---|---|---|
| **Total Consumption** | **-24,982** | **0** | **1** |
| Food & Beverage Consumption | -37,416 | 0 | -18 |
| Recurring Consumption | -2,127 | 0 | 12 |
| Infrequent Consumption | 13,121 | 1,594 | -887 |
| **Total Net Assets** | **19,283** | **0** | **0** |
| Livestock Assets | 19,185 | 0 | 0 |
| Durable Assets | 163 | 0 | 0 |
| Net Financial Position | 1,101 | 0 | -118 |
| **Total Productive Cash Inflows** | **-5,154** | **0** | **0** |
| Net Cash Inflows from Farming | 5,863 | 0 | -289 |
| Income from Other Self-Employment | -7,411 | 0 | 0 |
| Income from Paid Employment | -11,333 | 0 | 4 |

Notes: Outcomes are pooled across survey rounds. Coefficients are reported in current UGX, rounded to the nearest shilling. $\beta_{preferred}$ are the coefficients for comparison set [e] in Table 4 and Table 5. $\beta_{testspecific}$ is the coefficient that is derived from using the model with the highest conditional probability for any given test. $\beta_{averaged}$ is derived from model averaging procedure (Burnham & Anderson, 2004) that weighs all available cofficients with the corresponding Akaike weight:. $\beta_{averaged} = \sum_m^M \beta_m w_m$. Here, M is the set of available models. Only the results for comparison set [e] are shown because all increments are exactly UGX 0 in all other comparison sets when using test-specific model selection, and round to zero in all other comparison sets when using model averaging.

### *Sensitivity to Assumptions about Attriters*

In Table 3 we found that study participants in the cash transfer groups attrited at significantly lower rates than ones in the pure control (comparison set [c]) and the microenterprise groups (comparison set [f]). Table 8 uses the procedure by Lee (2009) to put bounds on the estimates found in these comparison sets; the impact estimates are not robust to this.

**Table 8: Lee Bounds**

| | | Conservatively Trimmed Estimate | | | Aggressively Trimmed Estimate | | |
|---|---|---|---|---|---|---|---|
| | | Total Consumption | Total Net Assets | Total Productive Cash Inflows | Total Consumption | Total Net Assets | Total Productive Cash Inflows |
| Impact of Transfer Programs [c][iii] | Coefficient | -48,001 | 287 | -35,716 | -6,417 | 18,420 | -992 |
| | Error | 17,043 | 7,044 | 8,649 | 20,379 | 8,516 | 11,704 |
| | p value | 0.006 *** | 0.968 | 0.000 *** | 0.753 | 0.032 ** | 0.933 |
| | | Total Consumption | Total Net Assets | Total Productive Cash Inflows | Total Consumption | Total Net Assets | Total Productive Cash Inflows |
| Impact of Microenterprise over Transfer Programs [f] | Coefficient | 33,190 | -4,528 | 4,526 | 79,796 | 16,065 | 39,236 |
| | Error | 23,372 | 9,798 | 11,352 | 19,282 | 7,126 | 9,032 |
| | p value | 0.159 | 0.645 | 0.691 | 0.000 *** | 0.026 ** | 0.000 *** |

Notes: Outcomes are pooled across survey rounds. Coefficients are reported in current UGX. Standard errors are adjusted for cluster robustness. We define an aggressive trim as that which results in a higher estimate; this may involve trimming observations from the bottom of the treatment group or from the top of the counterfactual.

### *Sensitivity to Assumptions about Spillovers*

We found indications of negative spillovers in cash arm D. While spillovers are not significant in the aggregate, they tend negative. We repeat our calculation under the assumption that spillovers have tainted our within-village control groups. Table 9 illustrates the impact estimates for the microenterprise and cash transfer programs when using only control villages (sub-arm E1) as the counterfactual. Confidence intervals expand and point estimates are more attenuated.

**Table 9: Between-Village Counterfactuals**

| | | Total Consumption | Total Net Assets | Total Productive Cash Inflows |
|---|---|---|---|---|
| Impact of Microenterprise Programs [b][ii] | Coefficient | 15,164 | 13,517 | 9,057 |
| | Error | 19,131 | 7,265 | 9,470 |
| | p value | 0.430 | 0.066 * | 0.341 |
| | | Total Consumption | Total Net Assets | Total Productive Cash Inflows |
| Impact of Transfer Programs [c][ii] | Coefficient | -31,299 | 14,696 | -12,026 |
| | Error | 25,552 | 9,629 | 12,124 |
| | p value | 0.225 | 0.131 | 0.325 |

Notes: Outcomes are pooled across survey rounds. Coefficients are reported in current UGX. Only E1 is used as the counterfactual; this corresponds to the comparison sets ending in [ii].

### *COSTING*

We discussed the project's objective to align the costs of the microenterprise and cash transfer program variants. But implementation costs were also monitored over the course of the program, and incurred costs differed somewhat from budgeted costs. Managerial costs were higher than anticipated because of the administrative demands associated with the randomized implementation. Table 10 allocates incurred costs (as quantified in the implementer's internal financial reports) to each of the intervention sub-arms; for expenses that cannot be directly allocated, the relative time intensity of activities of the different sub-arms is used as an allocation key ("activity-based costing"). The simplest measure of financial return is the payback period: the time it would take the incremental flow of benefits gains to fully offset the investment. The results appear encouraging for the microenterprise program: Given the per-household the household size of roughly 6 members per household (see Table 2) and annual consumption gains of about UGX 26k per capita (see Table 4), the cost of the microenterprise program of UGX 571k[20] would be offset in less than four years. Accounting further for the gains in per capita asset stock of UGX 16k, the payback period comes close to three years. In other words, break-even was plausibly achieved not far beyond the measurement period.[21] If we wish to engage in more thorough cost-benefit assessments[22] and believe that gains continue beyond the measurement period, estimated benefits will be heavily influenced by relatively arbitrary assumptions, e.g., about the long-term interpretation of short-term consumption and asset outcomes, and about the appropriate discount rate. These assumptions could influence our relative appraisal of the full microenterprise program versus that without savings groups.[23] It is not likely that they would influence the relative appraisal to the transfer programs, which failed to yield any detectable impacts – indeed, even positive point estimates – on consumption.

[20] This is the average cost of arms A2 and B2, weighted by the number of study participants; see Table 10.
[21] If we assume (as was done in the section "Robustness Checks") that the appropriate counterfactual set is not [b][iii] but [b][ii], the payback period comes close to 6 years.
[22] To deflate the UGX values presented in this paper, consult "Appendix I: Exchange Rates".
[23] Consumption estimates are higher in the former, but asset estimates are slightly higher in the latter. If we assume that asset positions predict consumption trajectories, then under certain conditions we may appraise the microenterprise program without savings groups more highly.

**Table 10: Activity-Based Costing of Sub-Arms**

| Activity[1] | *Field h/ activity* | Controls: A1, B1, C1, D1, E1 | Microenterprise: A2 | Microenterprise minus Savings: B2 | Business-in-a-Box: C2 | Transfers: D2 | Transfers plus Behavioral Intervention: D3 |
|---|---|---|---|---|---|---|---|
| Community mapping | *2* | ● | ● | ● | ● | ● | ● |
| Targeting | *24* | ● | ● | ● | ● | ● | ● |
| Cash transfer delivery | *12* | ○ | ● | ● | ● | ● | ● |
| Training: business administration | *34* | ○ | ● | ● | ● | ○ | ○ |
| Training: savings groups | *26* | ○ | ● | ○ | ● | ○ | ○ |
| Training: behavioral | *12* | ○ | ○ | ○ | ○ | ○ | ● |
| Training: asset management | *6* | ○ | ○ | ○ | ● | ○ | ○ |
| Training: other | *8* | ○ | ● | ● | ● | ○ | ○ |
| Business group coaching | *60* | ○ | ● | ● | ● | ○ | ○ |
| Savings group coaching | *6* | ○ | ● | ○ | ● | ○ | ○ |
| Field hours (per savings group) | | 26 | 172 | 140 | 178 | 38 | 50 |
| Trial households | | 3,324 | 1,179 | 791 | 186 | 243 | 237 |
| Total field-hours | | 2,881 | 6,760 | 3,691 | 1,104 | 308 | 395 |
| Cost allocation key[2] | | 19.03% | 44.65% | 24.38% | 7.29% | 2.03% | 2.61% |
| **Village Enterprise expenses, USD[3]** | **Total** | | | | | | |
| Field delivery costs[4] | 94,738 | 18,029 | 42,303 | 23,101 | 6,907 | 1,926 | 2,472 |
| Cash transfers[5] | 156,326 | - | 54,145 | 36,326 | 8,542 | 29,015 | 28,299 |
| *Subtotal* | *251,064* | *18,029* | *96,448* | *59,427* | *15,448* | *30,941* | *30,771* |
| Other Ugandan program costs[6] | 227,948 | 43,379 | 101,785 | 55,584 | 16,618 | 4,635 | 5,948 |
| *Subtotal* | *479,012* | *61,407* | *198,233* | *115,011* | *32,066* | *35,576* | *36,718* |
| Int't program & overhead costs[7] | 169,840 | 32,321 | 75,838 | 41,414 | 12,382 | 3,453 | 4,432 |
| **Grand total** | **648,852** | **93,728** | **274,071** | **156,425** | 44,448 | **39,029** | **41,150** |
| **Cost per household, UGX[8]** | | | | | | | |
| Field delivery costs | | 14,172 | 93,756 | 76,313 | 97,026 | 20,714 | 27,255 |
| Cash transfers | | - | 120,000 | 120,000 | 120,000 | 312,000 | 312,000 |
| *Subtotal* | | *14,172* | *213,756* | *196,313* | *217,026* | *332,714* | *339,255* |
| Other Ugandan program costs | | 34,100 | 225,585 | 183,616 | 233,454 | 49,839 | 65,577 |
| *Subtotal* | | *48,272* | *439,341* | *379,928* | *450,480* | *382,552* | *404,832* |
| Int'l program & overhead costs | | 25,407 | 168,079 | 136,809 | 173,943 | 37,134 | 48,860 |
| **Grand total** | | **73,680** | **607,420** | **516,737** | 624,423 | **419,686** | **453,692** |

Notes: (1) Field hours by activity are quantified by savings group (the typical unit of training) and include field transport time. Symbol ● indicates that the activity applies to the sub-arm in question. Group C2 is included to enable a full accounting of organizational costs. (2) We divide the number of field-hours per activity by 30 (i.e., the average savings group size) and multiply it by the number of trial participants to arrive at total field-hours spent per intervention. The cost allocation key is the proportion of total field hours. (3) Totals are based on internal financial reports of Village Enterprise. (4) Includes direct compensation and logistical costs associated with field coordinators, trainers, coaches; costed using allocation key. (5) Costed using exchange rates at intervention time; excludes rate gains / losses from mismatch between withdrawal and disbursement. (6) Includes internal monitoring & evaluation, administrative, and managerial costs incurred in Uganda. Costed using allocation key. (7) Includes US-based administrative, managerial, and fundraising costs. Costed using allocation key.(8) Translated to UGX using the nominal exchange rate at the time of intervention (see Appendix I: Exchange Rates) and divided by the number of households in the corresponding sub-arm.

## *DISCUSSION*

Consistent with Banerjee et al. (2015), Bandiera et al. (2017), and Blattman et al. (2016), we found that an integrated microenterprise program targeting people in extreme poverty increased entrepreneurial activity and reduced poverty. Impacts on key economic outcomes are significant, robust to multiple inference adjustments, and supported by consistent signals on subjective well-being and nutrition. The program seems unusually cost-effective, with estimated gains having nearly offset the full costs of the program within the 3-year measurement period. That said, it is possible that negative spillovers in the control group may have biased the results upward. The point estimates on the microenterprise program also indicate a possible attenuation in poverty effects over time, so we are not able to speak confidently to the sustainability of gains.

Like Karlan et al. (2017), we found that savings groups did little to encourage saving but that they did increase microenterprise activity and alter conditions in the community, especially with regards to the standing of women. Consistent with Banerjee, Karlan, Osei, Trachtman, & Udry (2018), Berge et al. (2015), and Chowdhury et al. (2017), the program seems to lose impact when reduced to mere transfers. Combining transfers with a light-touch psychological intervention did tend to manipulate psychological outlook and increase investment in productive (livestock) assets, but we do not see any indication of improvements consumption or cash inflows.

Consistent with Banerjee, Hanna, Kreindler, & Olken, (2015) we do not find that cash transfers reduced labor market participation – on the contrary. That said, the estimated economic impacts of the pure cash transfers were very muted in the medium term. We encounter some residual asset stock but no indications of increased income or consumption. Taken at face value, the results indicate that participants dissaved and consumed some assets quickly after the intervention, and that they did not invest the rest productively. This seems to vindicate the implementer's belief that without dedicated training and coaching, transfer recipients struggle to maintain their newly received assets and derive sustained economic value from them. Like the data used by Haushofer & Shapiro (2018), ours indicates negative spillovers in cash villages.

The impacts of pure cash transfers must be interpreted with ample caution. Participants who were assigned to cash transfers chose to respond to the follow-up surveys at higher rates than participants in the pure

control and microenterprise groups; this could have introduced attrition bias. Also, the pattern of results - low reported economic impacts of cash overall, high reported impacts on labor market participation, and apparent negative spillovers in cash villages – raises the question if "motivated misreporting" may have played a role. Prior exposure to the cash transfer program could have conceivably led respondents in cash transfer villages to incorrectly assume that our survey enumerators might be involved in the selection of future transfer recipients, leading to differential misreporting of economic conditions. While the possibility of misreporting is widely acknowledged, the systematic under-reporting of socioeconomic conditions with the objective of gaining future benefits has received little empirical support to date (Baird & Özler, 2012; Beegle, De Weerdt, Friedman, & Gibson, 2012; Moore, Stinson, & Welniak, 2000). That said, the tendency to misreport could be driven by social processes (say, village debates about fairness and equity) that are context-specific and erratic. Beyond the patterns of results, we have no basis – not even anecdotes – that suggest differential misreporting. But we consider the possibility a real one and underline the merits of corroborating survey measures with ones that are not self-reported.

Overall, the results support the notion that integrated microenterprise programs in the spirit of ultra-poor graduation are sensible poverty reduction tools. While some caution is warranted, they also tend to support the more specific belief of the implementer that an integrated package, designed with presumed market failures and internalities in mind, cannot be easily stripped of its components without adverse consequences.

## *APPENDIX I: EXCHANGE RATES*

All calculations are conducted in current Ugandan shillings (UGX). Where current USD numbers, 2016 USD numbers, and 2016 PPP USD numbers are reported, they are derived directly from UGX numbers, using UGX/USD midpoint rates from daily xe.com data for nominal rates; annual World Bank data for PPP rates; and monthly data from the US Bureau of Labor Statistics for USD inflation. The effective dates and corresponding rates used in this paper are as follows:

(a) The outset of the project is defined as the initial trial registration date, 8 Dec 2013. Applicable rates: 2,520 (current USD terms), 2,611 (2016 USD terms), 1,049 (2016 USD PPP terms).

(b) The baseline date is defined as half way through the planned survey time frame (15 March 2014). Applicable rates: 2,520 (current USD terms), 2,575 (2016 USD terms), 1,063 (2016 USD PPP terms).

(c) The intervention date is defined as the UGX-weighted average transfer date (1 Aug 2014). Applicable rates: 2,613 (current USD terms), 2,652 (2016 USD terms), 1,056 (2016 USD PPP terms).

(d) The midline date is defined as half way through the planned midline survey time frame (15 Nov 2015). Applicable rates: 3,468 (current USD terms), 3,528 (2016 USD terms), 1,008 (2016 USD PPP terms).

(e) The pooled follow-up date is defined as half way through the planned survey time frame of both mid- and endline (15 May 2016). Applicable rates: 3,323 (current USD terms), 3,340 (2016 USD terms), 1,094 (2016 USD PPP terms).

(f) The endline date is defined as half way through the planned endline survey time frame (15 Nov 2016). Applicable rates: 3,556 (current USD terms), 3,557 (2016 USD terms), 1,146 (2016 USD PPP).

## *APPENDIX II: PARTICIPANT FLOW*

**Table 11: Participant Flow over the Project's Life, by Sub-Arm and Cohort**

| Sub-arm | (1) Available Participant Slots | | | | (2) Successful Baseline | | | |
|---|---|---|---|---|---|---|---|---|
| | Cohort #1 | Cohort #2 | Cohort #3 | All | Cohort #1 | Cohort #2 | Cohort #3 | All |
| A1 | 360 | 360 | 360 | 1,080 | 347 | 331 | 336 | 1,014 |
| A2 | 420 | 420 | 420 | 1,260 | 404 | 384 | 391 | 1,179 |
| B1 | 240 | 240 | 240 | 720 | 229 | 235 | 221 | 685 |
| B2 | 280 | 280 | 280 | 840 | 266 | 265 | 260 | 791 |
| C1 | 60 | 60 | 60 | 180 | 54 | 57 | 56 | 167 |
| D1 | 168 | 168 | 168 | 504 | 156 | 155 | 152 | 463 |
| D2 | 84 | 84 | 84 | 252 | 81 | 80 | 82 | 243 |
| D3 | 84 | 84 | 84 | 252 | 78 | 81 | 78 | 237 |
| E1 | 360 | 360 | 360 | 1,080 | 341 | 322 | 332 | 995 |
| Total | 2,056 | 2,056 | 2,056 | 6,168 | 1,956 | 1,910 | 1,908 | 5,774 |

| Sub-arm | (3) Successful Midline | | | | | (4) Successful Endline | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Cohort #1 | Cohort #2 | Cohort #3 | All | Attrition[1] | Cohort #1 | Cohort #2 | Cohort #3 | All | Attrition[1] |
| A1 | 316 | 302 | 321 | 939 | 7.40% | 308 | 285 | 320 | 913 | 9.96% |
| A2 | 358 | 350 | 365 | 1,073 | 8.99% | 354 | 335 | 370 | 1,059 | 10.18% |
| B1 | 215 | 219 | 211 | 645 | 5.84% | 209 | 214 | 207 | 630 | 8.03% |
| B2 | 255 | 246 | 245 | 746 | 5.69% | 249 | 230 | 245 | 724 | 8.47% |
| C1 | 43 | 54 | 53 | 150 | 10.18% | 47 | 52 | 52 | 151 | 9.58% |
| D1 | 144 | 139 | 147 | 430 | 7.13% | 138 | 136 | 145 | 419 | 9.50% |
| D2 | 78 | 78 | 78 | 234 | 3.70% | 77 | 74 | 79 | 230 | 5.35% |
| D3 | 77 | 77 | 75 | 229 | 3.38% | 75 | 72 | 76 | 223 | 5.91% |
| E1 | 314 | 304 | 315 | 933 | 6.23% | 310 | 297 | 308 | 915 | 8.04% |
| Total | 1,800 | 1,769 | 1,810 | 5,379 | 6.84% | 1,767 | 1,695 | 1,802 | 5,264 | 8.83% |

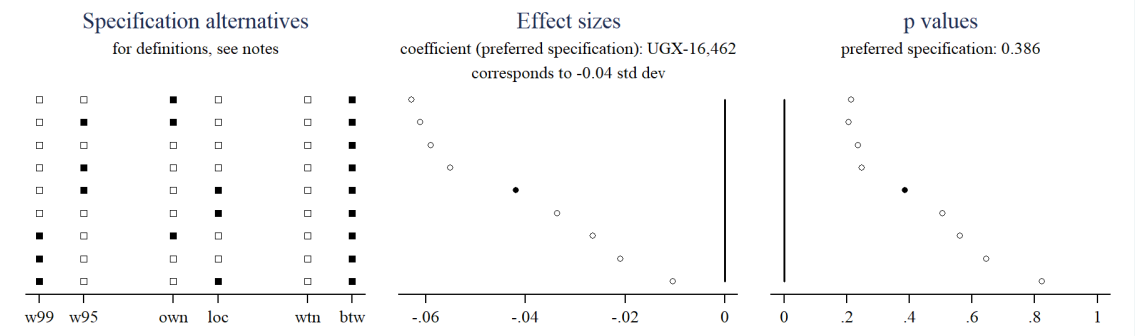
Notes: Available participant slots correspond to the numbers in Figure 1. Only respondents who consented to and completed to baseline survey were recruited into the study. Of the resulting study population, follow-ups were successful with 93% and 91% of respondents in the two respective follow-up surveys. (1) Attrition is defined as the share of baseline survey participants for whom the corresponding follow-up survey was unsuccessful.

## *APPENDIX III: SPECIFICATION CURVES*

As discussed above, this section is ultimately intended to become an online appendix that complements the journal article.

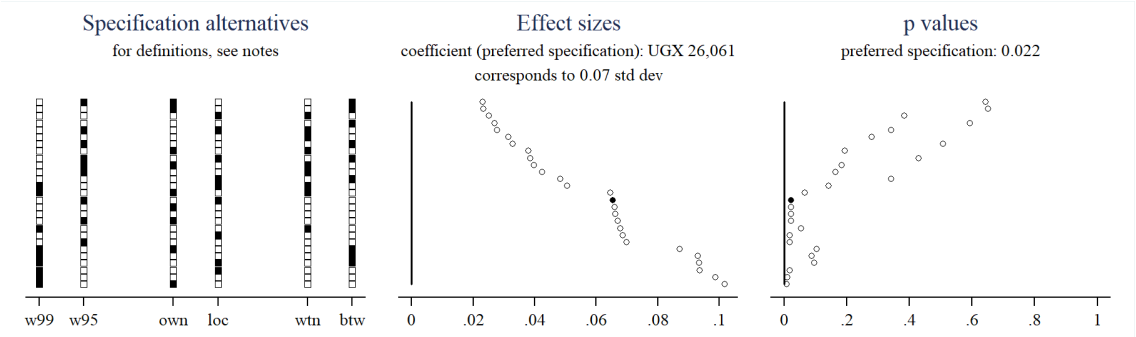
Specification curves display the intent-to-treat statistics that emerge from different combinations of plausible analytical specifications for any given test. Tests are displayed for all of the comparison sets. To avoid overwhelming the reader, only the results of total composite outcomes, pooled across follow up rounds, are included.

**Figure 9: Impact of Spillovers on Consumption**



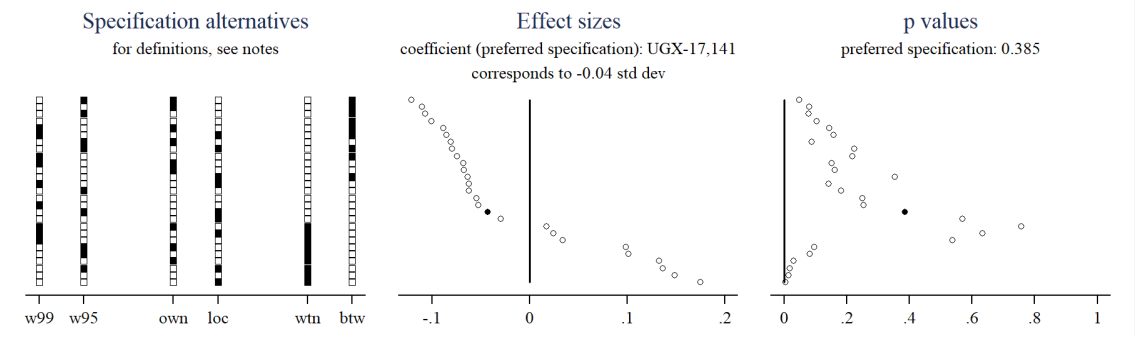

The notes from Figure 6 apply.

**Figure 10: Impact of Microenterprise Programs on Consumption**



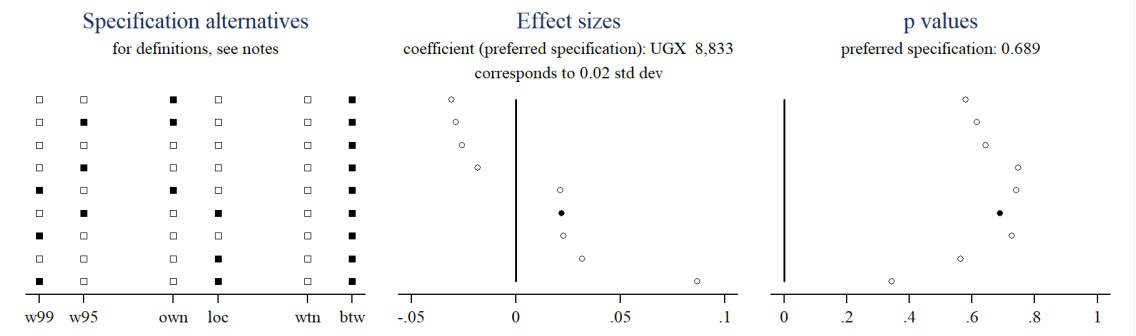

The notes from Figure 6 apply.

**Figure 11: Impact of Transfer Programs on Consumption**



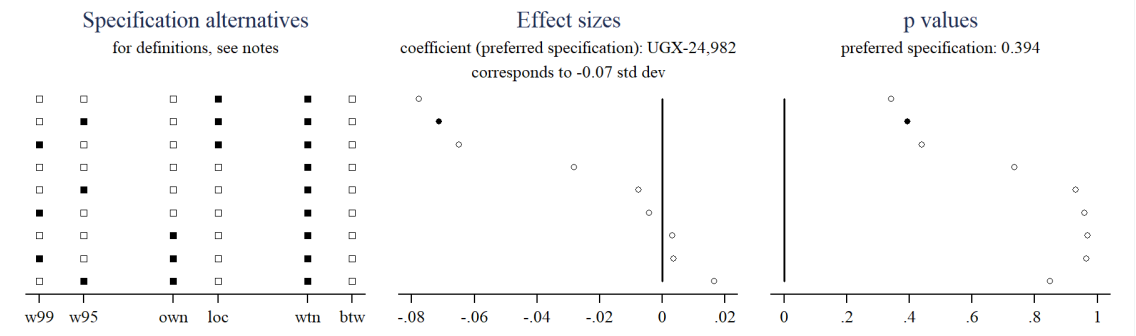

The notes from Figure 6 apply.

**Figure 12: Impact of Savings Component on Consumption**



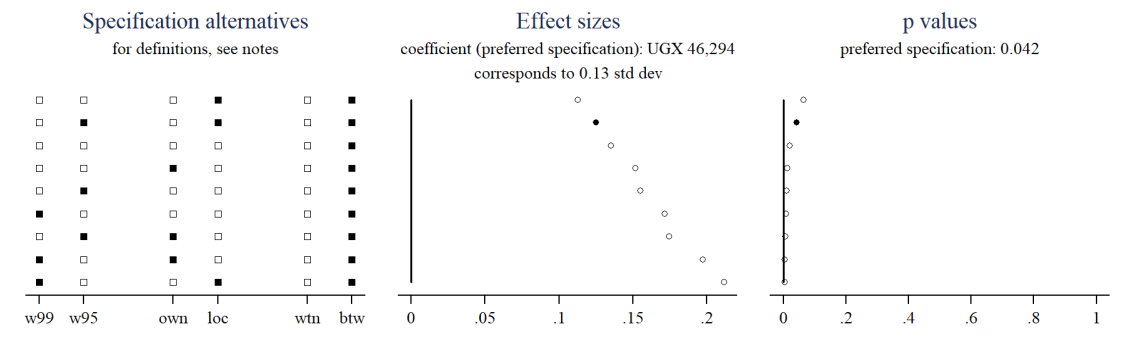

The notes from Figure 6 apply.

**Figure 13: Impact of Behavioral Component on Consumption**



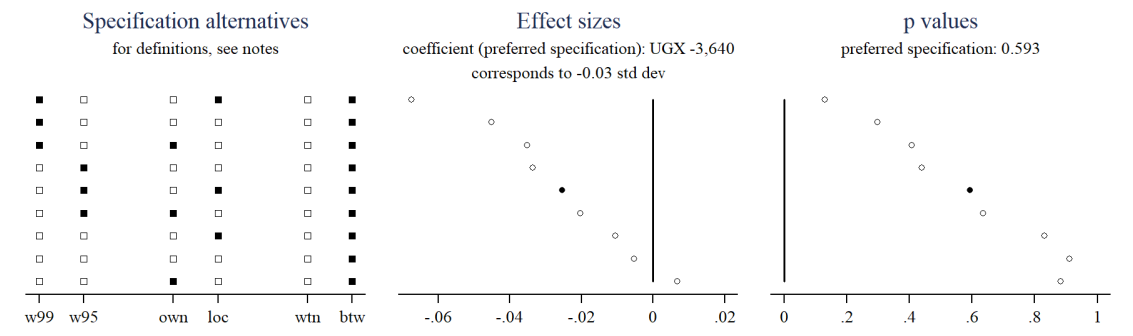

The notes from Figure 6 apply.

**Figure 14: Impact of Microenterprise over Transfer Programs on Consumption**



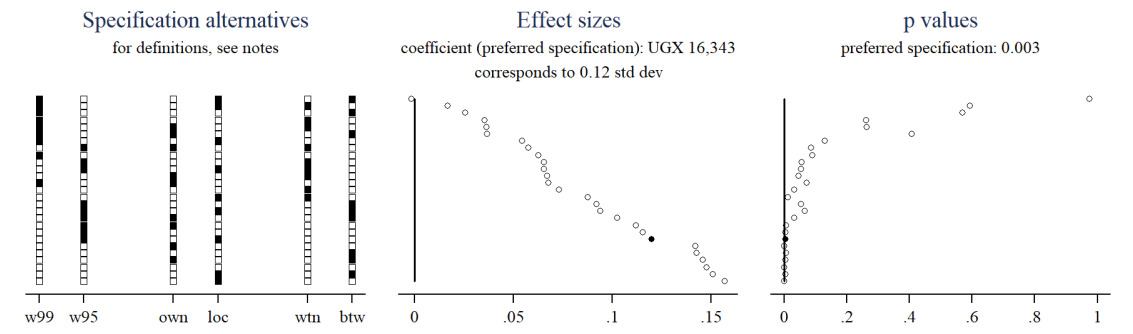

The notes from Figure 6 apply.

**Figure 15: Impact of Spillovers on Net Assets**



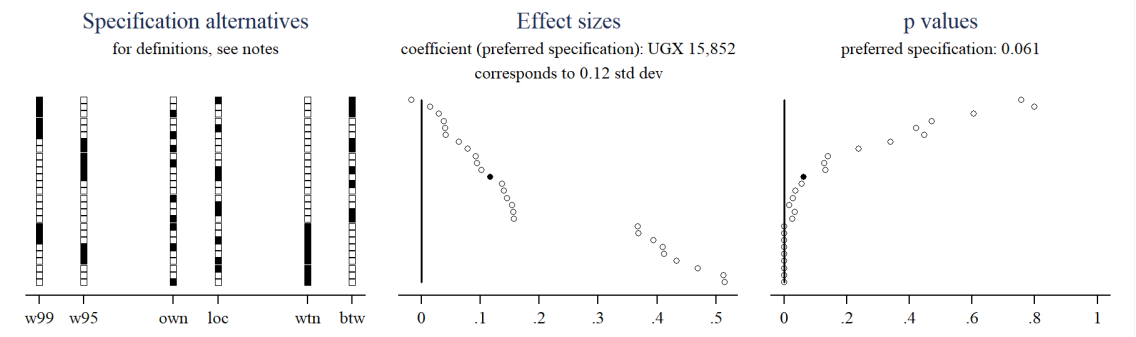

The notes from Figure 6 apply.

**Figure 16: Impact of Microenterprise Programs on Net Assets**



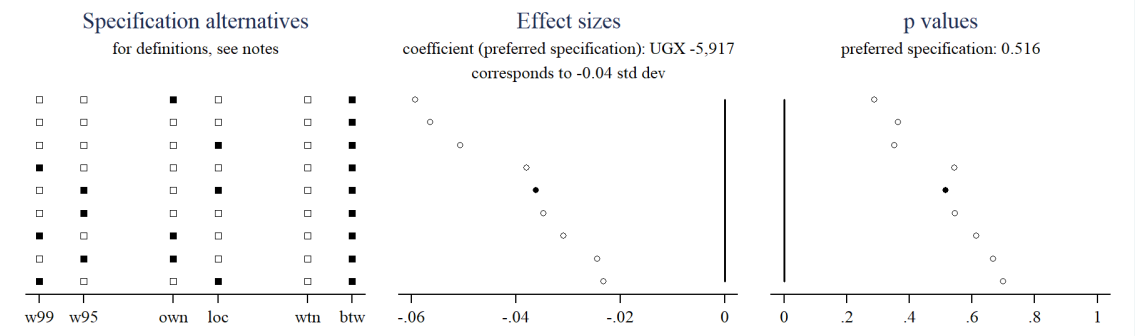

The notes from Figure 6 apply.

**Figure 17: Impact of Transfer Programs on Net Assets**



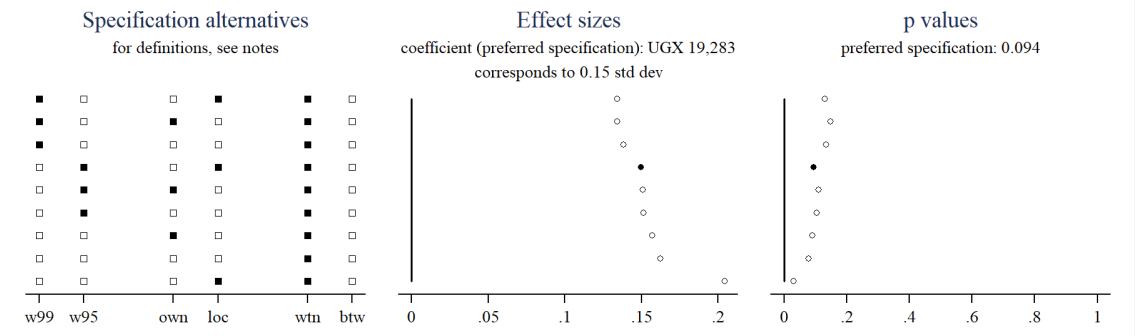

The notes from Figure 6 apply.

**Figure 18: Impact of Savings Component on Net Assets**



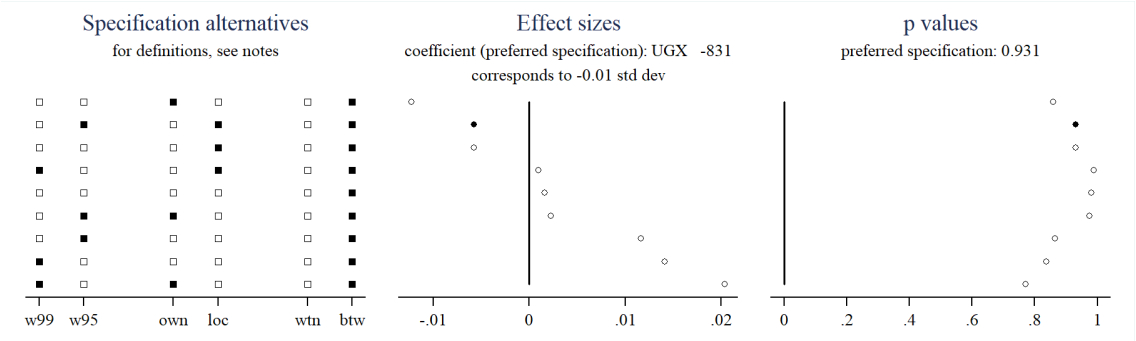

The notes from Figure 6 apply.

**Figure 19: Impact of Behavioral Component on Net Assets**



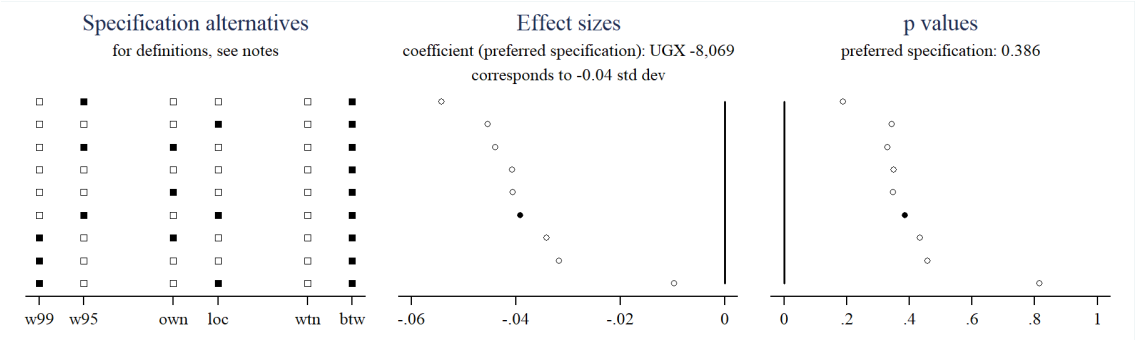

The notes from Figure 6 apply.

**Figure 20: Impact of Microenterprise over Transfer Programs on Net Assets**



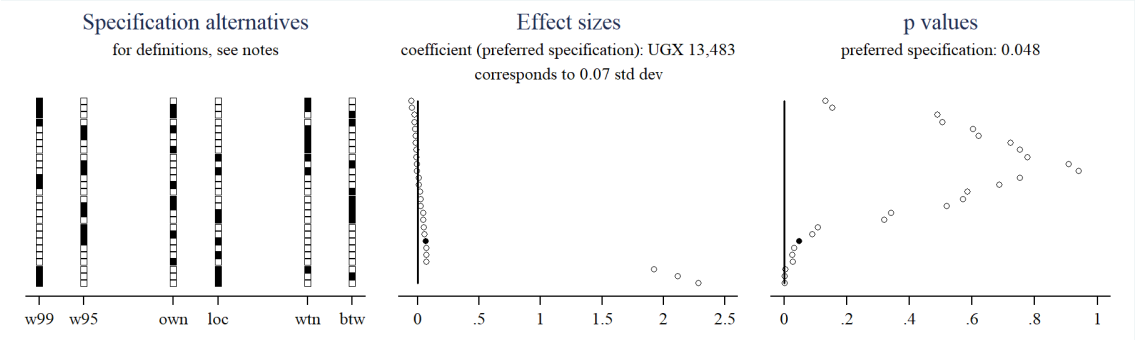

The notes from Figure 6 apply.

**Figure 21: Impact of Spillovers on Productive Cash Inflows**



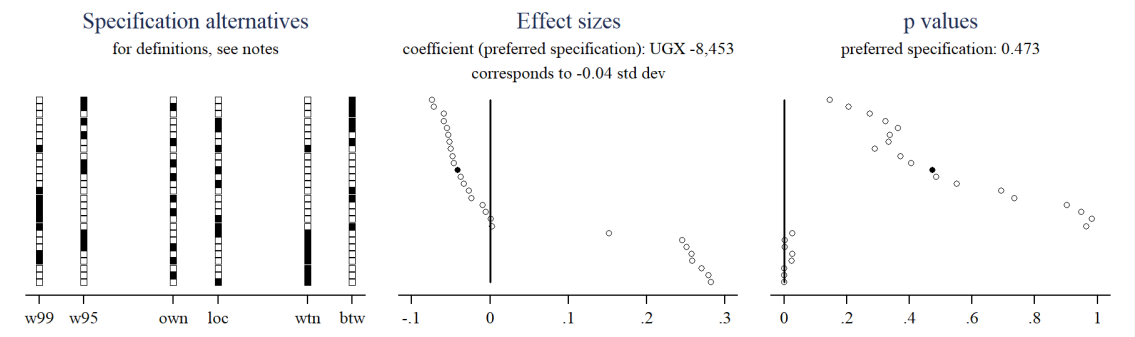

The notes from Figure 6 apply.

**Figure 22: Impact of Microenterprise Programs on Productive Cash Inflows**



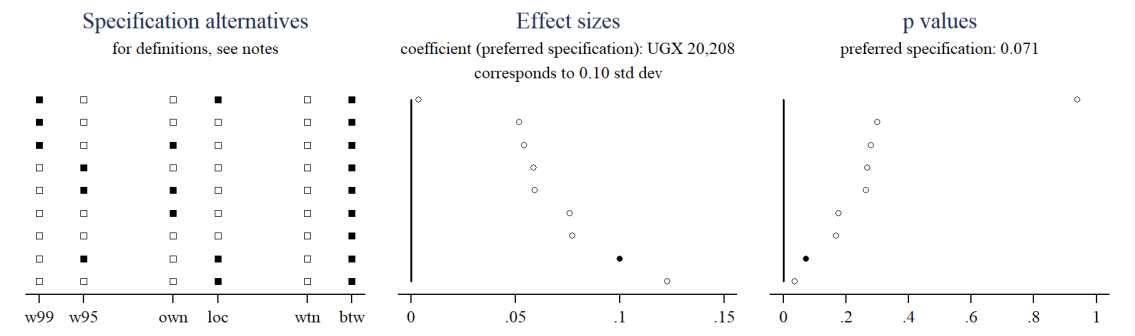

The notes from Figure 6 apply.

**Figure 23: Impact of Transfer Programs on Productive Cash Inflows**



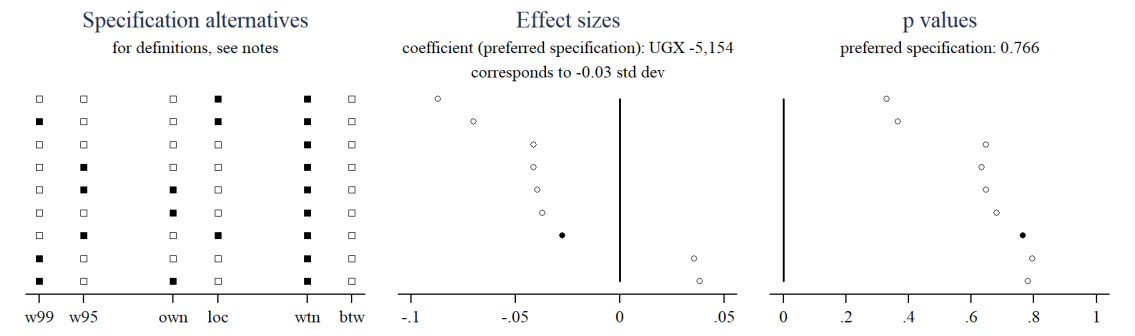

The notes from Figure 6 apply.

**Figure 24: Impact of Savings Component on Productive Cash Inflows**



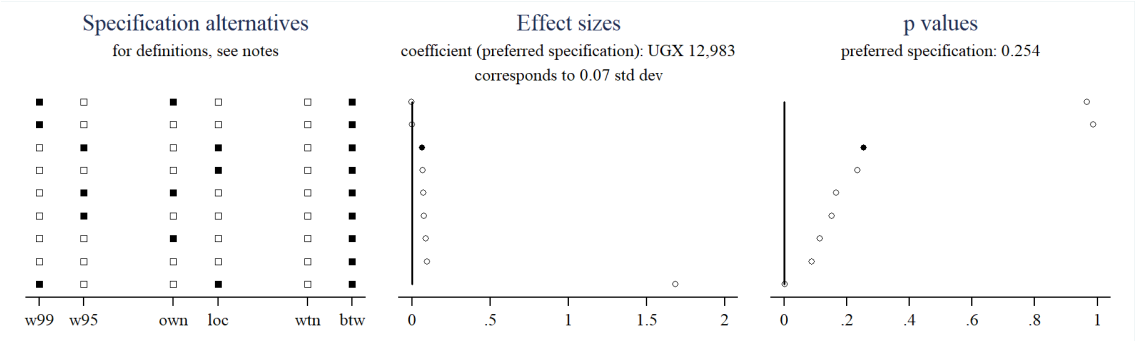

The notes from Figure 6 apply.

**Figure 25: Impact of Behavioral Component on Productive Cash Inflows**




The notes from Figure 6 apply.

**Figure 26: Impact of Microenterprise over Transfer Programs on Productive Cash Inflows**




The notes from Figure 6 apply.

## *APPENDIX IV: TABLES*

Like the previous section, this is intended as an online appendix to complement the journal article.

**Table 12: Impact of Spillovers on Poverty Indicators**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Net Assets | | | Total Productive Cash Inflows | | |
| Coefficient | -4,388 | -33,799 | -16,462 | -3,940 | -4,396 | -3,640 | -4,849 | -14,225 | -8,069 |
| Error | 22,784 | 19,963 | 18,915 | 6,923 | 7,608 | 6,789 | 10,718 | 11,752 | 9,273 |
| Effect/mean | -0.01 | -0.05 | -0.02 | -0.03 | -0.04 | -0.03 | -0.03 | -0.10 | -0.06 |
| Effect/sd | -0.01 | -0.08 | -0.04 | -0.03 | -0.03 | -0.03 | -0.02 | -0.06 | -0.04 |
| p value | 0.848 | 0.093 * | 0.386 | 0.570 | 0.564 | 0.593 | 0.652 | 0.228 | 0.386 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 3,004 | 2,941 | 3,094 | 3,004 | 2,274 | 3,004 | 3,090 | 2,396 | 2,529 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | 12,439 | -14,277 | 1,169 | -2,306 | -2,505 | -2,671 | -5,245 | -5,023 | -5,638 |
| Error | 17,296 | 14,327 | 13,429 | 3,955 | 4,219 | 3,846 | 4,636 | 3,582 | 3,508 |
| Effect/mean | 0.02 | -0.03 | 0.00 | -0.04 | -0.05 | -0.05 | -0.37 | -0.23 | -0.31 |
| Effect/sd | 0.03 | -0.04 | 0.00 | -0.02 | -0.03 | -0.03 | -0.06 | -0.06 | -0.09 |
| p value | 0.473 | 0.321 | 0.931 | 0.561 | 0.554 | 0.489 | 0.260 | 0.163 | 0.110 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 3,004 | 2,941 | 3,094 | 3,004 | 2,941 | 3,094 | 2,461 | 2,941 | 2,529 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | -2,803 | -7,794 | -5,188 | 1,947 | 106 | 917 | 577 | -11,197 | -5,648 |
| Error | 3,677 | 4,079 | 3,420 | 2,732 | 4,280 | 3,007 | 5,849 | 6,271 | 5,235 |
| Effect/mean | -0.03 | -0.12 | -0.07 | 0.04 | 0.00 | 0.02 | 0.01 | -0.17 | -0.09 |
| Effect/sd | -0.03 | -0.09 | -0.07 | 0.03 | 0.00 | 0.01 | 0.00 | -0.08 | -0.05 |
| p value | 0.447 | 0.058 * | 0.132 | 0.477 | 0.980 | 0.761 | 0.922 | 0.076 * | 0.283 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 3,097 | 3,028 | 3,188 | 2,461 | 2,328 | 2,461 | 3,090 | 3,021 | 3,181 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | -5,661 | -6,297 | -5,437 | 473 | 1,079 | 812 | -1,382 | 3,327 | 800 |
| Error | 4,569 | 4,263 | 4,045 | 646 | 1,071 | 687 | 3,701 | 5,243 | 3,529 |
| Effect/mean | -0.07 | -0.09 | -0.07 | 1.00 | 2.57 | 1.81 | -0.04 | 0.09 | 0.02 |
| Effect/sd | -0.05 | -0.07 | -0.06 | 0.03 | 0.05 | 0.05 | -0.02 | 0.03 | 0.01 |
| p value | 0.217 | 0.142 | 0.181 | 0.466 | 0.315 | 0.240 | 0.709 | 0.527 | 0.821 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 2,403 | 2,342 | 2,471 | 3,090 | 2,396 | 2,529 | 3,004 | 2,396 | 3,094 |

Notes: Coefficients are reported in current Ugandan Shillings per capita. Flow variables (consumption and income) are yearly. Effect/mean is the coefficient divided by the average value of the counterfactual. Effect/sd is the coefficient divided by the standard error of the counterfactual. Estimates pertain to coefficient β in the preferred specification for so-called clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. Errors are adjusted for cluster robustness. Food and beverage consumption is measured using regional market prices, i.e., median prices by region (East or West) and survey wave (Baseline, First Follow-up, Second follow-up). Recurring and infrequent consumption items are valued using self-reported prices. Total consumption is the sum of the three consumption sub-composites. Livestock assets are valued using local median prices. Durable assets are valued using self-reported prices and include the capitalized expenditures on home improvements; no depreciation is applied. The net financial position corresponds to the outstanding value of loans minus the value of savings. Total assets are the sum of the three asset sub-composites. Net cash inflows from farming are the cash sales of perennial crops by the household, plus the cash sales of crops grown by the household and with business partners over the last short and long season preceding the surveys, plus the cash sales of animals and animal products (with all sales being valued at regional market prices), minus all cash expenditures on farming and livestock. Income from other self-employment and paid employment are derived directly from the survey in cash terms. Total productive cash inflows are the aggregate of thee cash inflow sub-composites. For more information, consult the survey forms, data, and code.

**Table 13: Impact of Microenterprise Programs on Poverty Indicators**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Net Assets | | | Total Productive Cash Inflows | | |
| Coefficient | 27,526 | 18,859 | 26,061 | 20,189 | 10,570 | 16,343 | 20,447 | 8,889 | 13,483 |
| Error | 14,617 | 12,434 | 11,248 | 5,374 | 5,552 | 5,449 | 9,738 | 8,185 | 6,747 |
| Effect/mean | 0.04 | 0.03 | 0.04 | 0.17 | 0.09 | 0.14 | 0.15 | 0.07 | 0.10 |
| Effect/sd | 0.05 | 0.05 | 0.07 | 0.14 | 0.07 | 0.12 | 0.08 | 0.04 | 0.07 |
| p value | 0.062 * | 0.132 | 0.022 ** | 0.000 *** | 0.059 * | 0.003 *** | 0.038 ** | 0.279 | 0.048 ** |
| q value | 0.094 * | 0.142 | 0.055 * | 0.003 *** | 0.094 * | 0.021 ** | 0.076 * | 0.218 | 0.087 * |
| N | 4,750 | 4,655 | 4,906 | 4,750 | 3,598 | 3,796 | 3,901 | 3,815 | 4,021 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | 28,334 | 15,898 | 25,180 | 13,134 | 8,182 | 10,584 | -2,812 | 4,359 | 1,409 |
| Error | 12,875 | 10,088 | 9,381 | 3,092 | 2,954 | 2,657 | 2,934 | 2,596 | 2,161 |
| Effect/mean | 0.05 | 0.03 | 0.05 | 0.22 | 0.16 | 0.19 | -0.26 | 0.23 | 0.10 |
| Effect/sd | 0.07 | 0.05 | 0.08 | 0.14 | 0.10 | 0.14 | -0.03 | 0.06 | 0.02 |
| p value | 0.029 ** | 0.117 | 0.008 *** | 0.000 *** | 0.006 *** | 0.000 *** | 0.340 | 0.095 * | 0.515 |
| q value | 0.069 * | 0.133 | 0.029 ** | 0.002 *** | 0.029 ** | 0.002 *** | 0.252 | 0.118 | 0.304 |
| N | 4,750 | 4,655 | 4,906 | 4,750 | 3,718 | 4,906 | 3,901 | 4,801 | 4,021 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | -1,690 | -1,411 | -1,402 | 6,531 | 1,996 | 4,440 | 17,784 | 6,700 | 11,862 |
| Error | 2,056 | 2,377 | 1,917 | 2,510 | 2,936 | 2,452 | 5,477 | 5,381 | 4,361 |
| Effect/mean | -0.02 | -0.02 | -0.02 | 0.12 | 0.03 | 0.08 | 0.28 | 0.12 | 0.20 |
| Effect/sd | -0.02 | -0.02 | -0.02 | 0.09 | 0.02 | 0.06 | 0.13 | 0.05 | 0.11 |
| p value | 0.413 | 0.554 | 0.466 | 0.010 ** | 0.498 | 0.072 * | 0.001 *** | 0.215 | 0.007 *** |
| q value | 0.275 | 0.308 | 0.296 | 0.033 ** | 0.303 | 0.099 * | 0.013 ** | 0.190 | 0.029 ** |
| N | 4,916 | 4,811 | 5,073 | 3,901 | 3,695 | 3,901 | 3,796 | 4,655 | 3,916 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | 1,393 | 4,638 | 2,839 | 506 | 1,905 | 1,238 | -630 | -2,217 | -1,088 |
| Error | 3,260 | 2,443 | 2,605 | 701 | 707 | 572 | 2,416 | 3,622 | 2,681 |
| Effect/mean | 0.02 | 0.07 | 0.04 | 0.57 | 1.46 | 1.11 | -0.02 | -0.06 | -0.03 |
| Effect/sd | 0.01 | 0.06 | 0.04 | 0.03 | 0.10 | 0.09 | -0.01 | -0.02 | -0.01 |
| p value | 0.670 | 0.060 * | 0.278 | 0.472 | 0.008 *** | 0.032 ** | 0.795 | 0.542 | 0.686 |
| q value | 0.378 | 0.094 * | 0.218 | 0.296 | 0.029 ** | 0.070 * | 0.448 | 0.308 | 0.378 |
| N | 3,796 | 3,718 | 3,916 | 3,901 | 3,815 | 4,021 | 4,750 | 3,718 | 3,916 |

The notes from Table 12 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 14: Impact of Transfer Programs on Poverty Indicators**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Net Assets | | | Total Productive Cash Inflows | | |
| Coefficient | -44,555 | 8,895 | -17,141 | 19,336 | 15,720 | 15,852 | -36,874 | 27,468 | -8,453 |
| Error | 22,843 | 21,614 | 19,679 | 8,804 | 9,795 | 8,397 | 13,713 | 14,771 | 11,740 |
| Effect/mean | -0.06 | 0.01 | -0.03 | 0.16 | 0.13 | 0.13 | -0.27 | 0.21 | -0.06 |
| Effect/sd | -0.09 | 0.02 | -0.04 | 0.13 | 0.10 | 0.12 | -0.14 | 0.12 | -0.04 |
| p value | 0.053 * | 0.681 | 0.385 | 0.030 ** | 0.111 | 0.061 * | 0.008 *** | 0.065 * | 0.473 |
| q value | 0.125 | 0.646 | 0.580 | 0.079 * | 0.202 | 0.132 | 0.045 ** | 0.132 | 0.599 |
| N | 3,446 | 3,372 | 3,545 | 3,446 | 2,625 | 2,773 | 2,840 | 2,764 | 2,916 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | -27,064 | 7,295 | -10,261 | 15,695 | 13,343 | 15,155 | -3,878 | 1,302 | -2,612 |
| Error | 18,574 | 17,417 | 15,479 | 5,422 | 5,484 | 4,728 | 6,865 | 4,654 | 5,260 |
| Effect/mean | -0.05 | 0.01 | -0.02 | 0.26 | 0.26 | 0.28 | -0.36 | 0.07 | -0.18 |
| Effect/sd | -0.07 | 0.02 | -0.03 | 0.17 | 0.17 | 0.20 | -0.05 | 0.02 | -0.04 |
| p value | 0.147 | 0.676 | 0.508 | 0.004 *** | 0.016 ** | 0.002 *** | 0.573 | 0.780 | 0.620 |
| q value | 0.255 | 0.646 | 0.599 | 0.043 ** | 0.056 * | 0.032 ** | 0.637 | 0.713 | 0.646 |
| N | 3,446 | 3,372 | 3,545 | 3,446 | 2,701 | 3,545 | 2,840 | 3,473 | 2,916 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | -10,963 | -4,627 | -7,690 | 1,352 | 3,223 | 2,172 | -8,461 | 19,043 | 6,417 |
| Error | 3,320 | 3,407 | 2,819 | 4,260 | 5,224 | 4,416 | 8,605 | 7,713 | 6,881 |
| Effect/mean | -0.14 | -0.07 | -0.11 | 0.03 | 0.06 | 0.04 | -0.13 | 0.34 | 0.11 |
| Effect/sd | -0.12 | -0.06 | -0.11 | 0.02 | 0.04 | 0.03 | -0.06 | 0.14 | 0.06 |
| p value | 0.001 *** | 0.177 | 0.007 *** | 0.752 | 0.538 | 0.624 | 0.327 | 0.015 ** | 0.353 |
| q value | 0.032 ** | 0.297 | 0.045 ** | 0.713 | 0.610 | 0.646 | 0.572 | 0.056 * | 0.575 |
| N | 3,560 | 3,481 | 3,661 | 2,840 | 2,688 | 2,840 | 2,773 | 3,372 | 2,849 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | -3,401 | 4,759 | 1,171 | 2,887 | 3,446 | 3,041 | 1,916 | -4,182 | -3,472 |
| Error | 4,683 | 5,234 | 4,352 | 1,106 | 1,475 | 1,059 | 4,634 | 6,167 | 4,784 |
| Effect/mean | -0.04 | 0.07 | 0.02 | 3.25 | 2.63 | 2.74 | 0.05 | -0.11 | -0.09 |
| Effect/sd | -0.03 | 0.06 | 0.01 | 0.19 | 0.19 | 0.22 | 0.02 | -0.04 | -0.05 |
| p value | 0.469 | 0.365 | 0.788 | 0.010 ** | 0.021 ** | 0.005 *** | 0.680 | 0.499 | 0.469 |
| q value | 0.599 | 0.575 | 0.713 | 0.049 ** | 0.065 * | 0.043 ** | 0.646 | 0.599 | 0.599 |
| N | 2,773 | 2,701 | 2,849 | 2,840 | 2,764 | 2,916 | 3,446 | 2,701 | 2,849 |

The notes from Table 12 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 15: Impact of Savings Component on Poverty Indicators**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Net Assets | | | Total Productive Cash Inflows | | |
| Coefficient | 8,166 | 16,343 | 8,833 | -8,435 | 4,363 | -5,917 | 48,959 | -4,986 | 20,208 |
| Error | 28,971 | 22,433 | 21,944 | 11,109 | 8,289 | 9,048 | 15,150 | 13,747 | 11,007 |
| Effect/mean | 0.01 | 0.03 | 0.01 | -0.06 | 0.03 | -0.04 | 0.40 | -0.04 | 0.15 |
| Effect/sd | 0.02 | 0.04 | 0.02 | -0.04 | 0.03 | -0.04 | 0.19 | -0.02 | 0.10 |
| p value | 0.779 | 0.469 | 0.689 | 0.451 | 0.601 | 0.516 | 0.002 *** | 0.718 | 0.071 * |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 0.079 * | 1.000 | 0.506 |
| N | 1,746 | 1,714 | 1,812 | 1,393 | 1,648 | 1,746 | 1,746 | 1,783 | 1,885 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | 16,181 | 21,499 | 15,944 | -2,082 | -2,195 | -2,438 | 8,740 | -13,141 | -876 |
| Error | 25,909 | 17,803 | 18,221 | 5,895 | 4,514 | 4,900 | 4,836 | 6,233 | 4,188 |
| Effect/mean | 0.03 | 0.04 | 0.03 | -0.03 | -0.04 | -0.04 | 4.30 | -0.45 | -0.05 |
| Effect/sd | 0.04 | 0.07 | 0.05 | -0.02 | -0.02 | -0.02 | 0.09 | -0.15 | -0.01 |
| p value | 0.535 | 0.232 | 0.385 | 0.725 | 0.629 | 0.621 | 0.076 * | 0.039 ** | 0.835 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 0.506 | 0.365 | 1.000 |
| N | 1,746 | 1,714 | 1,812 | 1,746 | 1,714 | 1,812 | 1,746 | 1,419 | 1,882 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | 304 | -2,914 | -1,069 | -5,582 | -1,345 | -3,794 | 20,025 | 18,574 | 20,169 |
| Error | 3,575 | 3,939 | 3,282 | 4,405 | 4,173 | 3,830 | 8,905 | 8,745 | 6,792 |
| Effect/mean | 0.00 | -0.05 | -0.02 | -0.09 | -0.02 | -0.06 | 0.30 | 0.36 | 0.34 |
| Effect/sd | 0.00 | -0.04 | -0.02 | -0.06 | -0.02 | -0.05 | 0.13 | 0.15 | 0.19 |
| p value | 0.933 | 0.462 | 0.746 | 0.210 | 0.748 | 0.326 | 0.028 ** | 0.038 ** | 0.004 *** |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 0.365 | 0.365 | 0.082 * |
| N | 1,819 | 1,780 | 1,882 | 1,819 | 1,367 | 1,819 | 1,746 | 1,783 | 1,812 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | -754 | -5,082 | -4,145 | 590 | 1,402 | 996 | 1,915 | -6,743 | -2,198 |
| Error | 5,729 | 3,756 | 4,442 | 1,216 | 1,032 | 964 | 5,110 | 5,068 | 4,005 |
| Effect/mean | -0.01 | -0.07 | -0.06 | 0.52 | 0.70 | 0.60 | 0.06 | -0.18 | -0.06 |
| Effect/sd | -0.01 | -0.06 | -0.05 | 0.03 | 0.08 | 0.07 | 0.02 | -0.07 | -0.03 |
| p value | 0.896 | 0.181 | 0.355 | 0.629 | 0.180 | 0.306 | 0.709 | 0.188 | 0.585 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 1,393 | 1,714 | 1,445 | 1,746 | 1,783 | 1,812 | 1,393 | 1,714 | 1,812 |

The notes from Table 12 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 16: Impact of Behavioral Component on Poverty Indicators**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Net Assets | | | Total Productive Cash Inflows | | |
| Coefficient | -49,095 | -2,876 | -24,982 | 8,138 | 25,279 | 19,283 | -7,899 | 5,000 | -5,154 |
| Error | 33,085 | 37,211 | 29,279 | 12,660 | 13,863 | 11,479 | 19,562 | 24,477 | 17,309 |
| Effect/mean | -0.07 | 0.00 | -0.04 | 0.06 | 0.21 | 0.15 | -0.06 | 0.03 | -0.04 |
| Effect/sd | -0.11 | -0.01 | -0.07 | 0.05 | 0.19 | 0.15 | -0.04 | 0.02 | -0.03 |
| p value | 0.139 | 0.938 | 0.394 | 0.521 | 0.069 * | 0.094 * | 0.687 | 0.838 | 0.766 |
| q value | 0.478 | 1.000 | 1.000 | 1.000 | 0.373 | 0.451 | 1.000 | 1.000 | 1.000 |
| N | 462 | 431 | 451 | 462 | 442 | 462 | 442 | 431 | 472 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | -65,007 | -5,563 | -37,416 | 12,763 | 22,127 | 19,185 | 3,606 | 8,238 | 5,863 |
| Error | 28,038 | 30,610 | 23,907 | 8,286 | 9,177 | 7,790 | 9,689 | 8,481 | 7,007 |
| Effect/mean | -0.12 | -0.01 | -0.07 | 0.19 | 0.41 | 0.32 | 0.35 | 0.42 | 0.42 |
| Effect/sd | -0.17 | -0.02 | -0.13 | 0.14 | 0.32 | 0.26 | 0.03 | 0.12 | 0.08 |
| p value | 0.021 ** | 0.856 | 0.118 | 0.124 | 0.016 ** | 0.014 ** | 0.710 | 0.332 | 0.403 |
| q value | 0.335 | 1.000 | 0.463 | 0.463 | 0.335 | 0.335 | 1.000 | 0.992 | 1.000 |
| N | 462 | 431 | 451 | 442 | 431 | 451 | 442 | 452 | 451 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | 1,779 | -7,125 | -2,127 | -779 | 938 | 163 | -9,776 | 9,283 | -7,411 |
| Error | 5,647 | 5,587 | 4,574 | 5,359 | 7,838 | 5,684 | 11,616 | 16,981 | 10,889 |
| Effect/mean | 0.03 | -0.11 | -0.03 | -0.01 | 0.02 | 0.00 | -0.16 | 0.11 | -0.10 |
| Effect/sd | 0.03 | -0.11 | -0.04 | -0.01 | 0.01 | 0.00 | -0.07 | 0.06 | -0.06 |
| p value | 0.753 | 0.203 | 0.642 | 0.884 | 0.905 | 0.977 | 0.401 | 0.585 | 0.496 |
| q value | 1.000 | 0.624 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 463 | 453 | 473 | 462 | 442 | 462 | 442 | 431 | 472 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | 16,300 | 9,626 | 13,121 | -743 | 3,063 | 1,101 | -14,317 | -3,668 | -11,333 |
| Error | 8,114 | 7,724 | 6,407 | 1,624 | 2,344 | 1,661 | 7,720 | 7,299 | 5,581 |
| Effect/mean | 0.23 | 0.14 | 0.19 | -0.19 | 1.12 | 0.34 | -0.31 | -0.11 | -0.28 |
| Effect/sd | 0.23 | 0.12 | 0.20 | -0.03 | 0.11 | 0.05 | -0.14 | -0.04 | -0.15 |
| p value | 0.045 ** | 0.213 | 0.041 ** | 0.648 | 0.192 | 0.508 | 0.064 * | 0.616 | 0.043 ** |
| q value | 0.335 | 0.624 | 0.335 | 1.000 | 0.624 | 1.000 | 0.373 | 1.000 | 0.335 |
| N | 442 | 452 | 473 | 462 | 453 | 473 | 462 | 452 | 472 |

The notes from Table 12 apply, except as follows: Estimates pertain to coefficient β in the preferred specification for so-called non-clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 17: Impact of Microenterprise over Transfer Programs on Poverty Indicators**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Net Assets | | | Total Productive Cash Inflows | | |
| Coefficient | 75,542 | 11,366 | 46,294 | 4,143 | -7,577 | -831 | 55,630 | -15,903 | 12,983 |
| Error | 26,608 | 23,882 | 22,429 | 9,802 | 10,903 | 9,627 | 15,616 | 13,475 | 11,309 |
| Effect/mean | 0.11 | 0.02 | 0.07 | 0.03 | -0.05 | -0.01 | 0.45 | -0.10 | 0.09 |
| Effect/sd | 0.18 | 0.03 | 0.13 | 0.03 | -0.05 | -0.01 | 0.26 | -0.06 | 0.07 |
| p value | 0.006 *** | 0.635 | 0.042 ** | 0.673 | 0.489 | 0.931 | 0.001 *** | 0.241 | 0.254 |
| q value | 0.087 * | 1.000 | 0.193 | 1.000 | 1.000 | 1.000 | 0.022 ** | 1.000 | 1.000 |
| N | 2,188 | 2,145 | 2,263 | 1,763 | 1,727 | 1,819 | 1,819 | 2,232 | 2,354 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | 61,321 | 11,378 | 38,623 | -970 | -5,046 | -3,504 | -3,897 | 3,449 | 1,224 |
| Error | 22,916 | 19,374 | 18,194 | 5,729 | 6,121 | 5,383 | 7,632 | 5,253 | 5,555 |
| Effect/mean | 0.12 | 0.02 | 0.07 | -0.01 | -0.08 | -0.05 | -0.34 | 0.16 | 0.08 |
| Effect/sd | 0.18 | 0.03 | 0.13 | -0.01 | -0.06 | -0.04 | -0.04 | 0.04 | 0.02 |
| p value | 0.009 *** | 0.558 | 0.036 ** | 0.866 | 0.412 | 0.517 | 0.611 | 0.513 | 0.826 |
| q value | 0.087 * | 1.000 | 0.193 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 2,188 | 2,145 | 2,263 | 1,763 | 1,735 | 1,823 | 2,282 | 2,236 | 2,358 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | 9,392 | 3,094 | 6,039 | 2,944 | -1,732 | 1,864 | 24,763 | -11,150 | 6,927 |
| Error | 3,526 | 3,642 | 2,924 | 4,387 | 5,349 | 4,664 | 9,774 | 8,450 | 6,897 |
| Effect/mean | 0.13 | 0.05 | 0.09 | 0.05 | -0.03 | 0.03 | 0.43 | -0.14 | 0.10 |
| Effect/sd | 0.15 | 0.05 | 0.11 | 0.04 | -0.02 | 0.03 | 0.20 | -0.07 | 0.06 |
| p value | 0.009 *** | 0.398 | 0.042 ** | 0.504 | 0.747 | 0.690 | 0.013 ** | 0.190 | 0.318 |
| q value | 0.087 * | 1.000 | 0.193 | 1.000 | 1.000 | 1.000 | 0.091 * | 0.948 | 1.000 |
| N | 2,282 | 2,236 | 2,358 | 2,282 | 1,727 | 1,819 | 1,763 | 2,236 | 1,879 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | 3,510 | -227 | 442 | -1,495 | -1,409 | -1,749 | -1,788 | 841 | 628 |
| Error | 5,010 | 5,274 | 4,546 | 1,154 | 1,538 | 1,087 | 5,114 | 5,629 | 4,445 |
| Effect/mean | 0.04 | 0.00 | 0.01 | -0.49 | -0.33 | -0.49 | -0.05 | 0.03 | 0.02 |
| Effect/sd | 0.04 | 0.00 | 0.01 | -0.09 | -0.06 | -0.10 | -0.02 | 0.01 | 0.01 |
| p value | 0.485 | 0.966 | 0.923 | 0.198 | 0.362 | 0.111 | 0.727 | 0.882 | 0.888 |
| q value | 1.000 | 1.000 | 1.000 | 0.948 | 1.000 | 0.527 | 1.000 | 1.000 | 1.000 |
| N | 1,763 | 1,735 | 1,823 | 2,282 | 1,787 | 1,879 | 2,282 | 2,232 | 2,354 |

The notes from Table 12 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set D2∪D3.

**Table 18: Intent-to-Treat Estimates, Economic Aggregates (Log Dependent)**

| Comparison set | Impact of Spillovers [a] | Impact of Microenterprise Programs [b][iii] | Impact of Transfer Programs [c][iii] | Impact of Savings Component [d] | Impact of Behavioral Component [e] | Impact of Microenterprise over Transfer Programs [f] |
|---|---|---|---|---|---|---|
| **Total Consumption** | | | | | | |
| Coefficient | -0.030 | 0.039 | -0.013 | 0.018 | -0.052 | 0.057 |
| Error | 0.027 | 0.016 | 0.029 | 0.030 | 0.042 | 0.033 |
| p value | 0.279 | 0.017 ** | 0.669 | 0.555 | 0.215 | 0.090 * |
| q value | 0.364 | 0.024 ** | 0.684 | 0.760 | 0.673 | 0.193 |
| N | 3,094 | 4,906 | 3,545 | 1,812 | 451 | 2,263 |
| **Total Assets** | | | | | | |
| Coefficient | 0.096 | 0.213 | 0.252 | 0.067 | 0.135 | -0.054 |
| Error | 0.073 | 0.048 | 0.066 | 0.078 | 0.101 | 0.081 |
| p value | 0.193 | 0.000 *** | 0.000 *** | 0.395 | 0.184 | 0.509 |
| q value | 0.287 | 0.001 *** | 0.001 *** | 0.654 | 0.581 | 0.578 |
| N | 3,004 | 3,796 | 2,773 | 1,746 | 462 | 1,819 |
| **Total Productive Cash Inflows** | | | | | | |
| Coefficient | -0.068 | 0.001 | -0.032 | 0.066 | -0.016 | 0.015 |
| Error | 0.036 | 0.029 | 0.057 | 0.043 | 0.081 | 0.051 |
| p value | 0.057 * | 0.979 | 0.568 | 0.129 | 0.845 | 0.764 |
| q value | 0.171 | 0.325 | 0.643 | 0.299 | 1.000 | 0.722 |
| N | 2,529 | 4,021 | 2,916 | 1,885 | 472 | 2,354 |

This equals Table 4, except that the dependent variable is defined as $ln(y_{ijF} + c_{y_F})$. Constant $c_{y_F}$ serves to assure that all observations can be defined in logarithmic terms. Where the value of the lower winsorization cutoff for a given outcome $y$ in a given outcome round $F$ is above zero, $c_{y_F}$ is defined as zero. Where the cutoff value is equal to or below zero, $c_{y_F}$ is defined as the absolute value of the cutoff, plus UGX 1.

**Table 19: Impact of Spillovers on Poverty Indicators (Log Dependent)**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Assets | | | Total Productive Cash Inflows | | |
| Coefficient | -0.020 | -0.049 | -0.030 | -0.199 | 0.283 | 0.096 | 0.186 | 0.115 | -0.068 |
| Error | 0.031 | 0.029 | 0.027 | 0.082 | 0.093 | 0.073 | 0.081 | 0.093 | 0.036 |
| p value | 0.531 | 0.093 * | 0.279 | 0.017 ** | 0.003 *** | 0.193 | 0.023 ** | 0.216 | 0.057 * |
| q value | 0.563 | 0.201 | 0.364 | 0.078 * | 0.020 ** | 0.287 | 0.094 * | 0.287 | 0.171 |
| N | 3,004 | 2,941 | 3,094 | 3,004 | 2,274 | 3,004 | 3,090 | 2,396 | 2,529 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | -0.016 | -0.034 | -0.017 | 0.083 | -0.155 | 0.050 | -0.191 | 0.241 | -0.037 |
| Error | 0.035 | 0.027 | 0.025 | 0.198 | 0.223 | 0.179 | 0.058 | 0.068 | 0.022 |
| p value | 0.652 | 0.210 | 0.501 | 0.675 | 0.487 | 0.782 | 0.001 *** | 0.001 *** | 0.089 * |
| q value | 0.586 | 0.287 | 0.545 | 0.587 | 0.545 | 0.587 | 0.020 ** | 0.020 ** | 0.201 |
| N | 3,004 | 2,941 | 3,094 | 3,004 | 2,941 | 3,094 | 2,461 | 2,941 | 2,529 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | -0.080 | -0.069 | -0.067 | 0.034 | 0.045 | 0.024 | 0.119 | -0.552 | -0.074 |
| Error | 0.041 | 0.053 | 0.043 | 0.050 | 0.061 | 0.050 | 0.327 | 0.262 | 0.258 |
| p value | 0.055 * | 0.194 | 0.119 | 0.493 | 0.463 | 0.633 | 0.717 | 0.037 ** | 0.773 |
| q value | 0.171 | 0.287 | 0.231 | 0.545 | 0.545 | 0.586 | 0.587 | 0.131 | 0.587 |
| N | 3,097 | 3,028 | 3,188 | 2,461 | 2,328 | 2,461 | 3,090 | 3,021 | 3,181 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | -0.116 | -0.123 | -0.086 | 0.270 | 0.245 | 0.086 | 0.023 | 0.192 | 0.085 |
| Error | 0.064 | 0.069 | 0.056 | 0.088 | 0.079 | 0.032 | 0.366 | 0.261 | 0.320 |
| p value | 0.074 * | 0.079 * | 0.128 | 0.003 *** | 0.002 *** | 0.007 *** | 0.950 | 0.463 | 0.792 |
| q value | 0.195 | 0.195 | 0.233 | 0.020 ** | 0.020 ** | 0.041 ** | 0.656 | 0.545 | 0.587 |
| N | 2,403 | 2,342 | 2,471 | 3,090 | 2,396 | 2,529 | 3,004 | 2,396 | 3,094 |

The notes from Table 12 apply, except that the dependent variable is defined as $ln(y_{ijF} + c_{y_F})$. Constant $c_{y_F}$ serves to assure that all observations can be defined in logarithmic terms. Where the value of the lower winsorization cutoff for a given outcome $y$ in a given outcome round $F$ is above zero, $c_{y_F}$ is defined as zero. Where the cutoff value is equal to or below zero, $c_{y_F}$ is defined as the absolute value of the cutoff, plus UGX 1.

**Table 20: Impact of Microenterprise Programs on Poverty Indicators (Log Dependent)**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Assets | | | Total Productive Cash Inflows | | |
| Coefficient | 0.042 | 0.036 | 0.039 | 0.007 | 0.356 | 0.213 | -0.263 | -0.152 | 0.001 |
| Error | 0.019 | 0.020 | 0.016 | 0.065 | 0.068 | 0.048 | 0.058 | 0.064 | 0.029 |
| p value | 0.024 ** | 0.066 * | 0.017 ** | 0.915 | 0.000 *** | 0.000 *** | 0.000 *** | 0.018 ** | 0.979 |
| q value | 0.029 ** | 0.048 ** | 0.024 ** | 0.308 | 0.001 *** | 0.001 *** | 0.001 *** | 0.024 ** | 0.325 |
| N | 4,750 | 4,655 | 4,906 | 4,750 | 3,598 | 3,796 | 3,901 | 3,815 | 4,021 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | 0.049 | 0.030 | 0.042 | 0.542 | 0.315 | 0.351 | -0.291 | -0.212 | -0.012 |
| Error | 0.022 | 0.021 | 0.018 | 0.127 | 0.130 | 0.104 | 0.048 | 0.054 | 0.015 |
| p value | 0.032 ** | 0.146 | 0.019 ** | 0.000 *** | 0.017 ** | 0.001 *** | 0.000 *** | 0.000 *** | 0.427 |
| q value | 0.035 ** | 0.089 * | 0.024 ** | 0.001 *** | 0.024 ** | 0.003 *** | 0.001 *** | 0.001 *** | 0.206 |
| N | 4,750 | 4,655 | 4,906 | 4,750 | 3,718 | 4,906 | 3,901 | 4,801 | 4,021 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | 0.024 | 0.012 | 0.018 | 0.132 | 0.112 | 0.118 | 0.468 | 0.271 | 0.452 |
| Error | 0.025 | 0.037 | 0.026 | 0.040 | 0.040 | 0.037 | 0.210 | 0.191 | 0.185 |
| p value | 0.343 | 0.744 | 0.494 | 0.001 *** | 0.006 *** | 0.002 *** | 0.027 ** | 0.157 | 0.016 ** |
| q value | 0.190 | 0.265 | 0.237 | 0.004 *** | 0.012 ** | 0.005 *** | 0.031 ** | 0.089 * | 0.024 ** |
| N | 4,916 | 4,811 | 5,073 | 3,901 | 3,695 | 3,901 | 3,796 | 4,655 | 3,916 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | 0.036 | 0.116 | 0.059 | -0.261 | 0.273 | 0.004 | -0.439 | -0.081 | -0.306 |
| Error | 0.044 | 0.042 | 0.036 | 0.058 | 0.050 | 0.013 | 0.180 | 0.193 | 0.214 |
| p value | 0.414 | 0.006 *** | 0.107 | 0.000 *** | 0.000 *** | 0.773 | 0.016 ** | 0.676 | 0.155 |
| q value | 0.206 | 0.012 ** | 0.067 * | 0.001 *** | 0.001 *** | 0.265 | 0.024 ** | 0.265 | 0.089 * |
| N | 3,796 | 3,718 | 3,916 | 3,901 | 3,815 | 4,021 | 4,750 | 3,718 | 3,916 |

The notes from Table 18 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 21: Impact of Transfer Programs on Poverty Indicators (Log Dependent)**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Assets | | | Total Productive Cash Inflows | | |
| Coefficient | -0.033 | 0.025 | -0.013 | 0.453 | 0.458 | 0.252 | -0.366 | 0.380 | -0.032 |
| Error | 0.036 | 0.028 | 0.029 | 0.090 | 0.084 | 0.066 | 0.109 | 0.072 | 0.057 |
| p value | 0.360 | 0.387 | 0.669 | 0.000 *** | 0.000 *** | 0.000 *** | 0.001 *** | 0.000 *** | 0.568 |
| q value | 0.460 | 0.483 | 0.684 | 0.001 *** | 0.001 *** | 0.001 *** | 0.003 *** | 0.001 *** | 0.643 |
| N | 3,446 | 3,372 | 3,545 | 3,446 | 2,625 | 2,773 | 2,840 | 2,764 | 2,916 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | -0.027 | 0.017 | -0.012 | 0.874 | 0.935 | 0.789 | -0.307 | -0.260 | -0.028 |
| Error | 0.041 | 0.031 | 0.031 | 0.206 | 0.229 | 0.159 | 0.107 | 0.104 | 0.029 |
| p value | 0.507 | 0.588 | 0.702 | 0.000 *** | 0.000 *** | 0.000 *** | 0.005 *** | 0.014 ** | 0.335 |
| q value | 0.639 | 0.643 | 0.684 | 0.001 *** | 0.001 *** | 0.001 *** | 0.010 *** | 0.027 ** | 0.442 |
| N | 3,446 | 3,372 | 3,545 | 3,446 | 2,701 | 3,545 | 2,840 | 3,473 | 2,916 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | -0.015 | 0.003 | -0.018 | 0.080 | 0.100 | 0.070 | -0.142 | 0.783 | 0.472 |
| Error | 0.043 | 0.050 | 0.039 | 0.065 | 0.064 | 0.061 | 0.405 | 0.324 | 0.354 |
| p value | 0.722 | 0.953 | 0.632 | 0.221 | 0.119 | 0.248 | 0.727 | 0.017 ** | 0.185 |
| q value | 0.684 | 0.890 | 0.668 | 0.371 | 0.213 | 0.402 | 0.684 | 0.030 ** | 0.315 |
| N | 3,560 | 3,481 | 3,661 | 2,840 | 2,688 | 2,840 | 2,773 | 3,372 | 2,849 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | 0.088 | 0.078 | 0.058 | 0.345 | 0.301 | 0.066 | 0.005 | 0.369 | 0.223 |
| Error | 0.064 | 0.070 | 0.057 | 0.056 | 0.056 | 0.017 | 0.365 | 0.369 | 0.413 |
| p value | 0.173 | 0.265 | 0.310 | 0.000 *** | 0.000 *** | 0.000 *** | 0.989 | 0.319 | 0.591 |
| q value | 0.312 | 0.402 | 0.438 | 0.001 *** | 0.001 *** | 0.001 *** | 0.890 | 0.438 | 0.643 |
| N | 2,773 | 2,701 | 2,849 | 2,840 | 2,764 | 2,916 | 3,446 | 2,701 | 2,849 |

The notes from Table 18 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 22: Impact of Savings Component on Poverty Indicators (Log Dependent)**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Assets | | | Total Productive Cash Inflows | | |
| Coefficient | 0.030 | 0.019 | 0.018 | 0.285 | 0.326 | 0.067 | 0.399 | -0.281 | 0.066 |
| Error | 0.036 | 0.036 | 0.030 | 0.129 | 0.087 | 0.078 | 0.078 | 0.089 | 0.043 |
| p value | 0.406 | 0.609 | 0.555 | 0.031 ** | 0.000 *** | 0.395 | 0.000 *** | 0.003 *** | 0.129 |
| q value | 0.654 | 0.789 | 0.760 | 0.124 | 0.003 *** | 0.654 | 0.001 *** | 0.014 ** | 0.299 |
| N | 1,746 | 1,714 | 1,812 | 1,393 | 1,648 | 1,746 | 1,746 | 1,783 | 1,885 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | 0.061 | 0.027 | 0.035 | 0.258 | 0.238 | 0.087 | 0.337 | -0.346 | 0.024 |
| Error | 0.043 | 0.037 | 0.033 | 0.256 | 0.276 | 0.204 | 0.072 | 0.085 | 0.029 |
| p value | 0.155 | 0.474 | 0.294 | 0.317 | 0.392 | 0.672 | 0.000 *** | 0.000 *** | 0.414 |
| q value | 0.348 | 0.703 | 0.592 | 0.592 | 0.654 | 0.889 | 0.001 *** | 0.002 *** | 0.654 |
| N | 1,746 | 1,714 | 1,812 | 1,746 | 1,714 | 1,812 | 1,746 | 1,419 | 1,882 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | 0.001 | -0.012 | -0.009 | 0.015 | -0.041 | -0.032 | 0.869 | 0.549 | 0.572 |
| Error | 0.044 | 0.062 | 0.044 | 0.073 | 0.064 | 0.061 | 0.325 | 0.295 | 0.297 |
| p value | 0.977 | 0.843 | 0.830 | 0.834 | 0.526 | 0.606 | 0.010 *** | 0.068 * | 0.059 * |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 0.740 | 0.789 | 0.044 ** | 0.201 | 0.191 |
| N | 1,819 | 1,780 | 1,882 | 1,819 | 1,367 | 1,819 | 1,746 | 1,783 | 1,812 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | -0.053 | -0.025 | -0.074 | 0.200 | 0.278 | 0.036 | 0.102 | -0.504 | -0.252 |
| Error | 0.076 | 0.071 | 0.060 | 0.092 | 0.046 | 0.022 | 0.353 | 0.290 | 0.350 |
| p value | 0.491 | 0.729 | 0.224 | 0.034 ** | 0.000 *** | 0.105 | 0.774 | 0.087 * | 0.475 |
| q value | 0.703 | 0.977 | 0.475 | 0.124 | 0.001 *** | 0.271 | 1.000 | 0.243 | 0.703 |
| N | 1,393 | 1,714 | 1,445 | 1,746 | 1,783 | 1,812 | 1,393 | 1,714 | 1,812 |

The notes from Table 18 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 23: Impact of Behavioral Component on Poverty Indicators (Log Dependent)**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Assets | | | Total Productive Cash Inflows | | |
| Coefficient | -0.054 | -0.040 | -0.052 | 0.044 | 0.329 | 0.135 | -0.330 | -0.360 | -0.016 |
| Error | 0.049 | 0.049 | 0.042 | 0.124 | 0.156 | 0.101 | 0.152 | 0.168 | 0.081 |
| p value | 0.275 | 0.417 | 0.215 | 0.721 | 0.036 ** | 0.184 | 0.031 ** | 0.033 ** | 0.845 |
| q value | 0.775 | 0.789 | 0.673 | 0.971 | 0.312 | 0.581 | 0.312 | 0.312 | 1.000 |
| N | 462 | 431 | 451 | 462 | 442 | 462 | 442 | 431 | 472 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | -0.077 | -0.034 | -0.067 | 0.431 | 0.097 | 0.092 | -0.236 | -0.278 | 0.070 |
| Error | 0.054 | 0.052 | 0.044 | 0.323 | 0.319 | 0.230 | 0.145 | 0.135 | 0.070 |
| p value | 0.157 | 0.516 | 0.128 | 0.182 | 0.763 | 0.689 | 0.105 | 0.040 ** | 0.315 |
| q value | 0.581 | 0.933 | 0.581 | 0.581 | 0.971 | 0.971 | 0.539 | 0.312 | 0.789 |
| N | 462 | 431 | 451 | 442 | 431 | 451 | 442 | 452 | 451 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | -0.027 | -0.145 | -0.072 | -0.031 | 0.023 | 0.006 | -0.419 | -0.227 | -0.174 |
| Error | 0.088 | 0.085 | 0.070 | 0.082 | 0.090 | 0.078 | 0.515 | 0.546 | 0.509 |
| p value | 0.764 | 0.089 * | 0.301 | 0.705 | 0.801 | 0.938 | 0.416 | 0.677 | 0.732 |
| q value | 0.971 | 0.538 | 0.789 | 0.971 | 1.000 | 1.000 | 0.789 | 0.971 | 0.971 |
| N | 463 | 453 | 473 | 462 | 442 | 462 | 442 | 431 | 472 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | 0.144 | 0.002 | 0.125 | 0.285 | 0.333 | 0.082 | -0.202 | 0.057 | -0.291 |
| Error | 0.106 | 0.112 | 0.088 | 0.118 | 0.123 | 0.049 | 0.469 | 0.461 | 0.472 |
| p value | 0.174 | 0.985 | 0.157 | 0.016 ** | 0.007 *** | 0.093 * | 0.667 | 0.902 | 0.538 |
| q value | 0.581 | 1.000 | 0.581 | 0.312 | 0.312 | 0.538 | 0.971 | 1.000 | 0.933 |
| N | 442 | 452 | 473 | 462 | 453 | 473 | 462 | 452 | 472 |

The notes from Table 18 apply, except as follows: Estimates pertain to coefficient β in the preferred specification for so-called non-clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 24: Impact of Microenterprise over Transfer Programs on Poverty Indicators (Log Dependent)**

| Follow-up Round | First | Second | Pooled | First | Second | Pooled | First | Second | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | Total Consumption | | | Total Assets | | | Total Productive Cash Inflows | | |
| Coefficient | 0.081 | 0.015 | 0.057 | -0.287 | -0.268 | -0.054 | -0.160 | -0.334 | 0.015 |
| Error | 0.041 | 0.034 | 0.033 | 0.106 | 0.108 | 0.081 | 0.071 | 0.074 | 0.051 |
| p value | 0.049 ** | 0.659 | 0.090 * | 0.008 *** | 0.015 ** | 0.509 | 0.026 ** | 0.000 *** | 0.764 |
| q value | 0.132 | 0.682 | 0.193 | 0.053 * | 0.064 * | 0.578 | 0.076 * | 0.001 *** | 0.722 |
| N | 2,188 | 2,145 | 2,263 | 1,763 | 1,727 | 1,819 | 1,819 | 2,232 | 2,354 |
| | Food and Beverage Consumption | | | Livestock Assets | | | Net Cash Inflows from Farming | | |
| Coefficient | 0.088 | 0.021 | 0.060 | -0.223 | -0.651 | -0.357 | -0.273 | 0.250 | 0.010 |
| Error | 0.047 | 0.037 | 0.036 | 0.259 | 0.261 | 0.197 | 0.059 | 0.103 | 0.026 |
| p value | 0.065 * | 0.567 | 0.098 * | 0.392 | 0.014 ** | 0.073 * | 0.000 *** | 0.017 ** | 0.692 |
| q value | 0.164 | 0.616 | 0.196 | 0.479 | 0.064 * | 0.171 | 0.001 *** | 0.064 * | 0.682 |
| N | 2,188 | 2,145 | 2,263 | 1,763 | 1,735 | 1,823 | 2,282 | 2,236 | 2,358 |
| | Recurring Consumption | | | Durable Assets | | | Income from Other Self-Employment | | |
| Coefficient | 0.043 | 0.009 | 0.035 | 0.001 | 0.002 | 0.038 | 0.582 | -0.579 | -0.040 |
| Error | 0.047 | 0.057 | 0.042 | 0.070 | 0.071 | 0.070 | 0.427 | 0.367 | 0.360 |
| p value | 0.358 | 0.872 | 0.401 | 0.986 | 0.981 | 0.593 | 0.176 | 0.119 | 0.913 |
| q value | 0.479 | 0.816 | 0.479 | 0.871 | 0.871 | 0.616 | 0.291 | 0.228 | 0.840 |
| N | 2,282 | 2,236 | 2,358 | 2,282 | 1,727 | 1,819 | 1,763 | 2,236 | 1,879 |
| | Infrequent Consumption | | | Net Financial Position | | | Income from Paid Employment | | |
| Coefficient | -0.064 | 0.041 | -0.016 | -0.305 | -0.230 | -0.043 | -0.447 | -0.440 | -0.597 |
| Error | 0.071 | 0.073 | 0.062 | 0.058 | 0.057 | 0.018 | 0.405 | 0.352 | 0.414 |
| p value | 0.372 | 0.577 | 0.801 | 0.000 *** | 0.000 *** | 0.016 ** | 0.272 | 0.214 | 0.153 |
| q value | 0.479 | 0.616 | 0.741 | 0.001 *** | 0.001 *** | 0.064 * | 0.373 | 0.310 | 0.261 |
| N | 1,763 | 1,735 | 1,823 | 2,282 | 1,787 | 1,879 | 2,282 | 2,232 | 2,354 |

The notes from Table 18 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set D2∪D3.

**Table 25: Impact of Spillovers on Psychological Outlook**

| Follow-up Round | | First | | Second | | Pooled | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Well-being | Coefficient | 0.043 | -0.041 | -0.019 | -0.089 | 0.019 | -0.093 |
| | Error | 0.056 | 0.060 | 0.060 | 0.066 | 0.065 | 0.066 |
| | p value | 0.440 | 0.498 | 0.755 | 0.182 | 0.768 | 0.163 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,084 | 1,569 | 3,024 | 1,915 | 3,186 | 2,012 |
| Aspirations | Coefficient | -0.084 | -0.088 | -0.047 | -0.049 | -0.082 | -0.088 |
| | Error | 0.047 | 0.054 | 0.056 | 0.057 | 0.051 | 0.056 |
| | p value | 0.075 * | 0.104 | 0.402 | 0.389 | 0.108 | 0.116 |
| | q value | 0.683 | 0.869 | 1.000 | 1.000 | 0.880 | 0.880 |
| | N | 3,024 | 2,407 | 2,855 | 2,279 | 3,173 | 2,518 |
| Expectations | Coefficient | -0.110 | -0.112 | -0.081 | -0.072 | -0.116 | -0.118 |
| | Error | 0.042 | 0.044 | 0.047 | 0.051 | 0.047 | 0.049 |
| | p value | 0.010 ** | 0.012 ** | 0.088 * | 0.157 | 0.015 ** | 0.018 ** |
| | q value | 0.193 | 0.210 | 0.795 | 1.000 | 0.251 | 0.264 |
| | N | 2,997 | 2,388 | 2,807 | 2,237 | 3,167 | 2,513 |
| Self-control | Coefficient | -0.047 | -0.074 | -0.031 | -0.006 | -0.040 | -0.047 |
| | Error | 0.081 | 0.071 | 0.086 | 0.087 | 0.094 | 0.075 |
| | p value | 0.560 | 0.305 | 0.715 | 0.949 | 0.670 | 0.534 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,096 | 1,596 | 3,028 | 1,569 | 3,188 | 1,638 |
| Sense of Control | Coefficient | -0.068 | -0.049 | 0.042 | 0.041 | -0.020 | -0.017 |
| | Error | 0.049 | 0.053 | 0.055 | 0.050 | 0.053 | 0.050 |
| | p value | 0.164 | 0.362 | 0.441 | 0.409 | 0.710 | 0.734 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,097 | 2,461 | 3,028 | 3,021 | 3,188 | 3,181 |
| Sense of Status | Coefficient | -0.049 | -0.057 | -0.022 | -0.066 | -0.053 | -0.103 |
| | Error | 0.046 | 0.043 | 0.052 | 0.052 | 0.050 | 0.049 |
| | p value | 0.287 | 0.186 | 0.675 | 0.201 | 0.293 | 0.038 ** |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 0.459 |
| | N | 3,000 | 2,962 | 3,028 | 2,373 | 3,182 | 2,500 |
| Sense of Pride | Coefficient | -0.004 | -0.001 | 0.006 | 0.009 | 0.006 | 0.007 |
| | Error | 0.077 | 0.050 | 0.103 | 0.066 | 0.104 | 0.059 |
| | p value | 0.960 | 0.992 | 0.956 | 0.895 | 0.957 | 0.909 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,097 | 3,097 | 3,028 | 3,028 | 3,188 | 3,188 |
| Composite Index | Coefficient | -0.091 | -0.129 | -0.042 | -0.034 | -0.079 | -0.126 |
| | Error | 0.070 | 0.068 | 0.084 | 0.065 | 0.083 | 0.053 |
| | p value | 0.195 | 0.060 * | 0.616 | 0.598 | 0.345 | 0.019 ** |
| | q value | 1.000 | 0.597 | 1.000 | 1.000 | 1.000 | 0.264 |
| | N | 2,904 | 1,472 | 2,788 | 1,400 | 3,162 | 1,584 |

Notes: Specification 1 uses simple linear regression. Specification 2 uses the preferred model for so-called clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. Errors are adjusted for cluster robustness. The sub-composites are standardized, and the composite is the standardized sum of the sub-composites.

**Table 26: Impact of Microenterprise Programs on Psychological Outlook**

| Follow-up Round | | First | | Second | | Pooled | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Well-being | Coefficient | 0.028 | 0.028 | 0.095 | 0.142 | 0.079 | 0.140 |
| | Error | 0.035 | 0.039 | 0.035 | 0.044 | 0.036 | 0.041 |
| | p value | 0.430 | 0.466 | 0.007 *** | 0.002 *** | 0.029 ** | 0.001 *** |
| | q value | 1.000 | 1.000 | 0.169 | 0.057 * | 0.347 | 0.040 ** |
| | N | 4,899 | 3,152 | 4,803 | 2,454 | 5,070 | 2,588 |
| Aspirations | Coefficient | 0.031 | 0.037 | 0.004 | 0.012 | 0.014 | 0.021 |
| | Error | 0.031 | 0.035 | 0.029 | 0.033 | 0.030 | 0.033 |
| | p value | 0.314 | 0.300 | 0.901 | 0.724 | 0.626 | 0.520 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,809 | 3,825 | 4,550 | 3,640 | 5,048 | 4,004 |
| Expectations | Coefficient | 0.050 | 0.086 | 0.048 | 0.044 | 0.052 | 0.072 |
| | Error | 0.035 | 0.037 | 0.034 | 0.037 | 0.034 | 0.036 |
| | p value | 0.152 | 0.021 ** | 0.155 | 0.245 | 0.131 | 0.050 ** |
| | q value | 1.000 | 0.264 | 1.000 | 1.000 | 0.909 | 0.532 |
| | N | 4,771 | 3,799 | 4,478 | 3,580 | 5,041 | 3,998 |
| Self-control | Coefficient | 0.022 | 0.003 | -0.007 | 0.024 | 0.013 | 0.017 |
| | Error | 0.041 | 0.042 | 0.044 | 0.044 | 0.047 | 0.042 |
| | p value | 0.595 | 0.950 | 0.871 | 0.597 | 0.790 | 0.695 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,913 | 2,538 | 4,811 | 2,487 | 5,073 | 2,616 |
| Sense of Control | Coefficient | -0.007 | -0.019 | -0.034 | -0.031 | -0.023 | -0.030 |
| | Error | 0.032 | 0.035 | 0.031 | 0.029 | 0.031 | 0.035 |
| | p value | 0.840 | 0.576 | 0.266 | 0.294 | 0.454 | 0.390 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,916 | 3,901 | 4,811 | 4,801 | 5,073 | 4,021 |
| Sense of Status | Coefficient | 0.065 | 0.102 | 0.126 | 0.128 | 0.120 | 0.143 |
| | Error | 0.033 | 0.035 | 0.027 | 0.030 | 0.030 | 0.033 |
| | p value | 0.050 ** | 0.004 *** | 0.000 *** | 0.000 *** | 0.000 *** | 0.000 *** |
| | q value | 0.532 | 0.104 | 0.002 *** | 0.004 *** | 0.009 *** | 0.004 *** |
| | N | 4,761 | 3,749 | 4,811 | 3,777 | 5,061 | 3,973 |
| Sense of Pride | Coefficient | 0.004 | 0.008 | 0.025 | 0.037 | 0.023 | 0.029 |
| | Error | 0.039 | 0.030 | 0.047 | 0.034 | 0.047 | 0.031 |
| | p value | 0.920 | 0.790 | 0.598 | 0.280 | 0.624 | 0.349 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,916 | 4,916 | 4,811 | 4,811 | 5,073 | 5,073 |
| Composite Index | Coefficient | 0.064 | 0.106 | 0.080 | 0.129 | 0.078 | 0.143 |
| | Error | 0.042 | 0.045 | 0.041 | 0.044 | 0.043 | 0.042 |
| | p value | 0.133 | 0.021 ** | 0.055 * | 0.004 *** | 0.072 * | 0.001 *** |
| | q value | 0.909 | 0.264 | 0.559 | 0.111 | 0.683 | 0.040 ** |
| | N | 4,614 | 2,354 | 4,447 | 2,261 | 5,026 | 2,542 |

The notes from Table 25 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 27: Impact of Transfer Programs on Psychological Outlook**

| Follow-up Round | | First | | Second | | Pooled | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Well-being | Coefficient | 0.064 | 0.081 | 0.014 | 0.005 | 0.063 | 0.080 |
| | Error | 0.059 | 0.068 | 0.059 | 0.076 | 0.055 | 0.065 |
| | p value | 0.281 | 0.234 | 0.817 | 0.950 | 0.254 | 0.223 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,544 | 2,279 | 3,477 | 1,757 | 3,659 | 1,855 |
| Aspirations | Coefficient | 0.055 | 0.035 | 0.078 | 0.074 | 0.094 | 0.067 |
| | Error | 0.060 | 0.063 | 0.077 | 0.083 | 0.070 | 0.077 |
| | p value | 0.366 | 0.584 | 0.314 | 0.372 | 0.182 | 0.387 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,482 | 2,783 | 3,297 | 2,641 | 3,646 | 2,905 |
| Expectations | Coefficient | 0.103 | 0.106 | -0.040 | -0.078 | 0.047 | 0.018 |
| | Error | 0.069 | 0.070 | 0.050 | 0.057 | 0.061 | 0.062 |
| | p value | 0.134 | 0.133 | 0.432 | 0.168 | 0.443 | 0.769 |
| | q value | 0.909 | 0.909 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,449 | 2,761 | 3,247 | 2,596 | 3,640 | 2,900 |
| Self-control | Coefficient | 0.048 | -0.022 | -0.045 | -0.036 | 0.025 | -0.004 |
| | Error | 0.075 | 0.070 | 0.086 | 0.093 | 0.092 | 0.080 |
| | p value | 0.526 | 0.755 | 0.605 | 0.702 | 0.787 | 0.965 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,558 | 1,839 | 3,481 | 1,791 | 3,661 | 1,888 |
| Sense of Control | Coefficient | -0.031 | 0.011 | -0.090 | -0.084 | -0.077 | -0.033 |
| | Error | 0.050 | 0.054 | 0.054 | 0.050 | 0.051 | 0.052 |
| | p value | 0.539 | 0.832 | 0.097 * | 0.093 * | 0.137 | 0.526 |
| | q value | 1.000 | 1.000 | 0.842 | 0.819 | 0.911 | 1.000 |
| | N | 3,560 | 2,840 | 3,481 | 3,473 | 3,661 | 2,916 |
| Sense of Status | Coefficient | 0.046 | 0.064 | 0.038 | 0.023 | 0.046 | 0.051 |
| | Error | 0.054 | 0.048 | 0.066 | 0.070 | 0.062 | 0.056 |
| | p value | 0.392 | 0.186 | 0.567 | 0.744 | 0.459 | 0.359 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,449 | 2,730 | 3,481 | 2,740 | 3,655 | 2,886 |
| Sense of Pride | Coefficient | 0.081 | 0.068 | 0.002 | -0.006 | 0.071 | 0.052 |
| | Error | 0.066 | 0.050 | 0.088 | 0.061 | 0.088 | 0.057 |
| | p value | 0.222 | 0.171 | 0.979 | 0.921 | 0.424 | 0.358 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,560 | 3,560 | 3,481 | 3,481 | 3,661 | 3,661 |
| Composite Index | Coefficient | 0.128 | 0.174 | -0.014 | -0.030 | 0.074 | 0.107 |
| | Error | 0.078 | 0.066 | 0.085 | 0.077 | 0.091 | 0.067 |
| | p value | 0.104 | 0.010 *** | 0.866 | 0.697 | 0.414 | 0.117 |
| | q value | 0.869 | 0.193 | 1.000 | 1.000 | 1.000 | 0.880 |
| | N | 3,340 | 1,698 | 3,226 | 1,619 | 3,635 | 1,822 |

The notes from Table 25 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 28: Impact of Savings Component on Psychological Outlook**

| Follow-up Round | | First | | Second | | Pooled | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Well-being | Coefficient | 0.009 | -0.026 | -0.018 | 0.036 | -0.021 | -0.014 |
| | Error | 0.065 | 0.065 | 0.063 | 0.062 | 0.066 | 0.062 |
| | p value | 0.887 | 0.687 | 0.781 | 0.569 | 0.754 | 0.825 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,815 | 935 | 1,779 | 923 | 1,884 | 1,208 |
| Aspirations | Coefficient | -0.064 | -0.082 | -0.097 | -0.114 | -0.104 | -0.127 |
| | Error | 0.054 | 0.058 | 0.047 | 0.052 | 0.051 | 0.053 |
| | p value | 0.245 | 0.162 | 0.045 ** | 0.031 ** | 0.045 ** | 0.019 ** |
| | q value | 1.000 | 1.000 | 0.504 | 0.373 | 0.504 | 0.264 |
| | N | 1,785 | 1,418 | 1,695 | 1,361 | 1,875 | 1,486 |
| Expectations | Coefficient | 0.008 | 0.010 | -0.052 | -0.051 | -0.035 | -0.032 |
| | Error | 0.085 | 0.075 | 0.062 | 0.063 | 0.079 | 0.070 |
| | p value | 0.923 | 0.892 | 0.412 | 0.427 | 0.661 | 0.656 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,774 | 1,411 | 1,671 | 1,343 | 1,874 | 1,485 |
| Self-control | Coefficient | -0.032 | 0.022 | -0.008 | 0.009 | -0.026 | 0.011 |
| | Error | 0.091 | 0.077 | 0.110 | 0.070 | 0.117 | 0.068 |
| | p value | 0.724 | 0.775 | 0.939 | 0.903 | 0.822 | 0.869 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,817 | 1,195 | 1,783 | 1,172 | 1,885 | 1,240 |
| Sense of Control | Coefficient | 0.058 | -0.005 | -0.080 | -0.071 | -0.016 | -0.087 |
| | Error | 0.054 | 0.061 | 0.050 | 0.047 | 0.048 | 0.054 |
| | p value | 0.289 | 0.939 | 0.115 | 0.139 | 0.738 | 0.113 |
| | q value | 1.000 | 1.000 | 0.880 | 0.911 | 1.000 | 0.880 |
| | N | 1,819 | 1,393 | 1,783 | 1,714 | 1,885 | 1,445 |
| Sense of Status | Coefficient | -0.028 | -0.016 | -0.026 | -0.017 | -0.044 | -0.027 |
| | Error | 0.060 | 0.054 | 0.055 | 0.048 | 0.064 | 0.052 |
| | p value | 0.647 | 0.770 | 0.635 | 0.731 | 0.492 | 0.602 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,761 | 1,739 | 1,783 | 1,697 | 1,879 | 1,855 |
| Sense of Pride | Coefficient | 0.010 | 0.012 | 0.031 | 0.032 | 0.017 | 0.017 |
| | Error | 0.094 | 0.058 | 0.118 | 0.075 | 0.121 | 0.066 |
| | p value | 0.918 | 0.840 | 0.796 | 0.665 | 0.886 | 0.791 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,819 | 1,819 | 1,783 | 1,780 | 1,885 | 1,885 |
| Composite Index | Coefficient | -0.027 | 0.029 | -0.052 | -0.081 | -0.059 | -0.060 |
| | Error | 0.100 | 0.068 | 0.095 | 0.066 | 0.104 | 0.066 |
| | p value | 0.788 | 0.667 | 0.585 | 0.226 | 0.571 | 0.375 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,710 | 1,084 | 1,659 | 1,051 | 1,864 | 1,184 |

The notes from Table 25 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 29: Impact of Behavioral Component on Psychological Outlook**

| Follow-up Round | | First | | Second | | Pooled | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Well-being | Coefficient | 0.241 | 0.276 | 0.081 | 0.089 | 0.200 | 0.235 |
| | Error | 0.095 | 0.119 | 0.094 | 0.114 | 0.089 | 0.114 |
| | p value | 0.012 ** | 0.021 ** | 0.388 | 0.436 | 0.025 ** | 0.040 ** |
| | q value | 0.210 | 0.264 | 1.000 | 1.000 | 0.302 | 0.459 |
| | N | 460 | 282 | 453 | 276 | 473 | 290 |
| Aspirations | Coefficient | -0.139 | -0.135 | -0.050 | -0.049 | -0.124 | -0.092 |
| | Error | 0.078 | 0.086 | 0.092 | 0.089 | 0.081 | 0.077 |
| | p value | 0.074 * | 0.118 | 0.585 | 0.581 | 0.126 | 0.233 |
| | q value | 0.683 | 0.880 | 1.000 | 1.000 | 0.909 | 1.000 |
| | N | 458 | 376 | 442 | 441 | 473 | 472 |
| Expectations | Coefficient | -0.065 | -0.024 | 0.292 | 0.327 | 0.084 | 0.137 |
| | Error | 0.087 | 0.084 | 0.124 | 0.125 | 0.096 | 0.093 |
| | p value | 0.456 | 0.778 | 0.019 ** | 0.009 *** | 0.380 | 0.140 |
| | q value | 1.000 | 1.000 | 0.264 | 0.193 | 1.000 | 0.911 |
| | N | 452 | 452 | 440 | 439 | 473 | 473 |
| Self-control | Coefficient | 0.050 | 0.025 | 0.021 | -0.082 | 0.049 | 0.034 |
| | Error | 0.096 | 0.117 | 0.098 | 0.114 | 0.095 | 0.112 |
| | p value | 0.599 | 0.830 | 0.829 | 0.476 | 0.608 | 0.765 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 462 | 297 | 453 | 239 | 473 | 252 |
| Sense of Control | Coefficient | -0.051 | -0.091 | 0.112 | 0.093 | 0.046 | 0.009 |
| | Error | 0.094 | 0.094 | 0.091 | 0.093 | 0.091 | 0.090 |
| | p value | 0.585 | 0.332 | 0.217 | 0.319 | 0.613 | 0.922 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 463 | 462 | 453 | 431 | 473 | 472 |
| Sense of Status | Coefficient | 0.126 | 0.100 | 0.057 | 0.057 | 0.091 | 0.098 |
| | Error | 0.095 | 0.095 | 0.097 | 0.099 | 0.095 | 0.100 |
| | p value | 0.182 | 0.292 | 0.556 | 0.569 | 0.338 | 0.325 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 449 | 426 | 453 | 367 | 473 | 377 |
| Sense of Pride | Coefficient | 0.317 | 0.320 | 0.147 | 0.156 | 0.275 | 0.302 |
| | Error | 0.100 | 0.096 | 0.097 | 0.088 | 0.098 | 0.088 |
| | p value | 0.002 *** | 0.001 *** | 0.131 | 0.079 * | 0.005 *** | 0.001 *** |
| | q value | 0.055 * | 0.040 ** | 0.909 | 0.713 | 0.123 | 0.040 ** |
| | N | 463 | 463 | 453 | 431 | 473 | 451 |
| Composite Index | Coefficient | 0.136 | 0.145 | 0.196 | 0.217 | 0.167 | 0.178 |
| | Error | 0.094 | 0.111 | 0.103 | 0.118 | 0.091 | 0.105 |
| | p value | 0.149 | 0.189 | 0.057 * | 0.067 * | 0.067 * | 0.091 * |
| | q value | 0.997 | 1.000 | 0.578 | 0.647 | 0.647 | 0.819 |
| | N | 436 | 278 | 438 | 265 | 473 | 289 |

The notes from Table 25 apply, except as follows: Specification 2 uses the preferred model for so-called non-clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 30: Impact of Microenterprise over Transfer Programs on Psychological Outlook**

| Follow-up Round | | First | | Second | | Pooled | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Well-being | Coefficient | -0.035 | -0.041 | 0.083 | 0.136 | 0.015 | 0.053 |
| | Error | 0.064 | 0.071 | 0.066 | 0.078 | 0.063 | 0.061 |
| | p value | 0.585 | 0.563 | 0.209 | 0.086 * | 0.807 | 0.383 |
| | q value | 1.000 | 1.000 | 1.000 | 0.790 | 1.000 | 1.000 |
| | N | 2,275 | 1,409 | 2,232 | 1,149 | 2,357 | 1,462 |
| Aspirations | Coefficient | -0.023 | 0.000 | -0.065 | -0.049 | -0.074 | -0.040 |
| | Error | 0.059 | 0.063 | 0.069 | 0.065 | 0.067 | 0.071 |
| | p value | 0.704 | 0.998 | 0.351 | 0.453 | 0.274 | 0.572 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,243 | 1,794 | 2,137 | 2,133 | 2,348 | 1,873 |
| Expectations | Coefficient | -0.045 | -0.029 | 0.103 | 0.138 | 0.005 | 0.040 |
| | Error | 0.065 | 0.066 | 0.065 | 0.070 | 0.070 | 0.070 |
| | p value | 0.491 | 0.659 | 0.116 | 0.053 * | 0.947 | 0.575 |
| | q value | 1.000 | 1.000 | 0.880 | 0.549 | 1.000 | 1.000 |
| | N | 2,226 | 1,730 | 2,111 | 1,702 | 2,347 | 1,872 |
| Self-control | Coefficient | -0.027 | -0.024 | 0.036 | 0.058 | -0.013 | 0.013 |
| | Error | 0.087 | 0.060 | 0.100 | 0.096 | 0.108 | 0.073 |
| | p value | 0.753 | 0.692 | 0.722 | 0.545 | 0.902 | 0.861 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,279 | 1,500 | 2,236 | 1,191 | 2,358 | 1,553 |
| Sense of Control | Coefficient | 0.026 | 0.018 | 0.053 | 0.057 | 0.056 | 0.043 |
| | Error | 0.058 | 0.058 | 0.055 | 0.051 | 0.055 | 0.054 |
| | p value | 0.652 | 0.760 | 0.337 | 0.269 | 0.316 | 0.431 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,282 | 2,278 | 2,236 | 2,145 | 2,358 | 2,354 |
| Sense of Status | Coefficient | 0.020 | 0.027 | 0.087 | 0.095 | 0.076 | 0.094 |
| | Error | 0.064 | 0.057 | 0.069 | 0.069 | 0.068 | 0.057 |
| | p value | 0.753 | 0.640 | 0.205 | 0.172 | 0.264 | 0.104 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 0.869 |
| | N | 2,210 | 1,700 | 2,236 | 1,771 | 2,352 | 1,859 |
| Sense of Pride | Coefficient | -0.080 | -0.061 | 0.023 | 0.041 | -0.048 | -0.012 |
| | Error | 0.081 | 0.057 | 0.107 | 0.075 | 0.107 | 0.066 |
| | p value | 0.326 | 0.287 | 0.829 | 0.587 | 0.652 | 0.854 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,282 | 2,278 | 2,236 | 2,232 | 2,358 | 2,354 |
| Composite Index | Coefficient | -0.059 | -0.079 | 0.102 | 0.170 | 0.005 | 0.033 |
| | Error | 0.088 | 0.061 | 0.097 | 0.081 | 0.100 | 0.071 |
| | p value | 0.499 | 0.197 | 0.293 | 0.039 ** | 0.959 | 0.644 |
| | q value | 1.000 | 1.000 | 1.000 | 0.459 | 1.000 | 1.000 |
| | N | 2,146 | 1,362 | 2,097 | 1,080 | 2,337 | 1,196 |

The notes from Table 25 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set D2∪D3.

**Table 31: Impact of Spillovers on Nutrition**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Food Insecurity | Coefficient | -0.652 | -0.629 | 0.135 | 0.144 | -0.312 | -0.279 |
| | Error | 0.703 | 0.324 | 0.596 | 0.327 | 0.623 | 0.283 |
| | p value | 0.356 | 0.054 * | 0.821 | 0.661 | 0.617 | 0.326 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,097 | 3,090 | 3,028 | 3,021 | 3,188 | 3,181 |
| Dietary Diversity | Coefficient | 0.019 | -0.025 | -0.020 | -0.033 | 0.009 | -0.030 |
| | Error | 0.172 | 0.098 | 0.150 | 0.095 | 0.150 | 0.080 |
| | p value | 0.915 | 0.802 | 0.892 | 0.729 | 0.951 | 0.713 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,097 | 3,097 | 3,028 | 3,021 | 3,188 | 3,188 |
| Composite Index | Coefficient | 0.063 | 0.040 | -0.020 | -0.031 | 0.037 | 0.012 |
| | Error | 0.112 | 0.050 | 0.100 | 0.052 | 0.116 | 0.050 |
| | p value | 0.573 | 0.427 | 0.845 | 0.551 | 0.753 | 0.809 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,097 | 3,090 | 3,028 | 3,021 | 3,188 | 3,181 |

Notes: Specification 1 uses simple linear regression. Specification 2 uses the preferred model for so-called clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. Errors are adjusted for cluster robustness. Food insecurity is measured using the Household Food Insecurity Access Score (HFIAS) and dietary diversity is measured using the Household Dietary Diversity Score (HDDS). The composite is calculated by standardizing both HFIAS and HDDS, and subtracting the former from the latter; the composite is then itself standardized.

**Table 32: Impact of Microenterprise Programs on Nutrition**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Food Insecurity | Coefficient | -0.528 | -0.936 | -0.396 | -0.495 | -0.475 | -0.719 |
| | Error | 0.363 | 0.222 | 0.280 | 0.189 | 0.302 | 0.178 |
| | p value | 0.148 | 0.000 *** | 0.160 | 0.010 *** | 0.118 | 0.000 *** |
| | q value | 0.102 | 0.001 *** | 0.102 | 0.019 ** | 0.102 | 0.001 *** |
| | N | 4,916 | 3,901 | 4,811 | 3,815 | 5,073 | 4,021 |
| Dietary Diversity | Coefficient | 0.140 | 0.167 | 0.100 | 0.147 | 0.107 | 0.159 |
| | Error | 0.091 | 0.061 | 0.083 | 0.073 | 0.080 | 0.062 |
| | p value | 0.125 | 0.007 *** | 0.230 | 0.046 ** | 0.185 | 0.011 ** |
| | q value | 0.102 | 0.016 ** | 0.131 | 0.054 * | 0.109 | 0.019 ** |
| | N | 4,916 | 4,906 | 4,811 | 3,815 | 5,073 | 4,021 |
| Composite Index | Coefficient | 0.091 | 0.148 | 0.070 | 0.094 | 0.090 | 0.135 |
| | Error | 0.057 | 0.035 | 0.048 | 0.032 | 0.057 | 0.034 |
| | p value | 0.114 | 0.000 *** | 0.146 | 0.004 *** | 0.121 | 0.000 *** |
| | q value | 0.102 | 0.001 *** | 0.102 | 0.012 ** | 0.102 | 0.001 *** |
| | N | 4,916 | 3,901 | 4,811 | 3,815 | 5,073 | 4,021 |

The notes from Table 31 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 33: Impact of Transfer Programs on Nutrition**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Food Insecurity | Coefficient | -0.014 | 0.132 | -0.588 | -0.130 | -0.356 | -0.046 |
| | Error | 0.584 | 0.357 | 0.543 | 0.362 | 0.522 | 0.302 |
| | p value | 0.981 | 0.712 | 0.281 | 0.720 | 0.497 | 0.879 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,560 | 2,840 | 3,481 | 2,764 | 3,661 | 2,916 |
| Dietary Diversity | Coefficient | 0.124 | 0.073 | 0.066 | 0.020 | 0.092 | 0.056 |
| | Error | 0.137 | 0.095 | 0.124 | 0.095 | 0.115 | 0.080 |
| | p value | 0.369 | 0.442 | 0.597 | 0.836 | 0.426 | 0.489 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,560 | 3,552 | 3,481 | 2,764 | 3,661 | 2,916 |
| Composite Index | Coefficient | 0.040 | 0.016 | 0.078 | 0.016 | 0.071 | 0.021 |
| | Error | 0.089 | 0.054 | 0.082 | 0.050 | 0.091 | 0.050 |
| | p value | 0.651 | 0.764 | 0.341 | 0.744 | 0.437 | 0.670 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,560 | 2,840 | 3,481 | 2,764 | 3,661 | 2,916 |

The notes from Table 31 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 34: Impact of Savings Component on Nutrition**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Food Insecurity | Coefficient | 0.541 | 0.433 | 0.469 | 0.355 | 0.483 | 0.399 |
| | Error | 0.988 | 0.412 | 0.758 | 0.372 | 0.846 | 0.339 |
| | p value | 0.586 | 0.297 | 0.538 | 0.344 | 0.570 | 0.244 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,819 | 1,816 | 1,783 | 1,783 | 1,885 | 1,885 |
| Dietary Diversity | Coefficient | 0.060 | 0.069 | 0.100 | 0.107 | 0.048 | 0.054 |
| | Error | 0.238 | 0.116 | 0.192 | 0.116 | 0.210 | 0.107 |
| | p value | 0.801 | 0.554 | 0.605 | 0.360 | 0.820 | 0.614 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,819 | 1,819 | 1,783 | 1,783 | 1,885 | 1,885 |
| Composite Index | Coefficient | -0.029 | -0.017 | -0.017 | 0.000 | -0.034 | -0.019 |
| | Error | 0.150 | 0.057 | 0.131 | 0.061 | 0.156 | 0.060 |
| | p value | 0.845 | 0.767 | 0.900 | 0.995 | 0.827 | 0.757 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,819 | 1,816 | 1,783 | 1,783 | 1,885 | 1,885 |

The notes from Table 31 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 35: Impact of Behavioral Component on Nutrition**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Food Insecurity | Coefficient | -0.889 | -0.894 | -0.482 | -0.158 | -0.669 | -0.623 |
| | Error | 0.587 | 0.570 | 0.552 | 0.480 | 0.464 | 0.379 |
| | p value | 0.131 | 0.117 | 0.383 | 0.742 | 0.150 | 0.101 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 463 | 379 | 453 | 453 | 473 | 473 |
| Dietary Diversity | Coefficient | -0.140 | -0.122 | 0.016 | 0.099 | -0.070 | -0.032 |
| | Error | 0.173 | 0.160 | 0.171 | 0.158 | 0.140 | 0.125 |
| | p value | 0.420 | 0.444 | 0.927 | 0.532 | 0.614 | 0.797 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 463 | 462 | 453 | 452 | 473 | 472 |
| Composite Index | Coefficient | 0.043 | 0.046 | 0.055 | 0.052 | 0.049 | 0.066 |
| | Error | 0.099 | 0.085 | 0.096 | 0.084 | 0.096 | 0.079 |
| | p value | 0.665 | 0.585 | 0.564 | 0.536 | 0.610 | 0.399 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 463 | 462 | 453 | 453 | 473 | 473 |

The notes from Table 31 apply, except as follows: Specification 2 uses the preferred model for so-called non-clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 36: Impact of Microenterprise over Transfer Programs on Nutrition**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Food Insecurity | Coefficient | -0.514 | -0.739 | 0.192 | -0.302 | -0.120 | -0.608 |
| | Error | 0.738 | 0.398 | 0.637 | 0.397 | 0.646 | 0.344 |
| | p value | 0.488 | 0.067 * | 0.764 | 0.450 | 0.854 | 0.081 * |
| | q value | 1.000 | 0.939 | 1.000 | 1.000 | 1.000 | 0.939 |
| | N | 2,282 | 2,282 | 2,236 | 1,787 | 2,358 | 1,879 |
| Dietary Diversity | Coefficient | 0.016 | 0.115 | 0.034 | 0.106 | 0.015 | 0.104 |
| | Error | 0.172 | 0.112 | 0.152 | 0.113 | 0.149 | 0.092 |
| | p value | 0.927 | 0.310 | 0.822 | 0.352 | 0.919 | 0.264 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,282 | 2,282 | 2,236 | 2,236 | 2,358 | 2,358 |
| Composite Index | Coefficient | 0.054 | 0.117 | -0.008 | 0.044 | 0.020 | 0.083 |
| | Error | 0.120 | 0.064 | 0.107 | 0.062 | 0.125 | 0.062 |
| | p value | 0.650 | 0.069 * | 0.937 | 0.480 | 0.876 | 0.183 |
| | q value | 1.000 | 0.939 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,282 | 2,282 | 2,236 | 2,236 | 2,358 | 2,358 |

The notes from Table 31 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set D2∪D3.

**Table 37: Impact of Spillovers on Social Conditions**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Sense of Community | Coefficient | -0.017 | -0.024 | 0.044 | 0.048 | -0.017 | -0.024 |
| | Error | 0.043 | 0.040 | 0.047 | 0.050 | 0.043 | 0.040 |
| | p value | 0.690 | 0.558 | 0.349 | 0.336 | 0.690 | 0.558 |
| | q value | 1.000 | 1.000 | 0.970 | 0.970 | 1.000 | 1.000 |
| | N | 3,046 | 3,014 | 3,028 | 2,372 | 3,046 | 3,014 |
| Sense of Trust | Coefficient | 0.076 | 0.079 | 0.040 | 0.038 | 0.077 | 0.082 |
| | Error | 0.058 | 0.057 | 0.047 | 0.046 | 0.054 | 0.053 |
| | p value | 0.189 | 0.166 | 0.401 | 0.404 | 0.162 | 0.127 |
| | q value | 0.939 | 0.939 | 1.000 | 1.000 | 0.939 | 0.939 |
| | N | 3,093 | 3,086 | 3,019 | 3,012 | 3,188 | 3,188 |
| Risk Sharing | Coefficient | -0.001 | 0.001 | 0.084 | 0.120 | 0.047 | 0.044 |
| | Error | 0.081 | 0.078 | 0.089 | 0.056 | 0.096 | 0.073 |
| | p value | 0.986 | 0.995 | 0.346 | 0.035 ** | 0.626 | 0.544 |
| | q value | 1.000 | 1.000 | 0.970 | 0.690 | 1.000 | 1.000 |
| | N | 3,096 | 3,089 | 3,027 | 2,396 | 3,188 | 3,181 |
| Empowerment of Women | Coefficient | 0.114 | 0.128 | -0.042 | -0.038 | 0.042 | 0.056 |
| | Error | 0.044 | 0.056 | 0.043 | 0.047 | 0.044 | 0.045 |
| | p value | 0.011 ** | 0.024 ** | 0.322 | 0.419 | 0.342 | 0.209 |
| | q value | 0.661 | 0.690 | 0.970 | 1.000 | 0.970 | 0.939 |
| | N | 3,324 | 2,575 | 3,324 | 2,575 | 3,324 | 3,221 |
| Safety from Intimate Partner Violence | Coefficient | 0.034 | 0.027 | 0.076 | 0.057 | 0.050 | 0.037 |
| | Error | 0.058 | 0.053 | 0.053 | 0.053 | 0.052 | 0.049 |
| | p value | 0.556 | 0.607 | 0.160 | 0.284 | 0.338 | 0.458 |
| | q value | 1.000 | 1.000 | 0.939 | 0.970 | 0.970 | 1.000 |
| | N | 2,013 | 1,955 | 1,905 | 1,843 | 2,317 | 2,246 |
| Composite Index | Coefficient | 0.120 | 0.124 | 0.076 | 0.061 | 0.096 | 0.079 |
| | Error | 0.061 | 0.066 | 0.061 | 0.061 | 0.057 | 0.060 |
| | p value | 0.053 * | 0.064 * | 0.219 | 0.316 | 0.093 * | 0.190 |
| | q value | 0.731 | 0.731 | 0.939 | 0.970 | 0.929 | 0.939 |
| | N | 1,992 | 1,914 | 1,898 | 1,819 | 2,243 | 2,150 |

Notes: Specification 1 uses simple linear regression. Specification 2 uses the preferred model for so-called clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. Errors are adjusted for cluster robustness. The sub-composites are standardized; the composite is the standardized sum of the sub-composites.

**Table 38: Impact of Microenterprise Programs on Social Conditions**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Sense of Community | Coefficient | 0.055 | 0.071 | 0.053 | 0.032 | 0.055 | 0.071 |
| | Error | 0.027 | 0.030 | 0.030 | 0.033 | 0.027 | 0.030 |
| | p value | 0.046 ** | 0.018 ** | 0.078 * | 0.343 | 0.046 ** | 0.018 ** |
| | q value | 0.309 | 0.309 | 0.338 | 0.783 | 0.309 | 0.309 |
| | N | 4,839 | 3,811 | 4,811 | 3,688 | 4,839 | 3,811 |
| Sense of Trust | Coefficient | 0.061 | 0.055 | 0.048 | 0.049 | 0.076 | 0.082 |
| | Error | 0.037 | 0.039 | 0.036 | 0.035 | 0.037 | 0.039 |
| | p value | 0.097 * | 0.161 | 0.186 | 0.171 | 0.042 ** | 0.036 ** |
| | q value | 0.375 | 0.491 | 0.506 | 0.491 | 0.309 | 0.309 |
| | N | 4,912 | 3,794 | 4,796 | 4,786 | 5,073 | 4,021 |
| Risk Sharing | Coefficient | -0.026 | -0.029 | 0.030 | 0.021 | -0.003 | -0.009 |
| | Error | 0.044 | 0.041 | 0.045 | 0.032 | 0.047 | 0.036 |
| | p value | 0.557 | 0.477 | 0.509 | 0.508 | 0.947 | 0.811 |
| | q value | 0.880 | 0.842 | 0.842 | 0.842 | 1.000 | 1.000 |
| | N | 4,915 | 4,905 | 4,807 | 4,807 | 5,073 | 5,073 |
| Empowerment of Women | Coefficient | -0.004 | -0.005 | 0.021 | 0.026 | 0.002 | 0.004 |
| | Error | 0.030 | 0.031 | 0.032 | 0.032 | 0.031 | 0.031 |
| | p value | 0.888 | 0.872 | 0.522 | 0.415 | 0.949 | 0.907 |
| | q value | 1.000 | 1.000 | 0.842 | 0.842 | 1.000 | 1.000 |
| | N | 5,294 | 5,117 | 5,294 | 5,117 | 5,294 | 5,117 |
| Safety from Intimate Partner Violence | Coefficient | 0.020 | 0.025 | -0.001 | 0.006 | -0.002 | 0.007 |
| | Error | 0.037 | 0.036 | 0.038 | 0.039 | 0.035 | 0.035 |
| | p value | 0.580 | 0.487 | 0.974 | 0.886 | 0.946 | 0.844 |
| | q value | 0.883 | 0.842 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,195 | 3,086 | 3,034 | 2,560 | 3,677 | 3,551 |
| Composite Index | Coefficient | 0.048 | 0.046 | 0.060 | 0.080 | 0.054 | 0.088 |
| | Error | 0.039 | 0.041 | 0.041 | 0.042 | 0.037 | 0.041 |
| | p value | 0.225 | 0.267 | 0.144 | 0.059 * | 0.156 | 0.032 ** |
| | q value | 0.578 | 0.692 | 0.491 | 0.309 | 0.491 | 0.309 |
| | N | 3,162 | 3,024 | 3,025 | 2,533 | 3,555 | 2,926 |

The notes from Table 37 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 39: Impact of Transfer Programs on Social Conditions**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Sense of Community | Coefficient | -0.010 | 0.012 | 0.079 | 0.054 | -0.010 | 0.012 |
| | Error | 0.054 | 0.043 | 0.054 | 0.058 | 0.054 | 0.043 |
| | p value | 0.850 | 0.789 | 0.149 | 0.352 | 0.850 | 0.789 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,497 | 2,766 | 3,481 | 2,679 | 3,497 | 2,766 |
| Sense of Trust | Coefficient | 0.057 | 0.048 | -0.169 | -0.176 | -0.057 | -0.047 |
| | Error | 0.054 | 0.050 | 0.059 | 0.060 | 0.053 | 0.051 |
| | p value | 0.290 | 0.337 | 0.005 *** | 0.004 *** | 0.285 | 0.357 |
| | q value | 1.000 | 1.000 | 0.091 * | 0.091 * | 1.000 | 1.000 |
| | N | 3,556 | 2,771 | 3,472 | 3,464 | 3,661 | 2,916 |
| Risk Sharing | Coefficient | -0.035 | -0.028 | -0.021 | -0.009 | -0.023 | -0.010 |
| | Error | 0.072 | 0.069 | 0.081 | 0.051 | 0.080 | 0.061 |
| | p value | 0.624 | 0.680 | 0.799 | 0.860 | 0.778 | 0.873 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,559 | 3,551 | 3,480 | 3,480 | 3,661 | 3,661 |
| Empowerment of Women | Coefficient | -0.043 | -0.033 | -0.025 | -0.015 | -0.067 | -0.052 |
| | Error | 0.057 | 0.055 | 0.054 | 0.051 | 0.053 | 0.048 |
| | p value | 0.452 | 0.550 | 0.651 | 0.770 | 0.205 | 0.283 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,804 | 3,679 | 3,804 | 3,679 | 3,804 | 3,679 |
| Safety from Intimate Partner Violence | Coefficient | 0.033 | 0.020 | -0.001 | 0.000 | 0.028 | 0.013 |
| | Error | 0.073 | 0.068 | 0.058 | 0.069 | 0.061 | 0.062 |
| | p value | 0.657 | 0.769 | 0.983 | 0.996 | 0.645 | 0.839 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,326 | 2,254 | 2,212 | 1,867 | 2,669 | 2,580 |
| Composite Index | Coefficient | -0.007 | -0.024 | -0.079 | -0.086 | -0.057 | -0.025 |
| | Error | 0.076 | 0.076 | 0.064 | 0.066 | 0.060 | 0.061 |
| | p value | 0.930 | 0.752 | 0.218 | 0.192 | 0.341 | 0.681 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,300 | 2,206 | 2,205 | 1,847 | 2,579 | 2,131 |

The notes from Table 37 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 40: Impact of Savings Component on Social Conditions**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Sense of Community | Coefficient | 0.050 | 0.049 | 0.031 | 0.029 | 0.050 | 0.049 |
| | Error | 0.061 | 0.055 | 0.052 | 0.044 | 0.061 | 0.055 |
| | p value | 0.423 | 0.371 | 0.561 | 0.512 | 0.423 | 0.371 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,793 | 1,780 | 1,783 | 1,768 | 1,793 | 1,780 |
| Sense of Trust | Coefficient | 0.035 | 0.029 | 0.050 | 0.046 | 0.050 | 0.042 |
| | Error | 0.074 | 0.071 | 0.060 | 0.062 | 0.074 | 0.073 |
| | p value | 0.636 | 0.682 | 0.416 | 0.463 | 0.506 | 0.562 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,819 | 1,746 | 1,777 | 1,708 | 1,885 | 1,812 |
| Risk Sharing | Coefficient | 0.031 | 0.032 | 0.028 | 0.023 | 0.037 | 0.036 |
| | Error | 0.103 | 0.089 | 0.115 | 0.062 | 0.121 | 0.074 |
| | p value | 0.765 | 0.723 | 0.811 | 0.714 | 0.761 | 0.627 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,819 | 1,816 | 1,780 | 1,780 | 1,885 | 1,882 |
| Empowerment of Women | Coefficient | 0.010 | 0.015 | 0.038 | 0.045 | 0.039 | 0.046 |
| | Error | 0.053 | 0.053 | 0.061 | 0.055 | 0.059 | 0.055 |
| | p value | 0.856 | 0.779 | 0.539 | 0.410 | 0.508 | 0.404 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,970 | 1,896 | 1,970 | 1,967 | 1,970 | 1,896 |
| Safety from Intimate Partner Violence | Coefficient | 0.192 | 0.191 | 0.097 | 0.120 | 0.155 | 0.147 |
| | Error | 0.072 | 0.064 | 0.064 | 0.068 | 0.065 | 0.069 |
| | p value | 0.010 ** | 0.004 *** | 0.133 | 0.082 * | 0.020 ** | 0.036 ** |
| | q value | 0.220 | 0.187 | 0.449 | 0.358 | 0.258 | 0.258 |
| | N | 1,182 | 1,131 | 1,129 | 957 | 1,360 | 1,131 |
| Composite Index | Coefficient | 0.162 | 0.187 | 0.107 | 0.115 | 0.149 | 0.165 |
| | Error | 0.085 | 0.086 | 0.070 | 0.070 | 0.072 | 0.074 |
| | p value | 0.061 * | 0.034 ** | 0.130 | 0.107 | 0.043 ** | 0.030 ** |
| | q value | 0.286 | 0.258 | 0.449 | 0.431 | 0.264 | 0.258 |
| | N | 1,170 | 1,110 | 1,127 | 1,076 | 1,312 | 1,246 |

The notes from Table 37 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 41: Impact of Behavioral Component on Social Conditions**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Sense of Community | Coefficient | -0.009 | 0.024 | -0.210 | -0.107 | -0.009 | 0.024 |
| | Error | 0.095 | 0.105 | 0.099 | 0.109 | 0.095 | 0.105 |
| | p value | 0.929 | 0.816 | 0.035 ** | 0.328 | 0.929 | 0.816 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 451 | 359 | 453 | 367 | 451 | 359 |
| Sense of Trust | Coefficient | 0.035 | 0.067 | -0.077 | -0.122 | 0.006 | -0.008 |
| | Error | 0.090 | 0.099 | 0.094 | 0.094 | 0.090 | 0.100 |
| | p value | 0.694 | 0.497 | 0.412 | 0.197 | 0.949 | 0.938 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 463 | 370 | 453 | 431 | 473 | 378 |
| Risk Sharing | Coefficient | -0.116 | -0.082 | 0.079 | 0.098 | -0.010 | -0.001 |
| | Error | 0.094 | 0.100 | 0.093 | 0.088 | 0.095 | 0.088 |
| | p value | 0.218 | 0.410 | 0.392 | 0.263 | 0.916 | 0.992 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 463 | 379 | 453 | 452 | 473 | 472 |
| Empowerment of Women | Coefficient | -0.110 | -0.115 | 0.054 | -0.063 | -0.025 | -0.124 |
| | Error | 0.090 | 0.088 | 0.090 | 0.104 | 0.087 | 0.098 |
| | p value | 0.221 | 0.193 | 0.547 | 0.547 | 0.775 | 0.208 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 480 | 479 | 480 | 384 | 480 | 384 |
| Safety from Intimate Partner Violence | Coefficient | -0.023 | 0.028 | 0.180 | 0.114 | 0.096 | 0.112 |
| | Error | 0.111 | 0.111 | 0.115 | 0.126 | 0.106 | 0.108 |
| | p value | 0.840 | 0.804 | 0.118 | 0.367 | 0.366 | 0.300 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 313 | 312 | 307 | 271 | 352 | 334 |
| Composite Index | Coefficient | -0.107 | -0.089 | 0.133 | 0.030 | 0.069 | 0.022 |
| | Error | 0.109 | 0.117 | 0.111 | 0.117 | 0.106 | 0.112 |
| | p value | 0.328 | 0.446 | 0.233 | 0.798 | 0.517 | 0.847 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 308 | 261 | 307 | 263 | 336 | 284 |

The notes from Table 37 apply, except as follows: Specification 2 uses the preferred model for so-called non-clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 42: Impact of Microenterprise over Transfer Programs on Social Conditions**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Sense of Community | Coefficient | 0.068 | 0.089 | -0.027 | -0.022 | 0.068 | 0.089 |
| | Error | 0.063 | 0.055 | 0.060 | 0.061 | 0.063 | 0.055 |
| | p value | 0.284 | 0.110 | 0.658 | 0.722 | 0.284 | 0.110 |
| | q value | 1.000 | 0.534 | 1.000 | 1.000 | 1.000 | 0.534 |
| | N | 2,244 | 2,138 | 2,236 | 1,777 | 2,244 | 2,138 |
| Sense of Trust | Coefficient | 0.003 | 0.010 | 0.208 | 0.223 | 0.132 | 0.144 |
| | Error | 0.060 | 0.055 | 0.061 | 0.064 | 0.061 | 0.059 |
| | p value | 0.966 | 0.863 | 0.001 *** | 0.001 *** | 0.035 ** | 0.017 ** |
| | q value | 1.000 | 1.000 | 0.017 ** | 0.017 ** | 0.393 | 0.233 |
| | N | 2,282 | 1,763 | 2,230 | 2,139 | 2,358 | 1,823 |
| Risk Sharing | Coefficient | 0.009 | -0.002 | 0.050 | 0.040 | 0.020 | -0.002 |
| | Error | 0.086 | 0.079 | 0.094 | 0.055 | 0.097 | 0.068 |
| | p value | 0.913 | 0.980 | 0.597 | 0.468 | 0.840 | 0.972 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,282 | 2,278 | 2,233 | 2,229 | 2,358 | 2,354 |
| Empowerment of Women | Coefficient | 0.034 | 0.031 | 0.047 | 0.034 | 0.066 | 0.033 |
| | Error | 0.059 | 0.055 | 0.059 | 0.058 | 0.058 | 0.053 |
| | p value | 0.568 | 0.579 | 0.433 | 0.561 | 0.258 | 0.535 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,450 | 2,446 | 2,450 | 2,354 | 2,450 | 1,948 |
| Safety from Intimate Partner Violence | Coefficient | -0.014 | -0.014 | -0.001 | 0.011 | -0.034 | -0.011 |
| | Error | 0.084 | 0.075 | 0.065 | 0.070 | 0.075 | 0.074 |
| | p value | 0.871 | 0.852 | 0.985 | 0.876 | 0.650 | 0.878 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,495 | 1,493 | 1,436 | 1,378 | 1,712 | 1,639 |
| Composite Index | Coefficient | 0.053 | 0.067 | 0.142 | 0.146 | 0.109 | 0.109 |
| | Error | 0.085 | 0.084 | 0.070 | 0.074 | 0.067 | 0.069 |
| | p value | 0.536 | 0.422 | 0.047 ** | 0.051 * | 0.106 | 0.116 |
| | q value | 1.000 | 1.000 | 0.393 | 0.393 | 0.534 | 0.534 |
| | N | 1,478 | 1,402 | 1,434 | 1,253 | 1,648 | 1,363 |

The notes from Table 37 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set D2∪D3.

**Table 43: Impact of Spillovers on Labor**

| Follow-up round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Active in Labor Force | Odds ratio | 1.021 | 1.098 | 1.117 | 1.108 |
| | Error | 0.102 | 0.113 | 0.092 | 0.101 |
| | p value | 0.834 | 0.363 | 0.180 | 0.261 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 8,313 | 6,401 | 9,089 | 6,456 |
| Active in Microenterprise | Odds ratio | 1.093 | 1.163 | 1.064 | 1.035 |
| | Error | 0.139 | 0.128 | 0.090 | 0.096 |
| | p value | 0.484 | 0.173 | 0.460 | 0.710 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 8,315 | 6,405 | 9,108 | 6,470 |
| Active as Employee or Day Laborer | Odds ratio | 0.904 | 0.864 | 1.029 | 1.010 |
| | Error | 0.109 | 0.070 | 0.124 | 0.091 |
| | p value | 0.401 | 0.071 * | 0.814 | 0.911 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 8,321 | 6,417 | 9,119 | 6,479 |
| Active in more than one Livelihood | Odds ratio | 1.070 | 1.085 | 0.984 | 1.004 |
| | Error | 0.111 | 0.109 | 0.085 | 0.093 |
| | p value | 0.514 | 0.414 | 0.853 | 0.965 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 8,323 | 6,419 | 9,124 | 6,484 |

Notes: Specification 1 uses simple regression. Specification 2 uses the preferred model for so-called clustered comparisons. In the case of binary outcomes, logistic regression is applied, estimates are presented as odds ratios, and no pooled follow-up round is created. Errors are adjusted for cluster robustness. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. The unit of observation is not the household, but the individual household member; only members aged 15 and over are included. Microenterprise activity implies either farm-based or non-farm-based self-employment. Labor force participation implies microenterprise activity or paid work activity.

**Table 44: Impact of Microenterprise Programs on Labor**

| Follow-up round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Active in Labor Force | Odds ratio | 0.928 | 0.994 | 1.034 | 1.029 |
| | Error | 0.060 | 0.066 | 0.055 | 0.070 |
| | p value | 0.251 | 0.932 | 0.537 | 0.672 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 13,290 | 10,222 | 14,438 | 10,238 |
| Active in Microenterprise | Odds ratio | 0.978 | 1.076 | 1.055 | 1.070 |
| | Error | 0.071 | 0.078 | 0.055 | 0.081 |
| | p value | 0.763 | 0.309 | 0.311 | 0.367 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 13,297 | 8,287 | 14,464 | 8,290 |
| Active as Employee or Day Laborer | Odds ratio | 0.874 | 0.870 | 0.959 | 0.958 |
| | Error | 0.057 | 0.054 | 0.060 | 0.058 |
| | p value | 0.038 ** | 0.026 ** | 0.500 | 0.476 |
| | q value | 0.442 | 0.442 | 1.000 | 1.000 |
| | N | 13,309 | 10,248 | 14,478 | 8,292 |
| Active in more than one Livelihood | Odds ratio | 1.024 | 1.039 | 1.012 | 1.018 |
| | Error | 0.058 | 0.064 | 0.054 | 0.057 |
| | p value | 0.670 | 0.539 | 0.829 | 0.746 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 13,311 | 10,254 | 14,482 | 10,277 |

The notes from Table 43 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 45: Impact of Transfer Programs on Labor**

| Follow-up round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Active in Labor Force | Odds ratio | 1.150 | 1.222 | 1.260 | 1.361 |
| | Error | 0.153 | 0.169 | 0.120 | 0.152 |
| | p value | 0.292 | 0.146 | 0.015 ** | 0.006 *** |
| | q value | 0.502 | 0.264 | 0.051 * | 0.028 ** |
| | N | 9,609 | 7,418 | 10,482 | 7,449 |
| Active in Microenterprise | Odds ratio | 1.278 | 1.317 | 1.402 | 1.550 |
| | Error | 0.151 | 0.157 | 0.122 | 0.194 |
| | p value | 0.038 ** | 0.021 ** | 0.000 *** | 0.000 *** |
| | q value | 0.075 * | 0.057 * | 0.002 *** | 0.004 *** |
| | N | 9,611 | 6,061 | 10,500 | 6,046 |
| Active as Employee or Day Laborer | Odds ratio | 0.945 | 1.011 | 0.999 | 1.033 |
| | Error | 0.124 | 0.127 | 0.140 | 0.127 |
| | p value | 0.666 | 0.933 | 0.994 | 0.793 |
| | q value | 0.999 | 1.000 | 1.000 | 1.000 |
| | N | 9,619 | 7,434 | 10,514 | 6,053 |
| Active in more than one Livelihood | Odds ratio | 0.981 | 1.058 | 0.883 | 0.915 |
| | Error | 0.108 | 0.121 | 0.114 | 0.126 |
| | p value | 0.860 | 0.622 | 0.337 | 0.517 |
| | q value | 1.000 | 0.999 | 0.508 | 0.871 |
| | N | 9,621 | 7,436 | 10,517 | 7,478 |

The notes from Table 43 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 46: Impact of Savings Component on Labor**

| Follow-up round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Active in Labor Force | Odds ratio | 1.114 | 1.112 | 0.841 | 0.814 |
| | Error | 0.140 | 0.113 | 0.074 | 0.105 |
| | p value | 0.390 | 0.300 | 0.049 ** | 0.110 |
| | q value | 1.000 | 1.000 | 0.674 | 0.889 |
| | N | 4,977 | 3,081 | 5,349 | 3,782 |
| Active in Microenterprise | Odds ratio | 1.121 | 1.166 | 0.927 | 0.883 |
| | Error | 0.173 | 0.130 | 0.082 | 0.102 |
| | p value | 0.457 | 0.168 | 0.393 | 0.280 |
| | q value | 1.000 | 0.889 | 1.000 | 1.000 |
| | N | 4,982 | 3,826 | 5,356 | 3,946 |
| Active as Employee or Day Laborer | Odds ratio | 0.904 | 0.784 | 0.907 | 0.860 |
| | Error | 0.141 | 0.097 | 0.138 | 0.089 |
| | p value | 0.517 | 0.050 * | 0.521 | 0.147 |
| | q value | 1.000 | 0.674 | 1.000 | 0.889 |
| | N | 4,988 | 3,089 | 5,359 | 3,788 |
| Active in more than one Livelihood | Odds ratio | 0.996 | 0.974 | 1.066 | 1.084 |
| | Error | 0.107 | 0.111 | 0.102 | 0.102 |
| | p value | 0.967 | 0.816 | 0.506 | 0.394 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,988 | 4,009 | 5,358 | 3,793 |

The notes from Table 43 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 47: Impact of Behavioral Component on Labor**

| Follow-up round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Active in Labor Force | Odds ratio | 0.944 | 1.061 | 0.759 | 0.919 |
| | Error | 0.137 | 0.200 | 0.110 | 0.207 |
| | p value | 0.689 | 0.754 | 0.057 * | 0.708 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,296 | 960 | 1,393 | 795 |
| Active in Microenterprise | Odds ratio | 0.991 | 1.034 | 0.829 | 0.995 |
| | Error | 0.123 | 0.160 | 0.102 | 0.180 |
| | p value | 0.942 | 0.829 | 0.128 | 0.980 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,296 | 1,055 | 1,392 | 837 |
| Active as Employee or Day Laborer | Odds ratio | 0.842 | 1.009 | 0.930 | 0.933 |
| | Error | 0.099 | 0.160 | 0.105 | 0.151 |
| | p value | 0.146 | 0.956 | 0.522 | 0.670 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,298 | 880 | 1,395 | 815 |
| Active in more than one Livelihood | Odds ratio | 0.848 | 0.975 | 0.815 | 0.893 |
| | Error | 0.108 | 0.144 | 0.098 | 0.137 |
| | p value | 0.194 | 0.863 | 0.090 * | 0.461 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,298 | 1,060 | 1,393 | 994 |

The notes from Table 43 apply, except as follows: Specification 2 uses the preferred model for so-called non-clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 48: Impact of Microenterprise over Transfer Programs on Labor**

| Follow-up round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Active in Labor Force | Odds ratio | 0.807 | 0.799 | 0.820 | 0.746 |
| | Error | 0.117 | 0.115 | 0.082 | 0.093 |
| | p value | 0.137 | 0.120 | 0.047 ** | 0.019 ** |
| | q value | 0.208 | 0.207 | 0.124 | 0.072 * |
| | N | 6,273 | 4,838 | 6,742 | 4,775 |
| Active in Microenterprise | Odds ratio | 0.765 | 0.736 | 0.752 | 0.727 |
| | Error | 0.107 | 0.093 | 0.071 | 0.082 |
| | p value | 0.055 * | 0.015 ** | 0.002 *** | 0.005 *** |
| | q value | 0.124 | 0.072 * | 0.040 ** | 0.040 ** |
| | N | 6,278 | 4,839 | 6,748 | 4,779 |
| Active as Employee or Day Laborer | Odds ratio | 0.925 | 0.908 | 0.960 | 0.894 |
| | Error | 0.138 | 0.119 | 0.154 | 0.123 |
| | p value | 0.601 | 0.462 | 0.799 | 0.414 |
| | q value | 0.587 | 0.468 | 0.743 | 0.468 |
| | N | 6,286 | 4,089 | 6,754 | 4,993 |
| Active in more than one Livelihood | Odds ratio | 1.044 | 1.005 | 1.146 | 1.110 |
| | Error | 0.127 | 0.125 | 0.163 | 0.164 |
| | p value | 0.722 | 0.966 | 0.338 | 0.478 |
| | q value | 0.702 | 0.935 | 0.430 | 0.468 |
| | N | 6,286 | 4,852 | 6,751 | 4,787 |

The notes from Table 43 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set D2∪D3.

**Table 49: Impact of Spillovers on Schooling**

| Follow-up Round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Enrolled in and Attending School | Odds ratio | 0.981 | 0.916 | 1.016 | 1.055 |
| | Error | 0.097 | 0.122 | 0.107 | 0.144 |
| | p value | 0.849 | 0.513 | 0.881 | 0.697 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 6,744 | 4,437 | 8,576 | 4,552 |
| Repeated Year | Odds ratio | 1.046 | 1.053 | 0.952 | 0.913 |
| | Error | 0.121 | 0.117 | 0.097 | 0.104 |
| | p value | 0.698 | 0.639 | 0.627 | 0.425 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 5,656 | 4,141 | 6,686 | 3,405 |
| Days worked last Month | Coefficient | -1.578 | -0.473 | 2.066 | 3.737 |
| | Error | 3.441 | 2.678 | 2.219 | 2.340 |
| | p value | 0.647 | 0.860 | 0.353 | 0.112 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 6,845 | 5,376 | 8,577 | 4,609 |
| School Days Missed last Month | Coefficient | 0.149 | 0.206 | 0.122 | 0.133 |
| | Error | 0.153 | 0.149 | 0.125 | 0.111 |
| | p value | 0.332 | 0.168 | 0.330 | 0.234 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 5,657 | 4,119 | 6,563 | 3,315 |

Notes: Specification 1 uses simple regression. Specification 2 uses the preferred model for so-called clustered comparisons. In the case of binary outcomes, logistic regression is applied, estimates are presented as odds ratios, and no pooled follow-up round is created. Errors are adjusted for cluster robustness. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. The unit of observation is not the household, but the individual household member; only members aged 5 through 17 are included.

**Table 50: Impact of Microenterprise Programs on Schooling**

| Follow-up Round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Enrolled in and Attending School | Odds ratio | 0.958 | 0.903 | 1.032 | 1.004 |
| | Error | 0.066 | 0.081 | 0.063 | 0.092 |
| | p value | 0.536 | 0.257 | 0.609 | 0.967 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 10,786 | 7,123 | 13,646 | 7,272 |
| Repeated Year | Odds ratio | 1.008 | 1.027 | 0.950 | 0.980 |
| | Error | 0.072 | 0.081 | 0.052 | 0.074 |
| | p value | 0.910 | 0.732 | 0.351 | 0.788 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 9,023 | 5,675 | 10,662 | 5,514 |
| Days worked last Month | Coefficient | 0.192 | 0.110 | 1.237 | 0.772 |
| | Error | 1.759 | 1.556 | 1.366 | 1.734 |
| | p value | 0.913 | 0.944 | 0.367 | 0.657 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 10,974 | 8,652 | 13,648 | 7,367 |
| School Days Missed last Month | Coefficient | -0.055 | -0.179 | 0.001 | 0.020 |
| | Error | 0.086 | 0.100 | 0.076 | 0.074 |
| | p value | 0.523 | 0.075 * | 0.986 | 0.792 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 9,024 | 5,491 | 10,476 | 5,364 |

The notes from Table 49 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 51: Impact of Transfer Programs on Schooling**

| Follow-up Round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Enrolled in and Attending School | Odds ratio | 0.950 | 0.871 | 1.324 | 1.182 |
| | Error | 0.135 | 0.147 | 0.162 | 0.205 |
| | p value | 0.716 | 0.414 | 0.022 ** | 0.336 |
| | q value | 1.000 | 1.000 | 0.537 | 1.000 |
| | N | 7,760 | 5,097 | 9,818 | 5,212 |
| Repeated Year | Odds ratio | 0.959 | 0.977 | 0.878 | 0.882 |
| | Error | 0.104 | 0.105 | 0.090 | 0.107 |
| | p value | 0.697 | 0.830 | 0.202 | 0.300 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 6,497 | 4,081 | 7,710 | 3,971 |
| Days worked last Month | Coefficient | -1.334 | -1.139 | 0.498 | -0.688 |
| | Error | 3.559 | 3.386 | 2.147 | 2.401 |
| | p value | 0.708 | 0.737 | 0.817 | 0.775 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 7,889 | 6,192 | 9,819 | 5,291 |
| School Days Missed last Month | Coefficient | -0.095 | -0.179 | -0.227 | -0.062 |
| | Error | 0.124 | 0.137 | 0.156 | 0.178 |
| | p value | 0.443 | 0.193 | 0.147 | 0.729 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 6,502 | 3,973 | 7,573 | 3,868 |

The notes from Table 49 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 52: Impact of Savings Component on Schooling**

| Follow-up Round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Enrolled in and Attending School | Odds ratio | 1.001 | 1.095 | 0.959 | 0.953 |
| | Error | 0.132 | 0.160 | 0.108 | 0.170 |
| | p value | 0.994 | 0.536 | 0.713 | 0.788 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,042 | 3,285 | 5,070 | 2,720 |
| Repeated Year | Odds ratio | 0.948 | 0.879 | 0.993 | 1.043 |
| | Error | 0.123 | 0.103 | 0.118 | 0.139 |
| | p value | 0.681 | 0.271 | 0.955 | 0.753 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,367 | 2,110 | 3,976 | 2,049 |
| Days worked last Month | Coefficient | 1.483 | 2.639 | -0.708 | -0.754 |
| | Error | 4.124 | 3.072 | 2.471 | 2.823 |
| | p value | 0.720 | 0.394 | 0.776 | 0.790 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,129 | 3,415 | 5,071 | 3,275 |
| School Days Missed last Month | Coefficient | -0.107 | -0.078 | -0.195 | -0.198 |
| | Error | 0.155 | 0.156 | 0.154 | 0.132 |
| | p value | 0.493 | 0.621 | 0.212 | 0.138 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,367 | 2,529 | 3,913 | 2,348 |

The notes from Table 49 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 53: Impact of Behavioral Component on Schooling**

| Follow-up Round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Enrolled in and Attending School | Odds ratio | 0.856 | 0.701 | 1.020 | 1.289 |
| | Error | 0.144 | 0.217 | 0.153 | 0.401 |
| | p value | 0.354 | 0.251 | 0.896 | 0.414 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,016 | 580 | 1,242 | 557 |
| Repeated Year | Odds ratio | 1.056 | 1.102 | 0.912 | 0.802 |
| | Error | 0.154 | 0.209 | 0.123 | 0.161 |
| | p value | 0.710 | 0.607 | 0.494 | 0.273 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 841 | 616 | 1,024 | 605 |
| Days worked last Month | Coefficient | -5.173 | -4.556 | -0.860 | 0.298 |
| | Error | 2.744 | 2.835 | 2.309 | 3.308 |
| | p value | 0.060 * | 0.108 | 0.710 | 0.928 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,044 | 816 | 1,242 | 666 |
| School Days Missed last Month | Coefficient | -0.103 | 0.136 | -0.202 | -0.192 |
| | Error | 0.184 | 0.222 | 0.151 | 0.202 |
| | p value | 0.576 | 0.540 | 0.181 | 0.342 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 845 | 600 | 1,010 | 603 |

The notes from Table 49 apply, except as follows: Specification 2 uses the preferred model for so-called clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 54: Impact of Microenterprise over Transfer Programs on Schooling**

| Follow-up Round | | First Follow-up | | Second Follow-up | |
|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Enrolled in and Attending School | Odds ratio | 1.009 | 1.054 | 0.779 | 0.846 |
| | Error | 0.159 | 0.187 | 0.104 | 0.159 |
| | p value | 0.952 | 0.767 | 0.062 * | 0.373 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 5,058 | 3,237 | 6,312 | 3,380 |
| Repeated Year | Odds ratio | 1.052 | 1.052 | 1.082 | 1.137 |
| | Error | 0.128 | 0.118 | 0.121 | 0.148 |
| | p value | 0.679 | 0.649 | 0.480 | 0.325 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,208 | 2,626 | 5,000 | 2,490 |
| Days worked last Month | Coefficient | 1.526 | 1.261 | 0.739 | 1.619 |
| | Error | 4.188 | 3.729 | 2.374 | 2.745 |
| | p value | 0.716 | 0.736 | 0.756 | 0.557 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 5,173 | 4,092 | 6,313 | 4,067 |
| School Days Missed last Month | Coefficient | 0.040 | 0.078 | 0.229 | 0.140 |
| | Error | 0.140 | 0.145 | 0.170 | 0.161 |
| | p value | 0.774 | 0.591 | 0.182 | 0.387 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,212 | 3,047 | 4,923 | 3,022 |

The notes from Table 49 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set D2∪D3.

**Table 55: Impact of Spillovers on Financial Position**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | -381 | -839 | -787 | -913 | -510 | -876 |
| | Error | 749 | 655 | 1,057 | 977 | 831 | 719 |
| | p value | 0.612 | 0.202 | 0.458 | 0.352 | 0.541 | 0.225 |
| | q value | 0.597 | 0.288 | 0.540 | 0.426 | 0.546 | 0.300 |
| | N | 3,097 | 2,461 | 3,028 | 2,396 | 3,188 | 2,529 |
| Loans | Coefficient | -1,124 | -1,131 | -1,895 | -1,801 | -1,464 | -1,455 |
| | Error | 727 | 624 | 963 | 887 | 744 | 672 |
| | p value | 0.124 | 0.072 * | 0.051 * | 0.044 ** | 0.051 * | 0.032 ** |
| | q value | 0.220 | 0.160 | 0.145 | 0.137 | 0.145 | 0.121 |
| | N | 3,097 | 3,090 | 3,028 | 3,021 | 3,188 | 3,181 |

Notes: Coefficients are reported in current UGX per capita. Specification 1 uses simple linear regression. Specification 2 uses the preferred model for so-called clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. Errors are adjusted for cluster robustness. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. Errors are adjusted for cluster robustness. Loans are defined as the estimated outstanding value.

**Table 56: Impact of Microenterprise Programs on Financial Position**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | 1,903 | 1,860 | 1,652 | 1,914 | 1,830 | 1,889 |
| | Error | 483 | 509 | 658 | 661 | 500 | 508 |
| | p value | 0.000 *** | 0.000 *** | 0.013 ** | 0.004 *** | 0.000 *** | 0.000 *** |
| | q value | 0.006 *** | 0.006 *** | 0.066 * | 0.030 ** | 0.006 *** | 0.006 *** |
| | N | 4,916 | 3,901 | 4,811 | 3,815 | 5,073 | 4,021 |
| Loans | Coefficient | 1,370 | 1,148 | -9 | -61 | 645 | 517 |
| | Error | 531 | 543 | 465 | 513 | 432 | 448 |
| | p value | 0.011 ** | 0.036 ** | 0.984 | 0.905 | 0.137 | 0.250 |
| | q value | 0.059 * | 0.123 | 0.670 | 0.632 | 0.229 | 0.322 |
| | N | 4,916 | 3,901 | 4,811 | 3,815 | 5,073 | 4,021 |

The notes from Table 55 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 57: Impact of Transfer Programs on Financial Position**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | 1,431 | 1,499 | 2,227 | 2,392 | 1,811 | 1,887 |
| | Error | 1,190 | 1,090 | 1,504 | 1,451 | 1,208 | 1,085 |
| | p value | 0.231 | 0.171 | 0.141 | 0.101 | 0.136 | 0.084 * |
| | q value | 0.301 | 0.265 | 0.229 | 0.198 | 0.229 | 0.180 |
| | N | 3,560 | 2,840 | 3,481 | 2,764 | 3,661 | 2,916 |
| Loans | Coefficient | -1,170 | -2,013 | -821 | -1,369 | -939 | -1,648 |
| | Error | 529 | 543 | 618 | 670 | 485 | 491 |
| | p value | 0.029 ** | 0.000 *** | 0.186 | 0.043 ** | 0.055 * | 0.001 *** |
| | q value | 0.114 | 0.006 *** | 0.268 | 0.137 | 0.148 | 0.009 *** |
| | N | 3,560 | 2,840 | 3,481 | 2,764 | 3,661 | 2,916 |

The notes from Table 55 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 58: Impact of Savings Component on Financial Position**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | 67 | 124 | 885 | 1,019 | 421 | 497 |
| | Error | 1,082 | 944 | 1,414 | 1,123 | 1,150 | 896 |
| | p value | 0.951 | 0.896 | 0.534 | 0.368 | 0.716 | 0.581 |
| | q value | 0.666 | 0.632 | 0.546 | 0.437 | 0.620 | 0.577 |
| | N | 1,819 | 1,746 | 1,783 | 1,783 | 1,885 | 1,812 |
| Loans | Coefficient | 294 | 409 | -288 | -86 | -5 | 144 |
| | Error | 1,106 | 944 | 771 | 687 | 844 | 716 |
| | p value | 0.791 | 0.667 | 0.710 | 0.901 | 0.995 | 0.841 |
| | q value | 0.629 | 0.620 | 0.620 | 0.632 | 0.670 | 0.632 |
| | N | 1,819 | 1,819 | 1,783 | 1,783 | 1,885 | 1,885 |

The notes from Table 55 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 59: Impact of Behavioral Component on Financial Position**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | -2,838 | -2,670 | 829 | 1,294 | -1,089 | -871 |
| | Error | 1,488 | 1,449 | 2,130 | 2,055 | 1,502 | 1,443 |
| | p value | 0.057 * | 0.066 * | 0.697 | 0.529 | 0.469 | 0.547 |
| | q value | 0.148 | 0.157 | 0.620 | 0.546 | 0.543 | 0.546 |
| | N | 463 | 462 | 453 | 452 | 473 | 472 |
| Loans | Coefficient | -1,784 | -1,923 | -1,509 | -1,662 | -1,665 | -1,937 |
| | Error | 844 | 814 | 1,209 | 1,156 | 884 | 830 |
| | p value | 0.035 ** | 0.019 ** | 0.213 | 0.151 | 0.060 * | 0.020 ** |
| | q value | 0.123 | 0.086 * | 0.297 | 0.236 | 0.150 | 0.086 * |
| | N | 463 | 463 | 453 | 452 | 473 | 472 |

The notes from Table 55 apply, except as follows: Specification 2 uses the preferred model for so-called non-lustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 60: Impact of Microenterprise over Transfer Programs on Financial Position**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | 472 | 728 | -575 | -351 | 20 | 402 |
| | Error | 1,284 | 1,042 | 1,668 | 1,452 | 1,321 | 1,014 |
| | p value | 0.714 | 0.486 | 0.731 | 0.809 | 0.988 | 0.693 |
| | q value | 0.620 | 0.546 | 0.620 | 0.629 | 0.670 | 0.620 |
| | N | 2,282 | 2,278 | 2,236 | 1,787 | 2,358 | 2,354 |
| Loans | Coefficient | 2,540 | 2,379 | 812 | 833 | 1,584 | 1,603 |
| | Error | 697 | 660 | 661 | 621 | 583 | 546 |
| | p value | 0.000 *** | 0.001 *** | 0.222 | 0.183 | 0.008 *** | 0.004 *** |
| | q value | 0.006 *** | 0.006 *** | 0.300 | 0.268 | 0.046 ** | 0.030 ** |
| | N | 2,282 | 2,278 | 2,236 | 2,145 | 2,358 | 2,354 |

The notes from Table 55 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 61: Impact of Spillovers on Financial Position (Log Dependent)**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | -0.435 | -0.603 | -0.077 | -0.197 | -0.225 | -0.437 |
| | Error | 0.301 | 0.291 | 0.293 | 0.292 | 0.291 | 0.278 |
| | p value | 0.355 | 0.077 * | 0.551 | 0.380 | 0.522 | 0.144 |
| | q value | 0.505 | 0.224 | 0.566 | 0.505 | 0.558 | 0.301 |
| | N | 3,097 | 2,461 | 3,028 | 2,396 | 3,188 | 2,529 |
| Loans | Coefficient | -0.224 | -0.239 | -0.605 | -0.611 | -0.484 | -0.501 |
| | Error | 0.247 | 0.222 | 0.235 | 0.230 | 0.250 | 0.243 |
| | p value | 0.196 | 0.120 | 0.019 ** | 0.014 ** | 0.046 ** | 0.027 ** |
| | q value | 0.367 | 0.301 | 0.135 | 0.115 | 0.179 | 0.152 |
| | N | 3,097 | 3,090 | 3,028 | 3,021 | 3,188 | 3,181 |

The notes from Table 55 apply, except that the dependent variable is defined as $ln(y_{ijF} + c_{y_F})$. Constant $c_{y_F}$ serves to assure that all observations can be defined in logarithmic terms. Where the value of the lower winsorization cutoff for a given outcome $y$ in a given outcome round $F$ is above zero, $c_{y_F}$ is defined as zero. Where the cutoff value is equal to or below zero, $c_{y_F}$ is defined as the absolute value of the cutoff, plus UGX 1.

**Table 62: Impact of Microenterprise Programs on Financial Position (Log Dependent)**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | 0.786 | 0.790 | 0.861 | 0.881 | 0.863 | 0.856 |
| | Error | 0.181 | 0.191 | 0.176 | 0.180 | 0.173 | 0.178 |
| | p value | 0.000 *** | 0.000 *** | 0.001 *** | 0.000 *** | 0.000 *** | 0.000 *** |
| | q value | 0.001 *** | 0.001 *** | 0.008 *** | 0.003 *** | 0.001 *** | 0.001 *** |
| | N | 4,916 | 3,901 | 4,811 | 3,815 | 5,073 | 4,021 |
| Loans | Coefficient | 0.079 | -0.068 | 0.082 | -0.037 | 0.097 | -0.054 |
| | Error | 0.163 | 0.173 | 0.150 | 0.168 | 0.158 | 0.171 |
| | p value | 0.066 * | 0.246 | 0.631 | 0.847 | 0.159 | 0.373 |
| | q value | 0.217 | 0.393 | 0.620 | 0.699 | 0.314 | 0.505 |
| | N | 4,916 | 3,901 | 4,811 | 3,815 | 5,073 | 4,021 |

The notes from Table 61 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 63: Impact of Transfer Programs on Financial Position (Log Dependent)**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | 0.637 | 0.611 | 0.214 | 0.270 | 0.561 | 0.527 |
| | Error | 0.313 | 0.309 | 0.321 | 0.302 | 0.278 | 0.245 |
| | p value | 0.184 | 0.137 | 0.228 | 0.136 | 0.141 | 0.069 * |
| | q value | 0.358 | 0.301 | 0.379 | 0.301 | 0.301 | 0.217 |
| | N | 3,560 | 2,840 | 3,481 | 2,764 | 3,661 | 2,916 |
| Loans | Coefficient | -0.207 | -0.475 | -0.194 | -0.335 | -0.291 | -0.544 |
| | Error | 0.284 | 0.303 | 0.278 | 0.295 | 0.298 | 0.313 |
| | p value | 0.144 | 0.005 *** | 0.570 | 0.213 | 0.206 | 0.011 ** |
| | q value | 0.301 | 0.045 ** | 0.582 | 0.367 | 0.367 | 0.101 |
| | N | 3,560 | 2,840 | 3,481 | 2,764 | 3,661 | 2,916 |

The notes from Table 61 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 64: Impact of Savings Component on Financial Position (Log Dependent)**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Savings | Coefficient | 0.627 | 0.724 | 0.630 | 0.693 | 0.688 | 0.792 |
| | Error | 0.342 | 0.322 | 0.390 | 0.339 | 0.361 | 0.327 |
| | p value | 0.459 | 0.347 | 0.362 | 0.215 | 0.370 | 0.217 |
| | q value | 0.506 | 0.505 | 0.505 | 0.367 | 0.505 | 0.367 |
| | N | 1,819 | 1,746 | 1,783 | 1,783 | 1,885 | 1,812 |
| Loans | Coefficient | 0.143 | 0.182 | 0.361 | 0.419 | 0.400 | 0.454 |
| | Error | 0.320 | 0.303 | 0.285 | 0.286 | 0.311 | 0.298 |
| | p value | 0.758 | 0.631 | 0.583 | 0.388 | 0.725 | 0.537 |
| | q value | 0.699 | 0.620 | 0.588 | 0.505 | 0.699 | 0.558 |
| | N | 1,819 | 1,819 | 1,783 | 1,783 | 1,885 | 1,885 |

The notes from Table 61 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 65: Impact of Behavioral Component on Financial Position (Log Dependent)**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| | Coefficient | -0.548 | -0.518 | 0.349 | 0.450 | 0.373 | 0.393 |
| | Error | 0.392 | 0.390 | 0.416 | 0.406 | 0.381 | 0.377 |
| Savings | p value | 0.068 * | 0.076 * | 0.387 | 0.255 | 0.814 | 0.883 |
| | q value | 0.217 | 0.224 | 0.505 | 0.401 | 0.699 | 0.716 |
| | N | 463 | 462 | 453 | 452 | 473 | 472 |
| | Coefficient | -0.686 | -0.734 | -0.351 | -0.348 | -0.556 | -0.642 |
| | Error | 0.350 | 0.343 | 0.388 | 0.374 | 0.379 | 0.367 |
| Loans | p value | 0.044 ** | 0.025 ** | 0.319 | 0.235 | 0.092 * | 0.034 ** |
| | q value | 0.179 | 0.150 | 0.498 | 0.382 | 0.247 | 0.164 |
| | N | 463 | 463 | 453 | 452 | 473 | 472 |

The notes from Table 61 apply, except as follows: Specification 2 uses the preferred model for so-called non-lustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 66: Impact of Microenterprise over Transfer Programs on Financial Position (Log Dependent)**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| | Coefficient | 0.149 | 0.214 | 0.647 | 0.633 | 0.302 | 0.383 |
| | Error | 0.358 | 0.323 | 0.366 | 0.326 | 0.326 | 0.281 |
| Savings | p value | 0.480 | 0.262 | 0.670 | 0.533 | 0.599 | 0.273 |
| | q value | 0.527 | 0.402 | 0.668 | 0.558 | 0.599 | 0.413 |
| | N | 2,282 | 2,278 | 2,236 | 1,787 | 2,358 | 2,354 |
| | Coefficient | 0.285 | 0.226 | 0.276 | 0.226 | 0.388 | 0.368 |
| | Error | 0.318 | 0.313 | 0.306 | 0.302 | 0.329 | 0.316 |
| Loans | p value | 0.023 ** | 0.034 ** | 0.432 | 0.436 | 0.056 * | 0.041 ** |
| | q value | 0.150 | 0.164 | 0.505 | 0.505 | 0.204 | 0.178 |
| | N | 2,282 | 2,278 | 2,236 | 2,145 | 2,358 | 2,354 |

The notes from Table 61 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 67: Impact of Spillovers on Health Related Outcomes**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Serious Illnesses | Coefficient | 0.004 | 0.004 | 0.030 | 0.032 | 0.017 | 0.018 |
| | Error | 0.050 | 0.049 | 0.036 | 0.028 | 0.034 | 0.031 |
| | p value | 0.930 | 0.931 | 0.401 | 0.264 | 0.613 | 0.554 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,324 | 3,221 | 3,324 | 3,221 | 3,324 | 3,221 |
| Clinic Visits | Coefficient | 0.003 | 0.005 | 0.028 | 0.031 | 0.005 | 0.014 |
| | Error | 0.055 | 0.055 | 0.029 | 0.038 | 0.035 | 0.034 |
| | p value | 0.960 | 0.922 | 0.339 | 0.422 | 0.883 | 0.675 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,410 | 946 | 1,491 | 1,033 | 2,142 | 1,437 |
| Child Deaths | Coefficient | 0.006 | 0.014 | 0.005 | 0.004 | 0.005 | 0.011 |
| | Error | 0.008 | 0.007 | 0.006 | 0.006 | 0.005 | 0.005 |
| | p value | 0.479 | 0.055 * | 0.459 | 0.459 | 0.323 | 0.032 ** |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,087 | 2,457 | 3,023 | 3,016 | 3,188 | 2,529 |
| Preventative Clinic Visits for Children | Coefficient | -0.021 | -0.018 | -0.032 | -0.027 | -0.028 | -0.023 |
| | Error | 0.023 | 0.019 | 0.025 | 0.021 | 0.021 | 0.016 |
| | p value | 0.351 | 0.343 | 0.208 | 0.205 | 0.190 | 0.171 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,097 | 3,004 | 3,028 | 2,941 | 3,188 | 3,094 |
| Ideal Number of Children | Coefficient | 0.015 | 0.028 | 0.002 | 0.030 | 0.016 | 0.044 |
| | Error | 0.139 | 0.106 | 0.137 | 0.120 | 0.121 | 0.090 |
| | p value | 0.912 | 0.790 | 0.986 | 0.802 | 0.898 | 0.626 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,079 | 2,389 | 2,995 | 2,319 | 3,186 | 2,470 |
| Pregnancies | Coefficient | -0.014 | -0.012 | 0.010 | 0.013 | -0.006 | -0.004 |
| | Error | 0.016 | 0.015 | 0.014 | 0.014 | 0.012 | 0.011 |
| | p value | 0.387 | 0.441 | 0.472 | 0.327 | 0.617 | 0.747 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,097 | 3,004 | 3,022 | 2,935 | 3,187 | 3,093 |

Notes: Specification 1 uses simple linear regression. Specification 2 uses the preferred model for so-called clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. Errors are adjusted for cluster robustness. Intent-to-treat assignment T is coded to the value one among households in set A1∪B1∪C1∪D1 and to the value zero in set E1. Errors are adjusted for cluster robustness.

**Table 68: Impact of Microenterprise Programs on Health Related Outcomes**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Serious Illnesses | Coefficient | -0.011 | -0.033 | 0.019 | 0.013 | 0.004 | 0.001 |
| | Error | 0.024 | 0.025 | 0.022 | 0.019 | 0.018 | 0.016 |
| | p value | 0.645 | 0.178 | 0.395 | 0.495 | 0.823 | 0.967 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 5,294 | 4,083 | 5,294 | 5,117 | 5,294 | 5,117 |
| Clinic Visits | Coefficient | -0.016 | -0.028 | 0.008 | 0.003 | -0.010 | -0.019 |
| | Error | 0.029 | 0.037 | 0.021 | 0.026 | 0.017 | 0.021 |
| | p value | 0.599 | 0.446 | 0.709 | 0.913 | 0.575 | 0.367 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,236 | 1,516 | 2,393 | 1,614 | 3,441 | 2,247 |
| Child Deaths | Coefficient | -0.004 | -0.007 | -0.003 | -0.003 | -0.003 | -0.003 |
| | Error | 0.005 | 0.005 | 0.005 | 0.005 | 0.003 | 0.003 |
| | p value | 0.396 | 0.175 | 0.567 | 0.573 | 0.367 | 0.377 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,903 | 3,896 | 4,802 | 4,792 | 5,073 | 4,906 |
| Preventative Clinic Visits for Children | Coefficient | -0.002 | -0.011 | -0.012 | -0.024 | -0.009 | -0.018 |
| | Error | 0.015 | 0.014 | 0.015 | 0.013 | 0.013 | 0.011 |
| | p value | 0.876 | 0.431 | 0.419 | 0.068 * | 0.493 | 0.103 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,916 | 4,750 | 4,811 | 4,655 | 5,073 | 4,906 |
| Ideal Number of Children | Coefficient | -0.003 | 0.030 | -0.016 | 0.037 | -0.010 | 0.037 |
| | Error | 0.082 | 0.083 | 0.079 | 0.078 | 0.071 | 0.070 |
| | p value | 0.973 | 0.722 | 0.843 | 0.639 | 0.887 | 0.601 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,890 | 3,774 | 4,745 | 3,674 | 5,070 | 3,914 |
| Pregnancies | Coefficient | 0.011 | 0.010 | 0.015 | 0.013 | 0.014 | 0.013 |
| | Error | 0.012 | 0.012 | 0.010 | 0.010 | 0.009 | 0.009 |
| | p value | 0.373 | 0.424 | 0.156 | 0.231 | 0.104 | 0.124 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 4,916 | 4,750 | 4,802 | 4,646 | 5,072 | 4,905 |

The notes from Table 67 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 69: Impact of Transfer Programs on Health Related Outcomes**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Serious Illnesses | Coefficient | 0.060 | 0.044 | 0.042 | 0.040 | 0.051 | 0.050 |
| | Error | 0.044 | 0.046 | 0.043 | 0.036 | 0.035 | 0.031 |
| | p value | 0.179 | 0.338 | 0.331 | 0.272 | 0.151 | 0.115 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,804 | 2,959 | 3,804 | 3,679 | 3,804 | 3,679 |
| Clinic Visits | Coefficient | 0.018 | 0.057 | 0.021 | 0.009 | 0.022 | 0.022 |
| | Error | 0.045 | 0.058 | 0.043 | 0.044 | 0.035 | 0.041 |
| | p value | 0.684 | 0.328 | 0.630 | 0.829 | 0.524 | 0.592 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,627 | 1,067 | 1,725 | 1,148 | 2,478 | 1,594 |
| Child Deaths | Coefficient | -0.010 | -0.010 | -0.005 | -0.005 | -0.007 | -0.006 |
| | Error | 0.009 | 0.010 | 0.007 | 0.007 | 0.007 | 0.007 |
| | p value | 0.277 | 0.309 | 0.460 | 0.481 | 0.298 | 0.389 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,549 | 2,836 | 3,475 | 3,467 | 3,661 | 3,545 |
| Preventative Clinic Visits for Children | Coefficient | -0.010 | -0.012 | -0.018 | -0.018 | -0.014 | -0.014 |
| | Error | 0.026 | 0.026 | 0.024 | 0.021 | 0.021 | 0.020 |
| | p value | 0.703 | 0.653 | 0.461 | 0.398 | 0.512 | 0.474 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,560 | 3,446 | 3,481 | 3,372 | 3,661 | 3,545 |
| Ideal Number of Children | Coefficient | -0.120 | -0.054 | 0.017 | 0.036 | -0.027 | 0.006 |
| | Error | 0.168 | 0.164 | 0.135 | 0.140 | 0.134 | 0.127 |
| | p value | 0.478 | 0.744 | 0.900 | 0.796 | 0.838 | 0.963 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,538 | 2,756 | 3,444 | 2,676 | 3,659 | 2,848 |
| Pregnancies | Coefficient | 0.002 | 0.007 | 0.008 | 0.009 | 0.004 | 0.006 |
| | Error | 0.017 | 0.016 | 0.024 | 0.025 | 0.014 | 0.013 |
| | p value | 0.902 | 0.661 | 0.736 | 0.704 | 0.769 | 0.620 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 3,560 | 3,446 | 3,475 | 3,366 | 3,660 | 3,544 |

The notes from Table 67 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set D2∪D3 and to the value zero in set A1∪B1∪C1∪D1∪E1.

**Table 70: Impact of Savings Component on Health Related Outcomes**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Serious Illnesses | Coefficient | -0.051 | -0.048 | 0.012 | 0.020 | -0.020 | -0.014 |
| | Error | 0.045 | 0.041 | 0.051 | 0.038 | 0.041 | 0.031 |
| | p value | 0.259 | 0.249 | 0.819 | 0.592 | 0.633 | 0.654 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,970 | 1,896 | 1,970 | 1,896 | 1,970 | 1,896 |
| Clinic Visits | Coefficient | -0.113 | -0.154 | -0.021 | -0.018 | -0.054 | -0.071 |
| | Error | 0.046 | 0.072 | 0.039 | 0.044 | 0.028 | 0.042 |
| | p value | 0.017 ** | 0.036 ** | 0.587 | 0.694 | 0.065 * | 0.093 * |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 826 | 425 | 902 | 605 | 1,299 | 648 |
| Child Deaths | Coefficient | -0.003 | -0.002 | -0.003 | -0.001 | -0.005 | -0.005 |
| | Error | 0.010 | 0.009 | 0.007 | 0.008 | 0.007 | 0.007 |
| | p value | 0.782 | 0.813 | 0.672 | 0.866 | 0.516 | 0.496 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,816 | 1,813 | 1,779 | 1,710 | 1,885 | 1,882 |
| Preventative Clinic Visits for Children | Coefficient | -0.018 | 0.005 | 0.015 | 0.028 | -0.001 | 0.017 |
| | Error | 0.029 | 0.024 | 0.026 | 0.023 | 0.023 | 0.019 |
| | p value | 0.541 | 0.838 | 0.577 | 0.235 | 0.952 | 0.396 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,819 | 1,746 | 1,783 | 1,714 | 1,885 | 1,812 |
| Ideal Number of Children | Coefficient | 0.223 | 0.254 | 0.146 | 0.130 | 0.174 | 0.182 |
| | Error | 0.171 | 0.129 | 0.167 | 0.115 | 0.161 | 0.106 |
| | p value | 0.198 | 0.052 * | 0.385 | 0.263 | 0.283 | 0.091 * |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,811 | 1,738 | 1,750 | 1,685 | 1,884 | 1,811 |
| Pregnancies | Coefficient | -0.005 | -0.007 | -0.002 | 0.000 | -0.005 | -0.003 |
| | Error | 0.020 | 0.021 | 0.020 | 0.019 | 0.018 | 0.018 |
| | p value | 0.820 | 0.737 | 0.928 | 0.979 | 0.771 | 0.866 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,819 | 1,816 | 1,780 | 1,711 | 1,885 | 1,812 |

The notes from Table 67 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

**Table 71: Impact of Behavioral Component on Health Related Outcomes**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Serious Illnesses | Coefficient | 0.015 | -0.034 | 0.022 | 0.038 | 0.018 | 0.026 |
| | Error | 0.072 | 0.080 | 0.060 | 0.066 | 0.048 | 0.046 |
| | p value | 0.839 | 0.669 | 0.709 | 0.564 | 0.697 | 0.571 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 480 | 393 | 480 | 393 | 480 | 479 |
| Clinic Visits | Coefficient | 0.035 | 0.009 | 0.090 | 0.095 | 0.070 | 0.050 |
| | Error | 0.101 | 0.192 | 0.063 | 0.118 | 0.060 | 0.109 |
| | p value | 0.726 | 0.962 | 0.151 | 0.421 | 0.244 | 0.643 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 217 | 121 | 234 | 110 | 336 | 157 |
| Child Deaths | Coefficient | -0.017 | -0.014 | -0.008 | -0.004 | -0.014 | -0.009 |
| | Error | 0.015 | 0.015 | 0.012 | 0.015 | 0.011 | 0.012 |
| | p value | 0.255 | 0.360 | 0.508 | 0.779 | 0.185 | 0.463 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 462 | 441 | 452 | 367 | 473 | 387 |
| Preventative Clinic Visits for Children | Coefficient | -0.031 | 0.049 | -0.018 | 0.020 | -0.018 | 0.015 |
| | Error | 0.046 | 0.048 | 0.047 | 0.045 | 0.038 | 0.035 |
| | p value | 0.501 | 0.312 | 0.702 | 0.654 | 0.643 | 0.677 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 463 | 370 | 453 | 431 | 473 | 451 |
| Ideal Number of Children | Coefficient | -0.236 | -0.123 | -0.391 | -0.294 | -0.304 | -0.190 |
| | Error | 0.238 | 0.229 | 0.224 | 0.215 | 0.199 | 0.186 |
| | p value | 0.322 | 0.591 | 0.081 * | 0.173 | 0.126 | 0.307 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 459 | 438 | 449 | 427 | 473 | 451 |
| Pregnancies | Coefficient | 0.011 | 0.009 | -0.036 | -0.037 | -0.012 | -0.013 |
| | Error | 0.030 | 0.029 | 0.044 | 0.044 | 0.025 | 0.025 |
| | p value | 0.711 | 0.757 | 0.407 | 0.400 | 0.622 | 0.617 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 463 | 462 | 453 | 452 | 473 | 472 |

The notes from Table 67 apply, except as follows: Specification 2 uses the preferred model for so-called clustered comparisons. Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2. Errors are not adjusted for cluster robustness.

**Table 72: Impact of Microenterprise over Transfer Programs on Health Related Outcomes**

| Follow-up Round | | First Follow-up | | Second Follow-up | | Pooled Follow-ups | |
|---|---|---|---|---|---|---|---|
| | | Specification 1 | Specification 2 | Specification 1 | Specification 2 | Specification 1 | Specification 2 |
| Serious Illnesses | Coefficient | -0.071 | -0.077 | -0.023 | -0.032 | -0.047 | -0.057 |
| | Error | 0.048 | 0.046 | 0.049 | 0.041 | 0.039 | 0.035 |
| | p value | 0.142 | 0.098 * | 0.642 | 0.438 | 0.236 | 0.105 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,450 | 2,446 | 2,450 | 2,446 | 2,450 | 2,446 |
| Clinic Visits | Coefficient | -0.034 | -0.081 | -0.013 | -0.006 | -0.032 | -0.042 |
| | Error | 0.048 | 0.084 | 0.044 | 0.044 | 0.035 | 0.040 |
| | p value | 0.482 | 0.341 | 0.768 | 0.895 | 0.368 | 0.292 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 1,043 | 522 | 1,136 | 744 | 1,635 | 1,070 |
| Child Deaths | Coefficient | 0.006 | 0.005 | 0.003 | 0.004 | 0.004 | 0.003 |
| | Error | 0.010 | 0.010 | 0.007 | 0.008 | 0.007 | 0.007 |
| | p value | 0.549 | 0.636 | 0.731 | 0.603 | 0.584 | 0.693 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,278 | 2,278 | 2,231 | 1,731 | 2,358 | 2,263 |
| Preventative Clinic Visits for Children | Coefficient | 0.007 | -0.002 | 0.006 | -0.006 | 0.005 | 0.003 |
| | Error | 0.028 | 0.030 | 0.026 | 0.023 | 0.023 | 0.022 |
| | p value | 0.795 | 0.943 | 0.821 | 0.798 | 0.827 | 0.907 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,282 | 1,763 | 2,236 | 2,145 | 2,358 | 1,823 |
| Ideal Number of Children | Coefficient | 0.117 | 0.084 | -0.033 | -0.049 | 0.017 | 0.036 |
| | Error | 0.188 | 0.178 | 0.149 | 0.124 | 0.154 | 0.135 |
| | p value | 0.535 | 0.640 | 0.827 | 0.695 | 0.911 | 0.788 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,270 | 1,752 | 2,199 | 2,112 | 2,357 | 1,822 |
| Pregnancies | Coefficient | 0.009 | 0.008 | 0.007 | 0.023 | 0.010 | 0.018 |
| | Error | 0.019 | 0.019 | 0.025 | 0.024 | 0.015 | 0.015 |
| | p value | 0.652 | 0.654 | 0.796 | 0.346 | 0.506 | 0.243 |
| | q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | N | 2,282 | 2,278 | 2,233 | 1,785 | 2,358 | 1,879 |

The notes from Table 67 apply, except as follows: Intent-to-treat assignment T is coded to the value one among households in set A2∪B2 and to the value zero in set A1∪B1∪C1∪D1∪E1.

## CHAPTER 2. CASH, COGNITION, AND SCHEMA ADOPTION


### *ABSTRACT*

The theory of scarcity (as defined in behavioral economics) holds that poverty imposes cognitive load by drawing attention to urgent poverty-related concerns. Cognitive load theory (as defined in educational psychology) holds that cognitive load impedes schema adoption. Taken jointly and in sequence, the two theories suggest that cash inflows should have an enabling effect on schema adoption among the poor. We test this hypothesis using a randomized trial, and we end up rejecting it. Our results challenge scarcity theory, as we detect no cognitive impacts from cash inflows in the first place.


### *DISCLOSURES*

This trial has been registered and a pre-analysis plan lodged.[24] All procedures in this paper follow this plan, except where otherwise noted. Additional study materials, including study data, will be made available online.[25] The data are shared with another trial.[26]

### *INTRODUCTION*

If poverty causes conditions that in turn cause poverty, individuals and societies may be "trapped" in it (Azariadis & Stachurski, 2005). Some such reinforcement mechanisms are behavioral in nature (Kraay & McKenzie, 2014). One of them is described by the theory of *scarcity,* as defined by Mullainathan & Shafir (2013). It suggests that poverty impedes cognitive performance – not only via physical stressors such as malnutrition, heat, noise pollution, and associated discomfort, disease, or sleep deprivation (Boswell Dean, Schilbach, & Schofield, 2017) – but also more directly by drawing the flow of cognitive resources towards urgent poverty-related concerns and thereby away from other potential objects of attention. Working with Indian sugarcane farmers, Shah, Mullainathan, Shafir, & Zhao (2013) demonstrate that cognitive performance (as measured by fluid intelligence and cognitive control) increased substantially in the immediate aftermath of harvest. Wealth in the broad sense of "economic prospects" or "net worth" was not

[24] socialscienceregistry.org/trials/3737
[25] osf.io/uwmjn
[26] socialscienceregistry.org/trials/996

technically affected by the harvest; it would be less precise to say that harvest made farmers richer than that it helped them transform an illiquid asset into a liquid one. But in the terminology of Mullainathan & Shafir (2013), farmers experienced temporary relief from the psychological burden of scarcity: they shed the cognitive load created by poverty. The theory implies that cash inflows should more generally have this effect, as they reduce the urgency of juggling pressing expenses and managing unsteady income.

Independent empirical research on scarcity theory yields mixed results: Carvalho, Meier, & Wang (2016) detect no changes in the cognitive performance of low-income in the Unites States around payday. Meanwhile, Ong, Theseira, & Ng (2019) do establish positive cognitive impacts from a debt relief program.

The expansion of cognitive bandwidth could have important economic repercussions because people may invest their freed-up attention productively: for example, they could use it to absorb new insights, re-think their convictions, and "change their minds". The mind has been described as a constellation of beliefs, theories, ideologies, and categories ("schemas") that construe the world in a simplified manner, thus enabling boundedly rational economic agents to navigate a complex and dynamic environment (Hoff & Stiglitz, 2015; North, 2010; Simon, 1955). People's schematic descriptions of themselves, their surroundings, and their future are known to shape economic outcomes both through their participation in institutions (Collier, 2016; Denzau & North, 1994; Gabaix, 2014) and their agency as individuals (Dweck, 1999; Hall et al., 2014; Hoff & Pandey, 2014; Lybbert & Wydick, 2018; Paunesku et al., 2015; Walton, 2014). Selten's (1998) aspiration adaptation theory defines goals as the quintessential cognitive crutch – as the central tool that boundedly rational economic agents use to avoid the impossible mental task of constantly re-optimizing. Oettingen & Gollwitzer (2010) argue that plans serve a similar purpose – to make executive function easy and quasi-automatic. So schemas may be thought of as a form of human capital.[27] Accordingly, the process of adopting new ones has been equated with the process of learning (Hanna et al., 2014). Distraction is thought to interfere with schema adoption (Sweller, Ayres, & Kalyuga, 2011; Sweller, 1994). We summarize *cognitive load theory* as the principle that schema adoption is cognitively costly and that the fewer cognitive resources people have at their disposal, the more they struggle to update

[27] Another line of economic argumentation defines schemas such as beliefs and aspirations not as capital assets but as elements of utility (see e.g. Akerlof & Kranton, 2000; Wydick, 2018).

their minds. Like scarcity theory, cognitive load theory has scientific challengers: Gilbert (1991) argues that accepting new beliefs is the cognitively easy while the subsequent rejection requires active processing, which implies that minds are more malleable – in other words, people are more persuadable – when cognitive load is high.

To recap: Scarcity theory argues that cash inflows temporarily free up cognitive resources among the poor; cognitive load theory argues that cognitive resources enhance schema adoption. Both theories have some, but not universal, support. If we accept and interpret them in sequence, we arrive at the hypothesis that cash inflows facilitate schema adoption.

***Pilot Evidence***

The above framework can be reconciled with a set of operational research findings that were generated by the microfinance organization BRAC in Uganda throughout 2013. BRAC evaluated the case for engaging Ugandan microcredit customers in goal-setting and plan-making. On the day loan takers arrived to collect their pre-approved disbursements, a convenience sample was invited to participate in a 10-minute exercise. Those who agreed were assigned at random to participate in one of three conditions: no psychological intervention; a psychological intervention administered just before loan disbursement; or the psychological intervention administered in the reverse order (so just after the cash disbursements). The psychological intervention did appear to alter short-term outcomes: treated participants reported a higher sense of control over their lives and there are indications that their cash revenues increased. Conditional on participating in the psychological intervention, both psychological (internal control) and economic (cash revenue) measures appeared more pronounced when the intervention was administered immediately *after*, as opposed to immediately before, the loan disbursement. For more details on this pilot project, consult Appendix V: Pilot Trial. As shown there, the impacts are tenuous, but they warrant closer investigation.

While indebtedness has been shown to increase cognitive load in the long run (Ong et al., 2019), the short-run effects might be reverse because loans are often used for consumption smoothing (Banerjee, Duflo, Glennerster, & Kinnan, 2015). In other words, the Ugandan loan takers might have experienced a similar cognitive boost from short-term cash inflow as the Indian sugarcane farmers in the study by Mani et al.,

(2013). This may have in turn allowed them to "pay more attention" to the goal-setting and plan-making intervention – an activity that is known to tax cognitive resources (Oettingen & Gollwitzer, 2010). So while BRAC's results are highly tentative, they might be explained by combining scarcity theory and cognitive load theory.

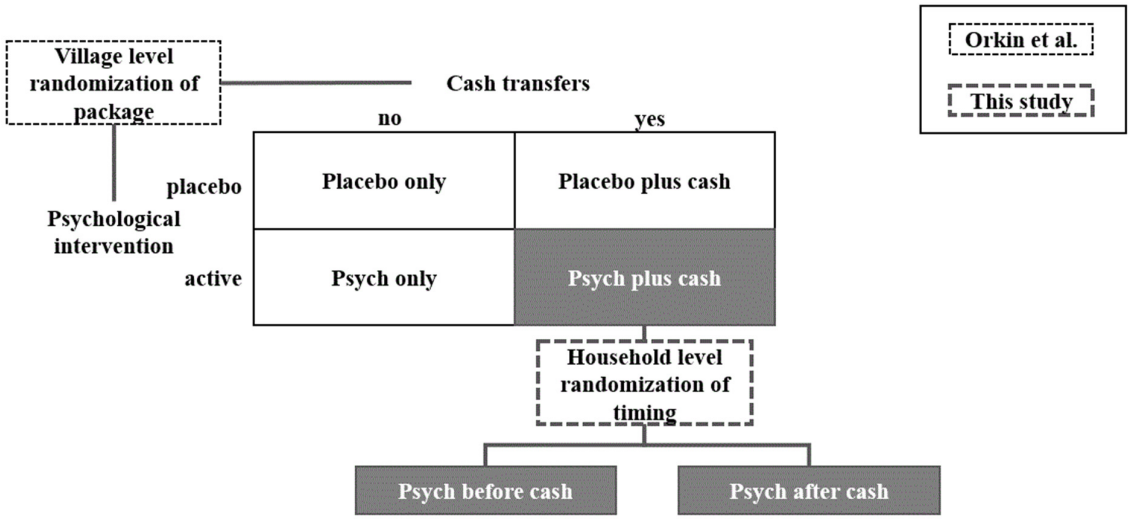
### *STUDY DESIGN*

We once again test the hypothesis that people derive higher economic benefits from goal-setting and plan-making if they engage in it right *after*, as opposed to right before, a cash inflow. We also try to establish if this may in fact be explained through a cognitive channel by assessing if cash inflows enabled participants to shed cognitive load, pay more attention to the psychological intervention, internalize it more deeply, and act upon it more forcefully. While this prediction comes from combining cognitive load theory with scarcity theory, we have the opportunity speak to both individually: if cognitive effects do not materialize, this conflicts with scarcity theory; meanwhile, if cognitive effects do materialize but economic ones don't follow, this conflicts cognitive load theory.[28]

We evaluate this not in the context of loans but cash transfers, whose cognitive effects appear theoretically less ambiguous. A trial by Orkin et al. (forthcoming)[29] crossed a cash transfer intervention and a psychological intervention in the form of a factorial design, with treatment clustered at the village level. In the subset of villages that were assigned to both the cash and psychological intervention, the opportunity presented itself to randomly assign the psychological intervention to take place either just before or just after a major cash flow. The relationship between this study and that by Orkin et al. (forthcoming) is illustrated in Figure 27.

[28] That said, if cognitive effects do not materialize in the first place, we cannot speak to cognitive load theory.
[29] See socialscienceregistry.org/trials/996.

**Figure 27: Study Design in Relation to Orkin et al.**




***Design of Participant Interaction***

The survey and psychological intervention activities were executed by Innovations for Poverty Action (IPA), a nonprofit organization that implements randomized evaluations in low-income countries. IPA informed villages in the Rachuonyo and Siaya regions of Western Kenya of its intention to administer a research study. It then proceeded to conduct an independent census with the dual objective of determining eligibility for the research study[30] and anticipating which households would be independently deemed eligible by GiveDirectly (GD), a nonprofit organization that targets ultra-poor households in the region and provides them with unconditional cash transfers. A random subset of villages was selected to receive cash transfers; the resulting list was provided to GD. GD proceeded to independently determine eligibility through its own census, using somewhat less restrictive criteria than IPA.[31] Eligible households were

[30] IPA inclusion criteria: 1. household's per capita housing space is below 5.8 square meters; 2. household does not have a telephone and the dwelling has a mud floor; 3. household is headed by a widow and the dwelling has a mud floor. IPA exclusion criteria: 1. household is homeless; household does not have any adult female member; household is polygamous; household does not have a complete and seemingly correct GPS coordinate.

[31] GD inclusion criteria: 1. household's per capita housing space is below 6.2 square meters; 2. household does not have a telephone and the dwelling has a mud floor; 3. household is headed by a widow and the dwelling has a mud floor; 4. household is child-headed; 5. household is homeless. GD exclusion criterion: household is composed of a single male. The reasons for the duplication in census activities were twofold: first, IPA needed to determine eligibility in villages that had not been assigned to cash transfers; and second, GD did not initially wish to rely on IPA's census activities for its own determination of eligibility of households in transfer villages. Half way through the experiment and upon satisfactory demonstration of alignment in the two organizations' eligibility determination, GD started to rely more heavily on IPA census data.

offered to enroll in GD's program; this entailed the provision of a mobile phone (unless the household chose to use an existing phone) and three mobile money payments of KES[32] 10k, 50k, and 50k made in two-month intervals. For households that chose to receive a mobile phone, its price was deducted from the final payment.

In villages that were assigned to receive the package comprising both cash transfers and the psychological intervention, IPA returned to study participants in the time frame between the initial transfer and the final transfer. Depending on randomized assignment at the household level, an adult female in the household was invited to participate in a psychological intervention either in the days before or in the days *after* the second of the three transfers. A test of cognitive performance was administered immediately preceding the psychological intervention, as described further below. The psychological intervention then started with two back-to-back ten-minute videos that were viewed by participants on a tablet. In one video, a female character envisions her family in a more prosperous future; in the other, a prosperous lady reflects on her humble beginnings. Both role models show how the planning of incremental steps culminates in a pathway towards their long-term aspirations. Both experience difficulties but overcome them by adjusting their approach, investing in their own capacities, and tenaciously adhering to the conviction that their future is in their hands. The intervention was grounded in literature on best possible selves (King, 2001); mental contrasting and implementation intentions (Oettingen & Gollwitzer, 2010); growth mindset (Dweck, 1986); and self-efficacy (Bandura, 1982). Note that not only goal-setting and plan-making, but also the updating of motivational beliefs more generally, has been described as cognitively demanding (Callan, Juth, & Jeong, 2019; Oettingen & Gollwitzer, 2010).

Two brief public service announcements are placed in the intermission. Following the videos, participants engaged in a facilitated exercise, drawing a desired future like one of the role models had; setting goals; anticipating challenges; and making plans for how to pursue goals and react to challenges. At the conclusion of the exercise, participants were also given reminders in the form of calendars and stickers.

[32] The nominal exchange rate in mid-2016 was KES/USD = 100.88, meaning that 50k Kenyan shillings corresponded to approximately 500 US dollars.

The psychological intervention was immediately followed by a manipulation check survey that measured participants' recall of aspects of the video and collected measures of growth mindset and self-efficacy.

**Figure 28: Randomized Project Timeline for Generic Participant**

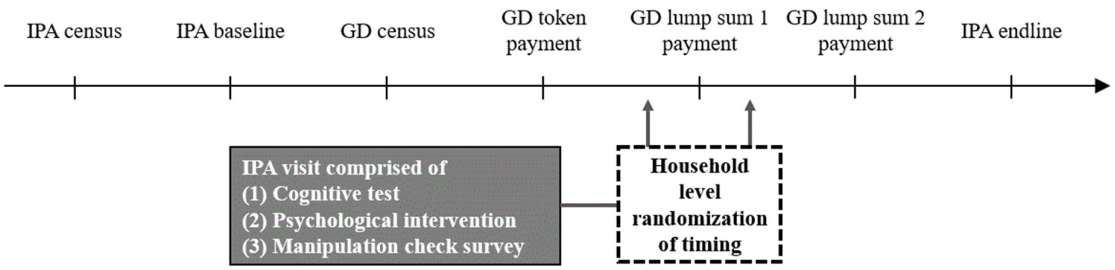



Figure 28 illustrates the timeline from the perspective of the generic participant. Implementation was spread out over sequential cohorts; the first participants received both the psychological intervention and the lump sum 1 payment in November 2016 and the last participants received these interventions in July 2017. An endline survey took place in late 2018, allowing us to measure medium-term household-level impacts on behaviors as well as economic investment, revenue, consumption, and assets.

### *SAMPLING*

As discussed, this study is limited to participants whose villages had for the purposes of the trial by Orkin et al. (forthcoming) been assigned into that arm which involves both cash transfers and the psychological intervention. All participants in this group were further assigned to receive the psychological intervention either just before or just after lump sum 1 ("after cash" condition). In addition, as discussed in the pre-analysis plan, the initial analysis will be limited to respondents with the following attributes: (A) successfully baselined; and (B) received the transfer in question in that month in which GD first made the transfer in question in the same village; and (C) received the psychologically active intervention within 72 hours of the first participant who received the psychological intervention in the same village and sub-arm.

These criteria allow us to clear the sample of those sources of attrition and noncompliance that are plausibly orthogonal to treatment assignment. Any bias introduced by these restrictions depends on the degree to which they cause differential exclusion by treatment arm. Criterion (A) appears entirely unproblematic; though the study was not double-blinded – the randomized assignment was known to the author at the time

of baseline – this information was neither accessible to enumerators nor respondents. It is implausible, but not impossible, that criteria (B) and (C) could have interacted with the individual level assignment. For instance, being invited to participate in the psychological intervention before the inflow of lump sum 1 could have influenced participants' decision to drop out of the GD program at the last minute; or receiving the lump sum 1 could have influenced participants' propensity to consent to the psychological intervention. We will therefore assess the potential for bias by testing for differential odds of exclusion via criteria (B) and (C) by randomized assignment and evaluating covariate balance with and without these restrictions. We will also include the balance and treatment estimates on the "raw" intent-to-treat population comprised of all baseline respondents, so restricted only by criterion (A).

***Data***

The data are derived from the IPA census (which serves as the source of socioeconomic covariates); a baseline survey (which serves as the source of baseline outcome measures); GD's administrative transfer records; a cognitive test administered immediately before the psychological intervention; a manipulation check survey collected immediately after the psychological intervention; and an endline survey. All outcomes and covariates are defined in Appendix III: Measures.

The cleaning and operationalization of all outcomes is equivalent to the analysis plan of the trial by Orkin et al. (forthcoming).[33] One exception is that here index construction will follow the methodology used in Kling, Liebman, & Katz (2007), so as to avoid deviation from previous versions of the analysis plan for this study. As the study by Orkin et al. (forthcoming) remains underway, further changes to the underlying data set remain possible; as we wish for data in this study to ultimately be fully consistent, such changes could have repercussions for the results presented here as well. For more details, consult Appendix IV: Status of Data.

***Empirical Strategy***

[33]socialscienceregistry.org/docs/analysisplan/2134/document

The following model will be estimated:

$$y_{ij} = \alpha_j + \beta A_{ij} + \gamma b_{ij} + \delta X_{ij} + \varepsilon_{ij}$$

Here $y_{ij}$ is the outcome of interest for household $i$ in village $j$; $\alpha_j$ is a village fixed effect; $A_{ij}$ is the randomized assignment, coded to 1 in the "after cash" condition and to 0 in the "before cash" condition; $X_{ij}$ is a vector of the three primary socioeconomic covariates from the census variables (as listed in Appendix III: Measures); and $\varepsilon_{ij}$ is an idiosyncratic error term. The term $b_{ij}$ is the baseline value of $y_{ij}$; it is only available for a subset of outcome measures, which is identified in Appendix III: Measures. Where baseline data are unavailable, the term is excluded.

We will present intent-to-treat coefficient $\beta$ in separate tables showing the constituent components of eight outcome classes: cognitive performance (as collected immediately before the psychological intervention); recall and self-beliefs (collected immediately after the psychological intervention); technology adoption behaviors and practices, as well as economic investment, revenue, consumption, and assets (collected from the endline survey). The main results will be a summary table of the eight aggregates. Each table will individually account for multiple inference by controlling for the false discovery rate (Benjamini & Hochberg, 1995). We will report sharpened q values following the two-stage method coded and described by Anderson (2008).

We will test sensitivity to different specification assumptions. We will substitute village fixed effects for stratum fixed effects; and instead of the three pre-defined socioeconomic covariates, we will use a set of three chosen among the eleven available ones (as listed in Appendix III: Measures) using the least angle regression algorithm (Efron et al., 2004).

We will also use randomization inference to estimate Fischer exact p values.[34] This may yield different results than parametric methods when outcomes are not normally distributed (Gerber & Green, 2012). The procedure will be as follows: First, we re-randomize the data set 10,000 times and extract the difference

[34] We speak of "estimating" them because we lack the computing power to define every possible randomization, and Fischer exact p values can be approximated through simulation.

between the treatment and counterfactual groups under each simulated assignment. (Under the sharp null hypothesis, these differences define our sampling distribution.[35]) Second, we rank the simulated differences, assigning lower ranks to those with higher absolute values. Third, we locate that rank which corresponds most closely to the absolute value of the difference that was actually observed in the trial. Finally, we divide this rank by the number of conducted simulations to compute a p value. (For intuition: if only 400 out of 10,000 simulations yield a difference between treatment and counterfactual above that which was actually observed, the p value equals 0.04 and the observed effect can be deemed significant, so not well explained by sampling error alone.)

## *RESULTS*

### *Participant Flow, Balance, Compliance, and Attrition*

In villages assigned to receive both cash transfers and the psychological intervention, 2,188 households consented to baseline. As displayed in Table 73, just over half of these were treated in compliance with the intended protocol, i.e., are not excluded via criteria (B) or (C). The odds of exclusion are slightly higher in the group that was assigned to receive the psychological intervention before, as opposed to after, the cash inflow; but this difference is not significant (i.e., that the ratio of odds is not significantly different from 1).

[35] The sharp null hypothesis assumes no treatment effect for any participant. This equates the unobserved with the observed outcome in the potential outcomes framework.

**Table 73: Participant Flow**

| | Assigned to receive psychological intervention | | |
|---|---|---|---|
| | **before cash inflow** | **after cash inflow** | **Total** |
| Sample that consented to baseline | 1,097 | 1,091 | 2,188 |
| Excluded via timing criteria | 545 | 524 | 1,069 |
| Restricted sample (primary study population) | 552 | 567 | 1,119 |
| Odds: excluded vs included | 0.987 | 0.924 | |
| Odds ratio (std dev) | 1.068 (0.0914) | | |
| p value | 0.440 | | |
| | | | |
| Primary study population | 552 | 567 | 1,119 |
| Attrited | 18 | 10 | 28 |
| Consented to endline | 534 | 557 | 1,091 |
| Odds: attrited vs consented | 0.034 | 0.018 | |
| Odds ratio (std dev) | 1.878 (0.749) | | |
| p value | 0.114 | | |

Note: Integers define the number of applicable observations.

As displayed in Table 74, neither individual census covariates nor the full covariate vector are significant determinants of the randomized assignment; in other words, both samples are well balanced.

**Table 74: Balance Checks**

| | **Baseline consenters** | | | **Primary study population** | | |
|---|---|---|---|---|---|---|
| Covariate | N | Ctrl mean | p value | N | Ctrl mean | p value |
| Respondent's age | 2,188 | 40.2 | 0.626 | 1,119 | 40.0 | 0.209 |
| Household's per capita housing space (sq m) | 1,969 | 6.90 | 0.984 | 1,005 | 6.03 | 0.717 |
| Respondent education grade 8 or above | 2,188 | 43% | 0.650 | 1,119 | 44% | 0.756 |
| Household married | 2,188 | 58% | 0.581 | 1,119 | 59% | 0.908 |
| Number of household members | 2,188 | 5.31 | 0.599 | 1,119 | 5.53 | 0.728 |
| Metal roof | 2,188 | 79% | 0.343 | 1,119 | 74% | 0.889 |
| Covered latrine or modern toilet | 2,188 | 26% | 0.387 | 1,119 | 29% | 0.961 |
| Mud wall | 2,188 | 79% | 0.596 | 1,119 | 76% | 0.136 |
| Mud floor | 2,188 | 76% | 0.822 | 1,119 | 72% | 0.393 |
| Asset index | 2,188 | 2.07 | 0.984 | 1,119 | 2.18 | 0.851 |
| Enumerator's poverty rating | 2,084 | 4.14 | 0.902 | 1,092 | 4.23 | 0.897 |
| **F test of joint orthogonality** | 1,865 | | **0.985** | 978 | | **0.948** |

Notes: N defines the number of available observations with defined covariate values; the F test applies only where all covariates are available. "Ctrl mean" defines the average value in the group that was assigned to receive the psychological intervention before the cash transfer. Values are in percentage terms in the case of binary variables. P values for individual coefficients and for the F statistic are derived from the linear regression $A_i = \alpha + \delta X_i + \varepsilon_i$.

One consequence of criterion (C) is that it restricts the sample to participants who actually received the psychological intervention, meaning that all of them also engaged in the cognitive test that immediately preceded it as well as the manipulation check survey that immediately succeeded it. Attrition can therefore

only materialize in the endline. As shown in Table 73, 28 study participants did not end up consenting to it, but there is no evidence of a significant difference by treatment arm.

Absent any indications of bias from differential compliance or differential attrition, we will continue to interpret those 1,119 households that survived exclusion via criteria (B) and (C) as the primary study population. However, we will include results for the full sample (i.e., the 2,188 baseline consenters) as a robustness check.

***Impact Estimates***

Aggregated results for the primary study population are displayed in Table 75. We see borderline significant effects on recall in the expected direction, though these do not withstand an adjustment for multiple inference. Aggregated results for all baseline consenters and disaggregated results for the primary study population, can be seen in Appendix I: Tables.

**Table 75: Impact of "After Cash" Condition (Primary Study Population)**

| | Cognitive performance | Recall | Self-beliefs | Tec adoption & info seeking |
|---|---|---|---|---|
| Coefficient | 0.057 | 0.108 | -0.003 | -0.021 |
| Error | 0.053 | 0.059 | 0.059 | 0.064 |
| Effect/mean | *N/A* | *N/A* | *N/A* | *N/A* |
| Effect/s.d. | 5.7% | 10.8% | -0.3% | -2.1% |
| p value | 0.284 | 0.066 * | 0.957 | 0.746 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 1,005 | 1,005 | 1,005 | 981 |
| | Economic investment | Revenue | Consumption | Assets |
| Coefficient | 395 | 741 | 359 | 4,180 |
| Error | 462 | 725 | 774 | 3,464 |
| Effect/mean | 8.0% | 7.7% | 1.6% | 5.7% |
| Effect/s.d. | 4.9% | 5.8% | 2.4% | 6.7% |
| p value | 0.392 | 0.307 | 0.642 | 0.228 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 980 | 980 | 980 | 980 |

Notes: Estimates are derived from the primary study population. The top row is comprised of standardized indices; the bottom row is comprised of monetary aggregates in Kenyan Shillings. In the computation of the asset aggregate, net financial liabilities are subtracted. The coefficient corresponds to $\beta$. Effect/mean is defined as the coefficient divided by the mean value of the measure in the counterfactual; this is not available for the index variables, as the standardization process defines the mean of the counterfactual as 0. Effect/s.d. is defined as the coefficient divided by the standard deviation of the measure in the counterfactual. We control for the false discovery rate across the p values reported in this table (Benjamini & Hochberg, 1995) and compute sharpened q values following the method used by Anderson (2008). Symbol * indicates a p or q value below 0.1; ** indicates a p or q below 0.05; *** indicates a p or q below 0.01.

Above we committed to adding analyses with a different definition of the sample, different fixed effects, and different socioeconomic covariates. Combining these options leads to $2^3$=8 possible analytical specifications for each outcome. Note also (as discussed in Appendix IV: Status of Data) that this data set is shared with another study and shared data cleaning procedures have not been finalized. Beyond the unadjusted data set, we will therefore use one that winsorizes[36] all monetary outcomes at the 95% level, meaning that 16 specifications become available for each monetary outcome. Appendix II: Figures uses so-called specification curves to illustrate the standardized effects and associated p values for each outcome aggregate and each applicable analytical specification. The findings appear robust to specification choices.

The results of the randomization inference are shown in Table 76. Recall that these test the sharp null hypothesis of zero effect for every participant. The results remain comparable to those of the regression analysis: borderline significant impacts again emerge only on the recall variable, but they are not robust to multiple inference.

**Table 76: Randomization Inference: "After Cash" Condition (Primary Study Population)**

| Outcome | Estimate | p value | q value |
|---|---|---|---|
| Cognitive performance | 0.0267 | 0.619 | 1.000 |
| Recall | 0.1049 | 0.060 * | 0.912 |
| Self-beliefs | -0.0336 | 0.535 | 1.000 |
| Tec adoption & info seeking | -0.0240 | 0.693 | 1.000 |
| Economic investment | 76 | 0.874 | 1.000 |
| Revenue | 242 | 0.754 | 1.000 |
| Consumption | -185 | 0.825 | 1.000 |
| Assets | 1,250 | 0.743 | 1.000 |

Notes: The first four outcomes are standardized indices; the bottom four are monetary aggregates in Kenyan Shillings. In the computation of the asset aggregate, net financial liabilities are subtracted. The impact estimate is defined as the raw difference of the corresponding outcome aggregate between the two assigned groups, i.e., $[y \mid A = 1] - [y \mid A = 0]$. For a description of the computation process for p values, consult the section "Empirical Strategy". We control for the false discovery rate across the p values reported in this table (Benjamini & Hochberg, 1995) and compute sharpened q values following the method used by Anderson (2008). Symbol * indicates a p or q value below 0.1; ** indicates a p or q below 0.05; *** indicates a p or q below 0.01.

***EXPLORATORY ANALYSES***

[36] 95% winsorization re-codes the top and bottom 2.5% of each monetary outcome (as listed Appendix III: Measures) to the cutoff value. To avoid an attenuating bias, this is done separately by treatment assignment. Aggregates measures are not again winsorized. This robustness check was not included in the pre-analysis plan.

We fail to establish cognitive impacts from the cash inflow, which seems to challenge scarcity theory. Here we explore possible shortcomings in our analysis that might explain this. None of the following analyses were identified in the pre-analysis plan.

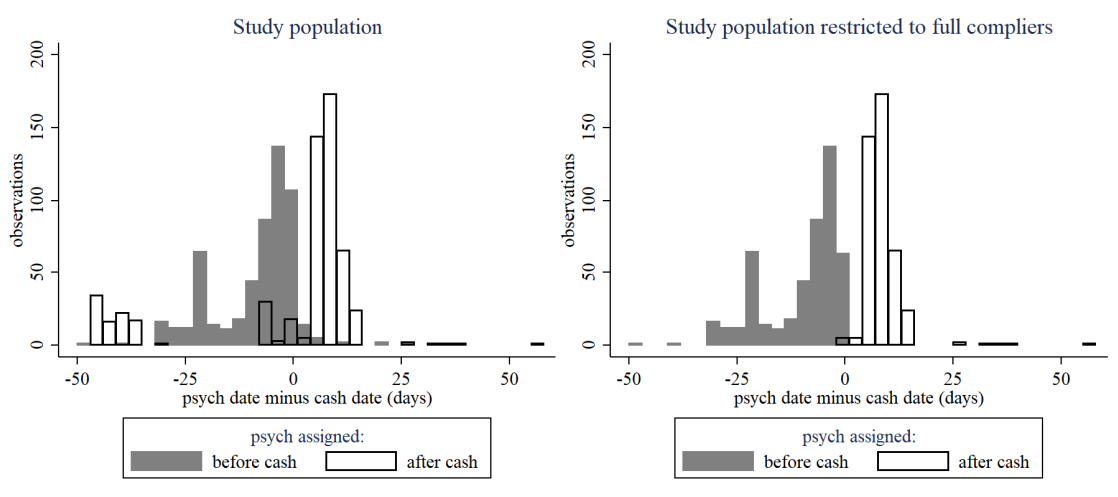
***Residual Cognitive Impacts from Previous Inflow***

The study by Carvalho et al. (2016) previously failed to establish cognitive impacts from payday. Mani, Mullainathan, Shafir, & Zhao prepared a comment (forthcoming) [37] on this study proposing that "*the increase in scarcity for their before-payday group is minor because of proximity to the previous payday. Re-examining their data in tighter time windows around payday, we find that poverty does affect cognitive function closer to payday.*" This argument could explain our results in principle: as illustrated in Figure 28, a small token payment had already been released to study participants two months before lump sum 1 was disbursed, so before the cognitive data were collected and the psychological intervention administered. This payment could have had residual impacts on cognitive function in the "before cash" condition, reducing the incremental impact of lump sum 1 in the "after cash" condition. To investigate this, we first follow the approach of analyzing the results in tighter time windows. We start by introducing an additional exclusion criterion that removes any residual noncompliers who had survived the other exclusion criteria but for logistical reasons did not receive the psychological intervention relative to lump sum 1 as assigned. This leaves in place a reduced population of 906 "full compliers", as illustrated in Figure 29.

We then repeat the estimation of the primary specification, gradually restricting the sample by keeping only those that received the psychological intervention (and concurrent cognitive test) within a given time window around lump sum 1. While sampling error will rise with a thinner sample, effects could conceivably also rise as the discontinuity around the moment of the transfer comes into sharper focus. But as Figure 30 illustrates, cognitive impacts do not approach significance for any time window.

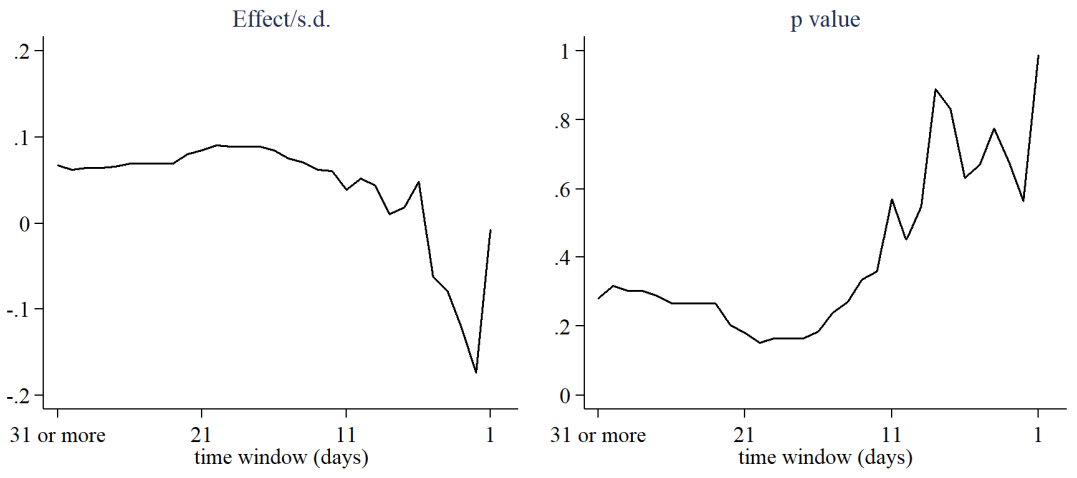
[37] Many thanks to Anandi Mani for sharing the draft and allowing for its citation.

**Figure 29: Relative Timing of Cash (Lump Sum 1) and Psychological Intervention**




Notes: The x axis is the number of days by which the implementation of the psychological intervention and associated cognitive test ("psych") preceded the disbursement of lump sum 1 ("cash"). It is a negative value where psych preceded cash. Isolated observations beyond the 50-day time window are omitted for visual ease.

**Figure 30: Impacts on Cognitive Performance Index as Function of Time Window**




Notes: Effects p values are calculated following the same procedures used in Table 75. Where the value on the x axis equals "31 or more", all full compliers are considered. For all other values on the x axes, observations are removed if they fall outside of the corresponding time window. This time window is defined as the maximum difference between the psych and cash interventions; i.e., the absolute value of the x axis in Figure 29.

We can further take advantage of the fact that the cognitive test was administered not only to participants who were assigned to receive the psychological intervention as well as cash, but in all four groups of the factorial design by Orkin et al. (forthcoming). We can therefore compare "any cash" – i.e., the condition of village-level assignment to cash transfers, irrespective of the household-level assignment of timing – to

"no cash", the condition of not being assigned to any cash transfers at all. This clears our counterfactual of any residual cognitive effects the token transfer may have had. (For a visual illustration and detailed description of the full experimental design, consult Figure 32 in Appendix II: Figures; the analysis that is now initiated uses comparison set B). The results are illustrated in Table 77: we continue to find no evidence of cognitive effects.

**Table 77: Impact of "Any Cash" Condition on Cognitive Performance and Index**

| | Working memory: digit span | Fluid intelligence: Raven's | Cognitive control: Stroop | Cognitive performance |
|---|---|---|---|---|
| Coefficient | -0.005 | 0.004 | 0.107 | 0.001 |
| Error | 0.028 | 0.049 | 0.429 | 0.027 |
| Effect/mean | -0.2% | 0.2% | 0.2% | *N/A* |
| Effect/s.d. | -0.5% | 0.2% | 0.6% | 0.1% |
| p value | 0.860 | 0.943 | 0.802 | 0.980 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 6,467 | 6,467 | 6,467 | 6,467 |

Notes: The index calculation and econometric specification follow the analysis plan of Orkin et al. (forthcoming). Specifically, the index is calculated using the method by Anderson (2008). The econometric model equals ours except as follows: first, it estimates the impacts of the "any cash" condition. Second, it uses stratification block fixed effects. Third, it uses the following set of socioeconomic covariates: household size, asset aggregate, respondent education, age, and self-belief index (all from baseline). Fourth, given the clustered randomization of the cash assignment, standard errors are adjusted for cluster robustness (Abadie et al., 2017). Effect/mean is defined as the coefficient divided by the mean value of the measure in the counterfactual; this is not available for the index variable on the right, where the standardization process defines the mean of the counterfactual as 0. Effect/s.d. is defined as the coefficient divided by the standard deviation of the measure in the counterfactual. We control for the false discovery rate across the p values reported in this table (Benjamini & Hochberg, 1995) and compute sharpened q values following the method used by Anderson (2008). Symbol * indicates a p or q value below 0.1; ** indicates a p or q below 0.05; *** indicates a p or q below 0.01.

### *Poor Cognitive Measurement*

Table 75 and Table 76 found borderline significant impacts on the recall index. When the analysis is broadened to all baseline compliers in Table 79, the impacts further gain significance. Table 81 suggests that this is primarily driven by improved recall of video characters' names. As we had presumed improved cognitive function to be a necessary condition for improved recall, the question arises if the validity of the cognitive test might have been impaired. If improved recall was in fact caused by the cash inflow and mediated by improved cognitive performance, but cognitive performance improvements were not detected because cognitive performance was poorly measured, then we have incorrectly rejected scarcity theory.

(Assuming that the psychological intervention is independently impactful, the existence of cognitive effects and the absence economic effects would then challenge cognitive load theory.)

We can again take advantage of the broader study population by Orkin et al. (forthcoming) to evaluate the impacts of assignment to cash on recall. (Unlike in Table 77, we cannot include participants who were not assigned to the psychological intervention, as two of the three recall tasks relate to this intervention. As defined in Figure 32 in Appendix II: Figures, the analysis that is now initiated uses comparison set C). Table 78 shows the results: we no longer detect an impact of assignment to cash transfers on recall – neither on the index, nor on any individual measure.

**Table 78: Impact of "Any Cash" Condition on Recall Tasks and Index**

| | Recall task 1 | Recall task 2 | Recall task 3 | Recall |
|---|---|---|---|---|
| Coefficient | 0.002 | 0.008 | 0.002 | 0.019 |
| Error | 0.007 | 0.012 | 0.024 | 0.042 |
| Effect/mean | 0.2% | 0.9% | 0.8% | *N/A* |
| Effect/s.d. | 1.0% | 2.3% | 0.5% | 0.5% |
| p value | 0.732 | 0.512 | 0.920 | 0.920 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 3,288 | 3,288 | 3,288 | 3,288 |

The notes from Table 77 apply.

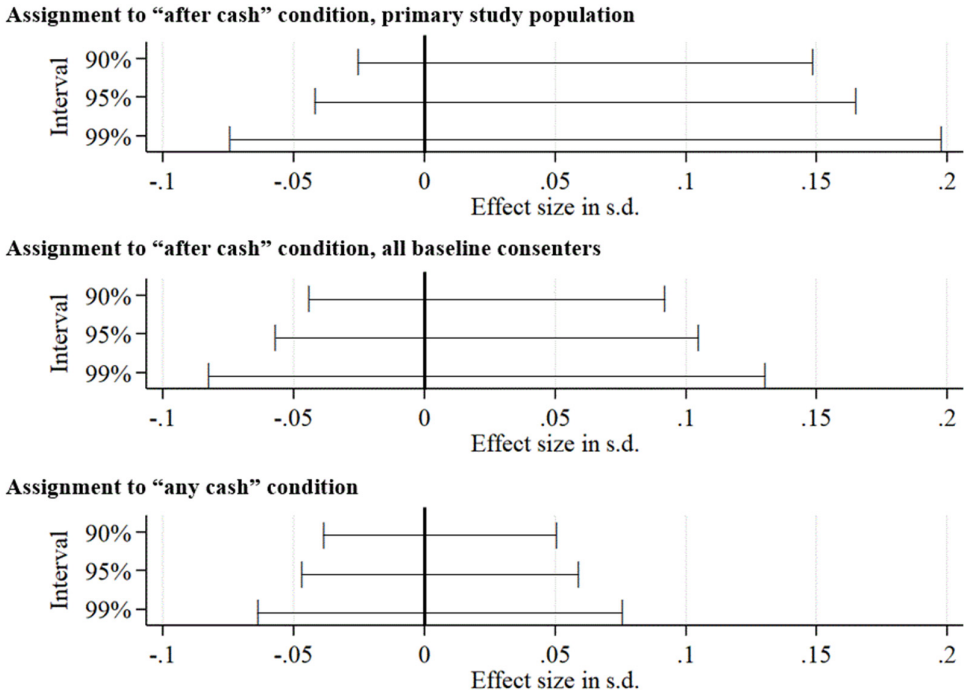
As there is no systematic difference in the timing of data collection between the "any cash" and the "no cash" group, Table 78 is immune to calendar effects. Meanwhile the findings in Table 75, Table 76, Table 79, and Table 81 are weaker on this front, as they had to rely on the assumption that the staggered implementation of the intervention sufficed to clear us of this potential confound. We conclude that cash inflows did not in fact improve recall.

***Insufficient Statistical Power***

Figure 31 visualizes the confidence intervals associated with our estimates to date. The top chart corresponds to the cognitive performance index estimates from Table 75; the middle one to the those from Table 79; and the bottom one to those from Table 77. As in any study that fails to reject the null hypothesis, small true effects could have resisted detection and our results could be false negatives. That said, we can reject a wide range of effects; for instance, the 0.15 standard deviation effect size in Mani et al. (2013)

would be dismissed by all three tests (with the first rejecting it at the 90% and the other two at the 99% level).

**Figure 31: Confidence Intervals for Cognitive Performance Estimates**




***DISCUSSION***

We investigated a possible interaction between a cash transfer and a psychological intervention aimed at schema change. Specifically, we tested if implementing the psychological intervention right after (as opposed to before) the cash disbursement enhances economic impact. This prediction was made by combining scarcity theory (i.e., that cash inflows temporarily boost cognitive function among the poor) with cognitive load theory (i.e., that reduced cognitive load has an enabling effect on schema adoption). The hypothesized impacts did not materialize. The results appear to challenge scarcity theory, as no cognitive impacts materialized in the first place. Future research might help illuminate under what conditions the cognitive effects of cash inflows do and do not materialize among the poor, and under what conditions they can be relieved.

While our null is bounded with relative precision, small effects – ones smaller than those estimated in previous corroborations of scarcity theory – could have resisted detection. In our data, the likelihood of a small positive economic effect is slightly higher than that of a small negative one. Therefore, if a decision-maker is forced to choose between implementing a cognitively demanding (e.g., schema change) intervention before or after cash transfer intervention, our results weakly support doing so after, assuming that these options are cost-equivalent. But we are near-indifferent and would not anticipate significant impacts from doing so. If it turned out that the two interventions complemented each other synergistically (i.e., demonstrated positive interaction effects), our results do not suggest the scarcity mechanism is responsible.

**Table 79: Aggregate Results for Baseline Consenters**

| | Cognitive performance | Recall | Self-beliefs | Tec adoption & info seeking |
|---|---|---|---|---|
| Coefficient | 0.019 | 0.130 | 0.039 | -0.004 |
| Error | 0.041 | 0.046 | 0.044 | 0.046 |
| Effect/mean | *N/A* | *N/A* | *N/A* | *N/A* |
| Effect/s.d. | 1.9% | 13.0% | 3.9% | -0.4% |
| p value | 0.648 | 0.005 *** | 0.374 | 0.939 |
| q value | 1.000 | 0.040 ** | 1.000 | 1.000 |
| N | 1,646 | 1,645 | 1,646 | 1,685 |
| | Economic investment | Revenue | Consumption | Assets |
| Coefficient | 70 | 326 | 278 | -1,977 |
| Error | 311 | 501 | 579 | 2,782 |
| Effect/mean | 1.5% | 3.7% | 1.3% | -2.8% |
| Effect/s.d. | 0.9% | 2.7% | 1.9% | -3.0% |
| p value | 0.823 | 0.516 | 0.631 | 0.477 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 1,684 | 1,684 | 1,684 | 1,684 |

The notes from Table 75 apply, except as follows: estimates are derived from the population of all baseline consenters.

**Table 80: Elements of Cognitive Performance**

| | Working memory: digit span | Fluid intelligence: Raven's | Cognitive control: Stroop |
|---|---|---|---|
| Coefficient | 0.075 | -0.003 | 0.874 |
| Error | 0.060 | 0.095 | 0.912 |
| Effect/mean | 2.5% | -0.1% | 1.8% |
| Effect/s.d. | 7.8% | -0.2% | 4.8% |
| p value | 0.207 | 0.971 | 0.338 |
| q value | 1.000 | 1.000 | 1.000 |
| N | 1,005 | 1,005 | 1,005 |

Notes: Estimates are derived from the primary study population. Effect/mean is defined as the coefficient divided by the mean value in of the measure in the counterfactual. Effect/s.d. is defined as the coefficient divided by the standard deviation of the measure in the counterfactual. We control for the false discovery rate across the p values reported in this table (Benjamini & Hochberg, 1995) and compute sharpened q values following the method used by Anderson (2008). Symbol * indicates a p or q value below 0.1; ** indicates a p or q below 0.05; *** indicates a p or q below 0.01.

**Table 81: Elements of Recall**

| | Recall task 1 | Recall task 2 | Recall task 3 |
|---|---|---|---|
| Coefficient | 0.017 | 0.038 | -0.005 |
| Error | 0.012 | 0.018 | 0.028 |
| Effect/mean | 1.8% | 4.3% | -1.6% |
| Effect/s.d. | 8.8% | 12.0% | -1.0% |
| p value | 0.136 | 0.036 ** | 0.871 |
| q value | 0.158 | 0.121 | 0.409 |
| N | 1,005 | 969 | 1,005 |

The notes from Table 80 apply.

**Table 82: Elements of Self-Beliefs**

| | Self-efficacy | Growth mindset |
|---|---|---|
| Coefficient | -0.048 | 0.049 |
| Error | 0.175 | 0.316 |
| Effect/mean | -0.2% | 0.2% |
| Effect/s.d. | -1.7% | 0.9% |
| p value | 0.784 | 0.878 |
| q value | 1.000 | 1.000 |
| N | 1,005 | 1,005 |

The notes from Table 80 apply.

**Table 83: Elements of Technology Adoption and Information Seeking**

| | Tec adoption in agriculture | Tec adoption in livestock | Enterprise expansion activities | Information seeking activities |
|---|---|---|---|---|
| Coefficient | -0.076 | 0.073 | -0.086 | 0.002 |
| Error | 0.114 | 0.068 | 0.060 | 0.038 |
| Effect/mean | -3.4% | 7.2% | -12.6% | 0.7% |
| Effect/s.d. | -4.3% | 6.9% | -8.0% | 0.3% |
| p value | 0.507 | 0.284 | 0.153 | 0.956 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 981 | 981 | 981 | 981 |

The notes from Table 80 apply.

**Table 84: Elements of Economic Investment**

| | Agricultural input expenditures | Livestock input expenditures | Non-agricultural expenditures |
|---|---|---|---|
| Coefficient | 62 | 22 | 307 |
| Error | 29 | 80 | 448 |
| Effect/mean | 16.6% | 2.7% | 8.3% |
| Effect/s.d. | 13.6% | 1.6% | 4.0% |
| p value | 0.036 ** | 0.783 | 0.492 |
| q value | 0.120 | 1.000 | 0.971 |
| N | 980 | 980 | 980 |

The notes from Table 80 apply, except as follows: the coefficient is defined in Kenyan Shillings.

**Table 85: Elements of Revenue**

| | Revenue from agriculture | Revenue from livestock | Non-agricultural revenue | Labor earnings |
|---|---|---|---|---|
| Coefficient | -27 | -248 | 922 | 155 |
| Error | 55 | 212 | 618 | 235 |
| Effect/mean | -3.1% | -13.3% | 16.8% | 11.4% |
| Effect/s.d. | -2.9% | -7.2% | 8.0% | 4.4% |
| p value | 0.624 | 0.241 | 0.135 | 0.511 |
| q value | 0.931 | 0.931 | 0.931 | 0.931 |
| N | 980 | 980 | 980 | 980 |

The notes from Table 84 apply.

**Table 86: Elements of Consumption**

| | Food consumption | Non-food consumption | Expenditure on durables | Social expenditure | Education expenditure |
|---|---|---|---|---|---|
| Coefficient | -13 | 110 | 0 | 17 | -49 |
| Error | 314 | 128 | 65 | 48 | 691 |
| Effect/mean | -0.1% | 3.4% | 0.0% | 3.5% | -0.6% |
| Effect/s.d. | -0.2% | 5.0% | 0.0% | 2.3% | -0.4% |
| p value | 0.968 | 0.391 | 0.996 | 0.722 | 0.943 |
| q value | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| N | 980 | 980 | 980 | 980 | 831 |

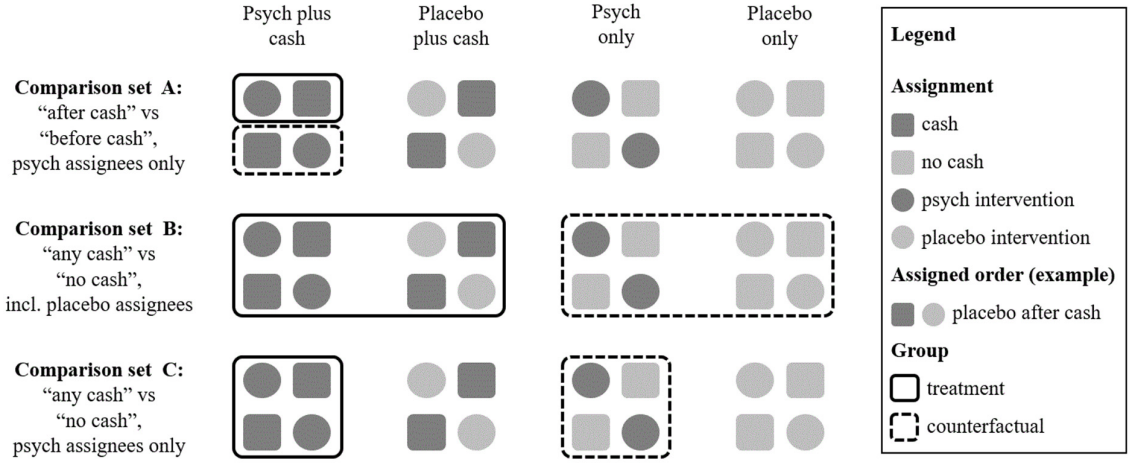
The notes from Table 84 apply.

**Table 87: Elements of Assets**

| | Durable assets | Livestock assets | Savings | Net financial liabilities | Stocks of dried maize |
|---|---|---|---|---|---|
| Coefficient | 687 | 2,339 | 711 | 874 | 149 |
| Error | 1,915 | 2,085 | 654 | 647 | 336 |
| Effect/mean | 1.8% | 7.8% | 12.1% | 25.3% | 3.8% |
| Effect/s.d. | 2.1% | 6.3% | 6.5% | 8.6% | 3.1% |
| p value | 0.720 | 0.262 | 0.277 | 0.177 | 0.659 |
| q value | 0.859 | 0.859 | 0.859 | 0.859 | 0.859 |
| N | 980 | 980 | 953 | 970 | 981 |

The notes from Table 84 apply.

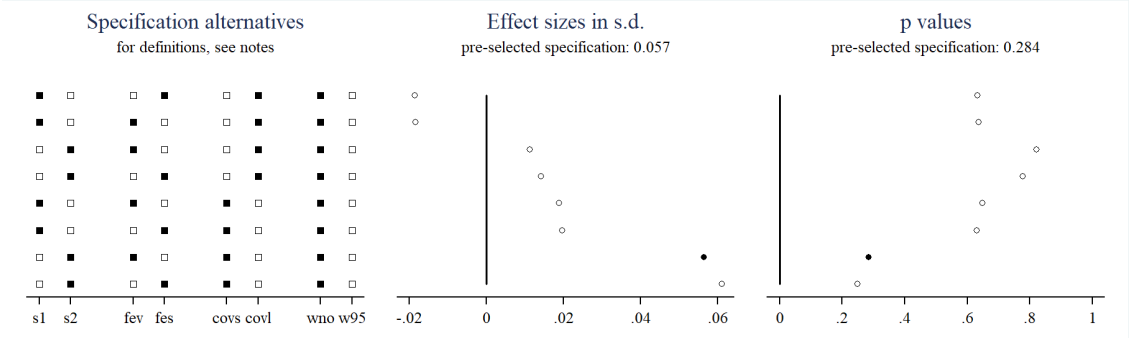
**Figure 32: Illustration of Full Experimental Design**




Notes:

- Four arms were assigned at random at the village level: (1) cash transfers plus a psychological ("psych") intervention; (2) cash transfers plus a placebo intervention (involving no active psych ingredient); (3) no cash transfers but a psych intervention; and (4) no cash transfers but a placebo intervention. The timing of the psych intervention (and associated cognitive and manipulation check data collection), relative to lum sum 1, followed *randomized* assignment at the household level *only* in cash-plus-psych villages. This defined the "after cash" and "before cash" conditions, using **comparison set A**.
- Both the psych and placebo intervention were immediately preceded by a cognitive test in all four village arms. We can therefore evaluate the impacts of assignment to cash on cognitive impacts using **comparison set B**. Households who were assigned the "before cash" and "after cash" condition are both subsumed under the "any cash"condition.
- While both the psych and placebo intervention were also followed by a manipulation check survey in all four arms, the recall measures from this survey (e.g., about the names of lead characters) only apply to those who participated in the psych intervention. For the purpose of analyzing the impact the "any cash" condition on recall, assignees to the placebo intervention must therefore be removed, leading to **comparison set C**.

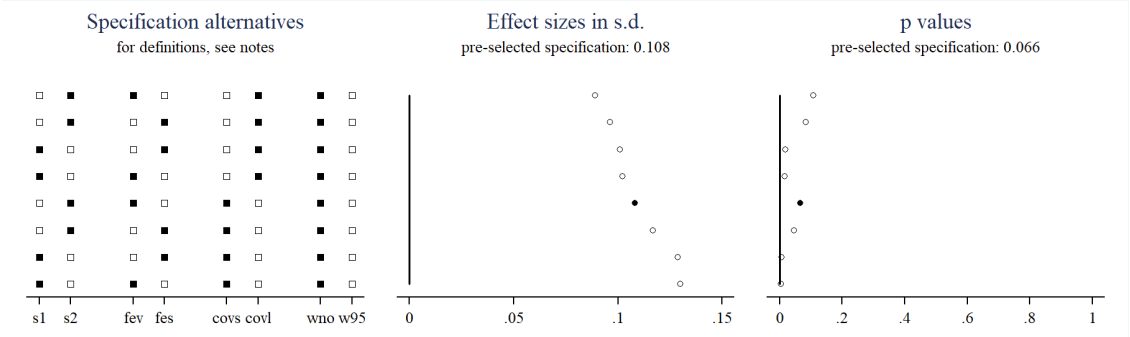
**Figure 33: Specification Curve – Cognitive Performance**




Notes: Each row in the chart on the left describes one specification. In the same row, the charts on the right show the corresponding effect size and p value. More specifically, read the three charts as follows:

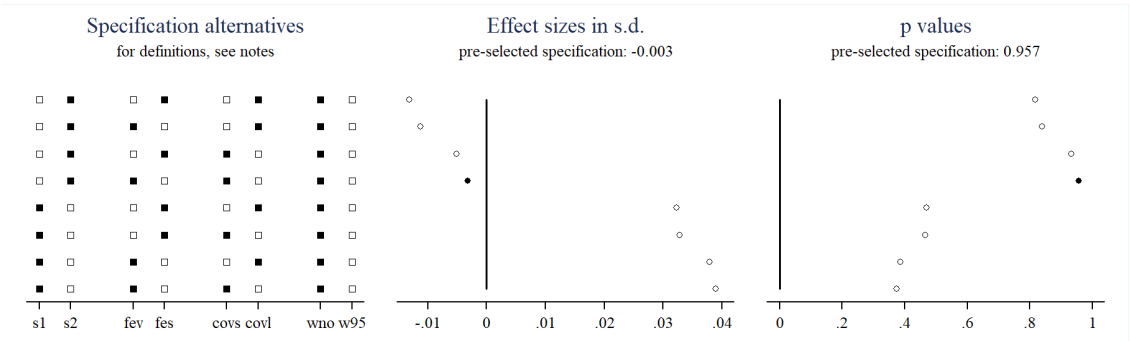
- **"Specification alternatives"**: Columns define specification features. A black square ■ indicates that a choice dimension applies, and a blank square □ that it doesn't. **s1** indicates that all baseline respondents are included in the sample, **s2** that only the primary study population is used. **fev** indicates village fixed effects, **fes** stratum fixed effects. **covs** indicates that the three pre-selected socioeconomic covariates are used, **covl** that three socioeconomic covariates are selected through the least angle regression algorithm. **wno** indicates that measures are not subject to any additional outlier correction; **w95** indicates that measures are winsorized at the 95% level. (By 95% winsorization we mean that the top and bottom 2.5% are coded to the respective cutoff levels. This is done separately by randomized assignment to avoid an attenuating bias. Only monetary values are considered for winsorization.)
- **"Effect sizes":** These display the treatment effect estimates that correspond to the specification choice, presented in standardized terms (i.e., in terms of standard deviations of the counterfactual). All numbers are per capita, and flow numbers are annualized. The preferred specification, identified in the paper, is highlighted through a black marker ●. Treatment effects are sorted in ascending order.
- **"p values":** These display the corresponding p values. The preferred specification is again highlighted.

**Figure 34: Specification Curve – Recall**



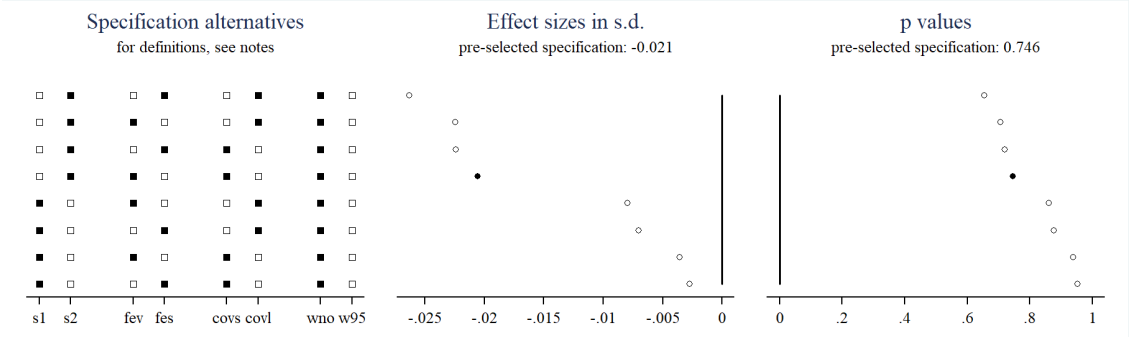

The notes from Figure 33 apply.

**Figure 35: Specification Curve – Self-Beliefs**



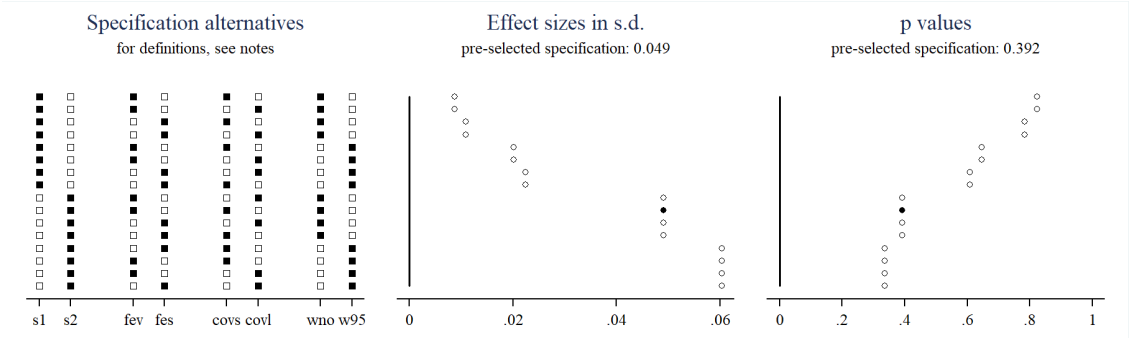

The notes from Figure 33 apply.

**Figure 36: Specification Curve – Technology Adoption and Information Seeking**



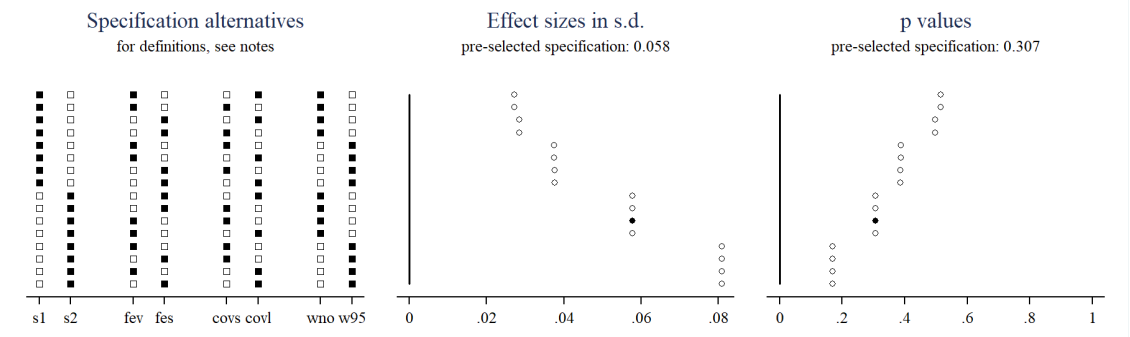

The notes from Figure 33 apply.

**Figure 37: Specification Curve – Economic Investment**



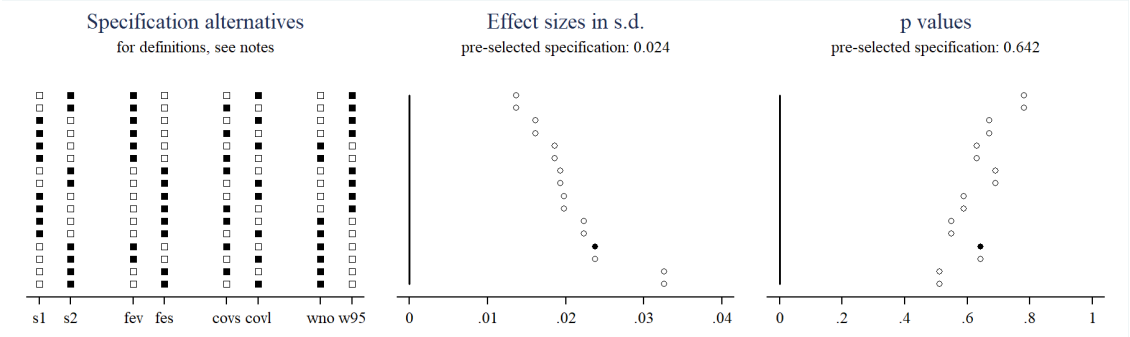

The notes from Figure 33 apply.

**Figure 38: Specification Curve – Revenue**



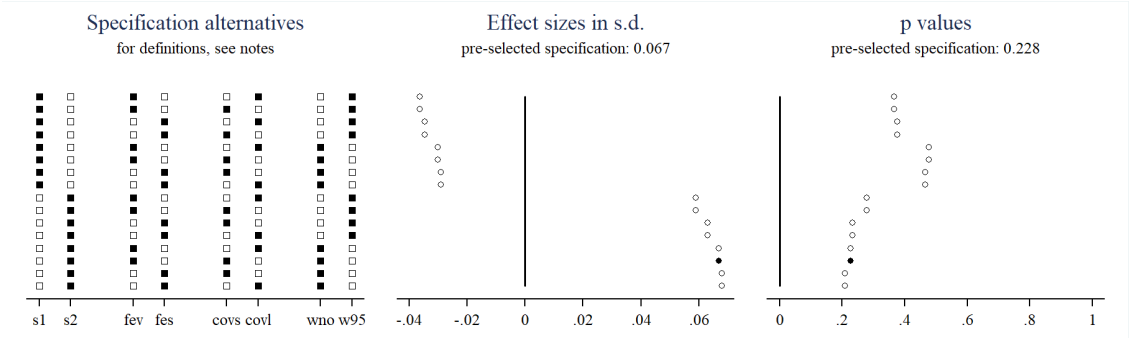

The notes from Figure 33 apply.

**Figure 39: Specification Curve – Consumption**




The notes from Figure 33 apply.

**Figure 40: Specification Curve – Assets**




The notes from Figure 33 apply.

## *APPENDIX III: MEASURES*

### Outcomes Collected Immediately Before the Psychological Intervention

The cleaning and operationalization of all outcomes is equivalent to the analysis plan[38] for trial #996 by Orkin et al. (forthcoming), with one exception: in the timing trial, index construction will follow the methodology used in Kling, Liebman, & Katz (2007), so as to avoid deviation from previous versions of the analysis plan for this study.

Outcomes that are marked with ◊ were not measured at baseline. Composites (i.e., indices and aggregates) are defined as having a baseline value available as long as it is available for at least one of the sub-composites.

1. Cognitive Performance:◊ An index[39] comprised of the following three variables:
    1.1. Working memory (digit span):◊ Where no response is correct, scored as zero. Otherwise scored as the length of the longest sequence that respondents can correctly recall, minus two points. (24 sequences ranging between 3 and 8 digits)
    1.2. Fluid intelligence (Raven's matrices):◊ Scored as the number of correct responses provided within the time limit. (6 problems)
    1.3. Cognitive control (numerical Stroop):◊ Scored as the number of correct responses provided within the time limit. (75 problems divided into 3 sets)

### Outcomes Collected Immediately After the Psychological Intervention ("Manipulation Checks")

2. Recall:◊ An index comprised of three variables:
    2.1. Recall task 1:◊ A dummy variable coded to one if the respondent correctly recalls that a character name was mentioned in the movie.
    2.2. Recall task 2:◊ A dummy variable coded to one if the respondent correctly recalls the name of a lead character in the movie.[40]

[38] socialscienceregistry.org/docs/analysisplan/2134/document
[39] All indices in this paper will follow the index constructed methodology used in Kling, Liebman, & Katz (2007).
[40] The pre-analysis plan incorrectly defined this name as "Judy". Both "Judy" and "Josephine" are correct answers to recall task 2.

2.3. Recall task 3:◊ A dummy variable coded to one if the respondent answers that young Kenyan men who do secondary school are more likely to end up working in a paid job than those who do not.[41]

3. Self-Beliefs: An index[42] comprised of two variables:

3.1. Self-efficacy: A scale from 0 to 28, constructed from the sum of 7 items, each scored from 0 to 4 points.[43]

3.2. Growth mindset:◊ A scale from 6 to 36, constructed from the sum of 6 items, each scored from 1 to 6 points.

**Endline Outcomes**

4. Technology Adoption and Information Seeking:◊ An index comprised of the following:

4.1. Technology adoption in crop agriculture ◊

4.2. Technology adoption in livestock related activities ◊

4.3. Enterprise expansion activities ◊

4.4. Information seeking activities ◊

5. Economic Investment: An aggregate comprised of the following:

5.1. Agricultural input expenditures

5.2. Livestock input expenditures

5.3. Non-agricultural expenditures

6. Revenue: An aggregate comprised of the following:

6.1. Revenue from agriculture

6.2. Revenue from livestock rearing and produce

6.3. Revenue from non-agricultural activities

6.4. Total household labor earnings

[41] This statement was made during the video intermission.

[42] These were labeled "agency" variables in the "Analysis Plan for Recall and Agency Variables in the Manipulation Check Data". At the time these outcomes were registered for the timing study, the study team associated with the main study by Orkin et al. (forthcoming) had already analyzed the impact of cash and/or psychological interventions on these variables. However, the impact of timing as described in this analysis plan had not been analyzed.

[43] The "Analysis Plan for Recall and Agency Variables in the Manipulation Check Data" did not account for the fact that baseline data are available for self-efficacy. The impact of the relative timing of the cash and psychologically active interventions on this outcome has not been initiated.

7. Consumption: An aggregate comprised of the following:

    7.1. Food consumption

    7.2. Non-food non-durable consumption

    7.3. Expenditure on durable goods

    7.4. Social expenditure

    7.5. Education expenditure

8. Assets: An aggregate comprised of the following:

    8.1. Durable assets

    8.2. Livestock

    8.3. Savings

    8.4. Net financial liabilities

    8.5. Stocks of dried maize ◊

**Covariates**

*Primary census covariates*

(g) Respondent's age;

(h) Household's per capita housing space;

(i) Respondent's secondary educational attainment (a binary variable; seventh grade or below versus eighth grade or above);

*Secondary census covariates*

(j) Household marital status (binary variable; married versus not)

(k) Household size (continuous variable)

(l) Roof material (binary variable; metal versus not)

(m) Toilet facility (binary variable; covered latrine, flush, or vip toilet versus other)

(n) Wall materials (binary variable; mud versus other)

(o) Floor material (binary variable; mud versus other)

(p) Asset index (continuous variable; number of assets reported)

(q) Enumerator's quintile ranking (continuous variable; five classes)

## *APPENDIX IV: STATUS OF DATA*

The research team associated with Orkin et al. (forthcoming) may continue to clean and thereby change the data. Not all edits are foreseeable. We currently expect the strata (as defined for the purpose of the household-level randomization) to be updated. Among 2,188 baseline consenters, strata are currently unavailable for 386; for purposes of the analysis presented here, all observations within a village for which strata are unavailable are assigned to a residual village-specific stratum.

## *APPENDIX V: PILOT TRIAL*

The interventions were conducted by the branch managers in five rural Ugandan branches, who spent an estimated 10 minutes with participants following an intervention template.[44] Follow-up of participants was conducted one and two months after the intervention, using a basic financial diary survey.[45] 329 participant responses were collected in the first month. A subset of participants had previously participated in routine socioeconomic data collection, allowing for the creation of covariates for most observations.

**Table 88: First-Month Effects among Loan Takers at BRAC Uganda**

**Impact of Psychological Intervention (Counterfactual: No Psychological Intervention)**

| | Internal Control Index | | Monthly Cash Outflow | | Weekly Cash Inflow | |
|---|---|---|---|---|---|---|
| Specification | (1) | (2) | (1) | (2) | (1) | (2) |
| Coefficient | 0.036 * | 0.049 *** | -162.1 | -41.3 | 23.33 ** | 7.77 |
| Error | (0.019) | (0.016) | (106.0) | (86.8) | (10.62) | (9.47) |
| N | 271 | 326 | 263 | 329 | 268 | 329 |

**Impact of Psychological Intervention *After* Loan Disbursement (Conterfactual: *Before*)**

| | Internal Control Index | | Monthly Cash Outflow | | Weekly Cash Inflow | |
|---|---|---|---|---|---|---|
| Specification | (1) | (2) | (1) | (2) | (1) | (2) |
| Coefficient | 0.007 | 0.031 ** | 24.0 | 80.4 | -2.92 | 18.07 * |
| Error | (0.019) | (0.015) | (74.7) | (56.7) | (12.41) | (9.33) |
| N | 213 | 248 | 210 | 249 | 208 | 249 |

Notes: Regression specification (1) is $y_{ij} = \alpha_j + \beta T_{ij} + \delta X_{ij} + \varepsilon_{ij}$, where $y_{ij}$ is the outcome of interest for individual $i$ from village $j$; $\alpha_j$ is a village fixed effect; $T_{ij}$ is the randomized assignment, coded to 1 among participants who were assigned to the treatment condition; $\beta$ is the coefficient estimating the impact of treatment assignment; $X_{ij}$ is a vector of three socioeconomic covariates selected using least angle regression (Efron, Hastie, Johnstone, & Tibshirani, 2004); and $\varepsilon_{ij}$ is an idiosyncratic error term. Specification (2) excludes village fixed effects and socioeconomic covariates: $y_{ij} = \alpha + \beta T_{ij} + \varepsilon_{ij}$. This specification allows for the inclusion of more observations (i.e., it includes ones for which covariates are unavailable). Symbol * indicates a p value < 0.1; ** p < 0.05; *** p < 0.01. The internal control index is derived from Duttweiler (1984); measures are standardized. Monthly cash outflows are the USD cash expenses associated with the respondent household's agricultural, livestock & poultry, and micro-enterprise operations, measured using a 30-day recall period. Weekly cash inflows are the USD cash revenues associated with the respondent household's agricultural, livestock & poultry, and micro-enterprise operations, as well as any income from salaried or day labor, measured using a 7-day recall period. All USD outcomes are translated from Ugandan Shillings using the mid-year nominal exchange rate of 2013 and winsorized at the 5% level.

[44] See online study materials: osf.io/uwmjn.
[45] See online study materials: osf.io/uwmjn.

The significance of economic effects is statistically borderline and very sensitive to specification choices; impacts subsided on economic measures by the second month; none emerged on additional available outcomes (i.e., working hours and consumption); and none are robust to multiple inference adjustments.

## *CHAPTER 3. REWARDING POVERTY ALLEVIATION: A CASE STUDY IN PAYMENT-BY-RESULTS*


### *ABSTRACT*

Payment-by-Results (PbR) involves funders' commitments to make future grant payments as a function of yet-unachieved results. Though the justification may go beyond the desire to overcome incentive problems (such as moral hazard or adverse selection), it is desirable for payments to be closely tied to impacts – "as if" to incentivize implementers to perform in the interests of program beneficiaries. We describe a so-called impact bond transaction in the poverty alleviation space that commits payments in direct proportion to the economic value created in beneficiary households. Even so, incentive-compatibility is not easily achieved in practice: when funders insure themselves against extreme outcomes, and when working capital providers absorb performance risk, they alter the shape of the reward function. The greater the error associated with impact measurement, the more incentives can be affected.


### *DISCLOSURES*

Supplementary materials, data, and code are accessible on the Open Science Framework.[46] My involvement in project design and donor advisory could have affected the neutrality of my assessments.

### *INTRODUCTION*

There is long-standing interest in the use of PbR mechanisms to enhance the effectiveness of development assistance (Savedoff, Perakis, & Schwanke, 2016). Following initial experiences in the United Kingdom, the concept of impact bonds has been gaining traction in development space (Floyd, 2017; Social Finance, 2019). These typically make results-based payments to non-governmental implementers and involve private investors to help bridge working capital gaps (Fraser, McHugh, & Warner, 2019). We will first conceptually review the justifications and pitfalls associated with PbR, then present a case study involving an impact bond aimed to alleviate poverty in Uganda. We will then contextualize the justifications and

[46] osf.io/mp5ns

pitfalls in the Ugandan case; for this purpose, we will simulate outcomes and payments using available pre-existing evaluation data.

## *CONCEPTUAL ANALYSIS OF PBR*

### *Justifications*

A fundamental problem of charitable aid transactions is that the benefits are not meant to accrue to the grantor (funder) nor the grantee (implementer), but to third parties that typically get to participate in the resource allocation process neither as market players nor as voters. This calls for rules and procedures to assure that the interests of the intended beneficiaries are accounted for. There are different ideas for to achieve this (though they do not claim to be universal, final, or mutually exclusive). For example, one prescription is to disburse resources directly as cash to beneficiaries to allow them to act upon their own preferences (Blattman & Niehaus, 2014). Another holds that interventions should be administered with enough granularity to allow for the identification of causal effects, which can then be used to adapt the development sector for the better by accelerating effective approaches and dropping ineffective ones (Banerjee, Deaton, Lustig, Rogoff, & Hsu, 2006; Banerjee & Duflo, 2011). Meanwhile, PbR prescribes that funders pay implementers directly as a function of outcomes, thereby creating accountability for impact (Savedoff et al., 2016).

#### *Asymmetric Information Frame*

Justifications for PbR often invoke the need to create appropriate incentives for implementers. This implies that funders have information gaps about an implementer's true capability or effort (Clist, 2016; Lazear, 2003).

*Moral hazard* problems may arise when the principal (the funder) lacks information about some hidden actions of an agent (say, about the amount of effort exerted by an implementer), and when the incentives of principals and agents differ (say, because the implementer values comfort). In this framework, PbR can be viewed as "performance pay" to align the agent's incentives with the objectives of the principal.

A variant of the principal-agent problem can arise when a market player's offer attracts a counterparty with undesirable traits on a hidden dimension. Let us posit that most implementers are in fact motivated to serve the poor as best they can, but have diverse beliefs sets about the proper approach to solving a problem. Among them, one bad apple is unconcerned with social impact and instead concerned with raising money to perpetuate its own existence and therefore willing to uncritically align its narratives with the beliefs of funders. If funders are naïve about this and fund only based on the alignment of an applicant's funding application with their own beliefs, then in a form of *adverse selection* the "narrative entrepreneur" will be selected for funding. Meanwhile, if funders are aware of their predicament, they can use PbR as a signaling device to encourage the self-selection of "impact entrepreneurs" who intend to execute a course of action that they truly believe to be sensible.

*Knowledge Gap Frame*

Both moral hazard and adverse selection are grounded in the principal-agent model. In framing the interests of implementers – but not funders – as misaligned with those of beneficiaries, this model builds on a questionable foundation. It can be sensible to assume that implementers and funders are equally motivated to achieve their stated objectives: altruism (and complementary desires for reputation and self-respect) are well-established drivers of human behavior that can dominate nonsocial incentives (Batson & Shaw, 1991; Fehr & Fischbacher, 2003; Ashraf, Bandiera, & Jack, 2014). Even so, PbR can remain sensible: it has the potential to trigger learning processes that overcome shared ignorance about how to achieve impact.

Translating evidence into policy comes with many difficulties. The body of accessible evidence may be biased,[47] and effects that emerged in different contexts may be moderated by unknown factors.[48] Even

[47] For instance, there may be selection bias in favor of evaluation sites where conditions are unusually favorable in the first place. Useful evidence can then only be retrieved from sites where implementation succeeds, introducing survivorship bias. This is aggravated by publication bias driven by the norms and incentives of academia (Ioannidis, 2005). Research outlets may elevate results that are significant, intuitive, or entertaining, and direct less attention towards results that are insignificant or perplexing. In anticipation of such biases in the publication process, researchers may mine for those analytical specifications that happen to yield welcome results and deprioritize the reporting of less compelling ones, further skewing the body of evidence. There are indications that experimental results tend to be less biased by statistical reporting conventions than nonexperimental ones, though they are not immune (Brodeur, Sangnier, Lé, Sangnier, & Zylberberg, 2016; Chan, Hrobjartsson, Haahr, Gotzsche, & Altman, 2004).

[48] Even methods that go to great lengths to identify causality in a given setting (such as randomized evaluations, which subject the entire implementation process to scientific protocol) may not provide any elevated insight into the

evidence that is both credible and context-relevant is not easily acted upon, e.g., because of skill and attention deficits (Callen, Khan, Khwaja, & Liaqat, forthcoming). PbR has the potential to draw decision-makers' limited *attention* to contextually relevant evidence of impact (Perakis & Savedoff, 2015). The role of funders is to focus on "ends": PbR forces them to realistically establish their willingness to pay for social outcomes based on the opportunity costs of capital. Meanwhile the role of implementers is to identify appropriate "means" and focus on the challenge of delivering impacts with limited resources. Both the impact and the cost drivers of cost-effectiveness therefore receive appropriate consideration. At the conclusion of a project, evidence of impact becomes the central determinant of payment streams and is certain to become top-of-mind for all parties involved.

Some forms of knowledge are inherently difficult to codify, diffuse, and learn. For example, implementers may accumulate tacit "know-how" (Polanyi, 1966) and diverse subtle, nuanced, and fleeting insights as they "muddle along" in trial-and-error (Andrews, Pritchett, & Woolcock, 2013). PbR can accommodate this by affording implementers the flexibility to mutate their approach (Savedoff et al., 2016) and then selecting for successful approaches in an evolutionary fashion (Barder, 2012).[49] For intuition, assume that funders and implementers are unable to use evidence for projection, and indeed altogether unable to form any expectations at all. Picture implementers as the equivalent of a basic biological organism that obstinately executes a given programmatic code, and funders as the equivalent of a natural selection mechanism that retrospectively rewards resources to those implementers that happen to find success. To the extent that these resources tend to propagate the success factors in the implementer's programmatic

factors that moderated the impacts. Generalizing from past evaluation results calls for an awareness of the contextual factors that moderated effects in the original settings, and of their role in the new and different settings (Cartwright & Hardie, 2012; Deaton, 2010). Without such awareness, it is difficult to extrapolate into new settings where moderators may differ, and difficult to cumulate evidence into a body of knowledge. A theoretical grasp of the barriers to progress, and of the mechanisms through which interventions unfold impact, is said to be required for the appropriate use of evidence in policymaking (Bates & Glennerster, 2017; Williams, 2017).

[49] Such forces are known to operate well beyond the domain of biology; they are often invoked in the discussion of technological change and economic progress (Beinhocker, 2007; Lynch, 1996; Nelson & Winter, 1982; Schumpeter, 1942) and are actively deployed in the domain of machine learning and artificial intelligence (Barto & Sutton, 1998). Evolutionary and incentive-based perspectives can often be reconciled as complementary scientific models of the same phenomena. Biologist Mayr (1961) argued that it is simultaneously true that a predator's hunt is caused by hunger and by natural selection; one can be seen as a "proximate" and the other as an "ultimate" cause. Economist Friedman (1953) illustrated the breadth of incentive-based argumentation by proposing that leaf canopies are shaped "as if" trees deliberately strived to maximize exposure to sunlight.

code, we have a system that could force the constituency of the development sector to be a successful one. The long-term repercussions will depend on specific assumptions about this propagation process. For instance, if we assume a stable set of implementers with immutable programmatic code, the unit of selection is the implementer: successful implementers will start to outgrow unsuccessful ones and start to dominate the sector. Meanwhile, if we assume that success leads to the replication of the successful implementer's programmatic code outside of the implementer's structures, the implementer would (using the terminology of Dawkins, 1976) merely be a vehicle, while the programmatic code (e.g., the active ingredients of the implementer's approach that caused success) would be the replicator. This *evolutionary* framing speaks to difficulties associated with the generalization of evidence. Even if both funders and implementers were overwhelmed by the complexity of projecting where effectiveness will materialize, a results-based funding system might select for effectiveness and thereby learn by reinforcement.

### *Concerns*

There are numerous concerns about unwelcome aspects and unintended consequences of PbR. (For a detailed discussion, see Clist, 2016):

*Costs*

PbR introduces contractual complexities not found in other arrangements. For instance, impact evaluation is no longer a luxury but required as a matter of routine. As specific evaluation results get directly tied to the financial standing of specific stakeholders, corrupting forces could threaten the independence of evaluation. Furthermore, as PbR removes the presumption of strategic alignment, the transacting parties may be more inclined to make zero-sum investments to prepare for or engage in disagreements (Eldridge & Tekolste, 2016).

When payments are made retrospectively based on demonstrated results, this also creates the need for working capital to support program execution in the interim. Such capital is subjected to performance risk, as it is repaid as a function of results that are not established at the time of investment. The implementers of development programs are frequently small nonprofit organizations that may not have such risk capital readily accessible. So-called impact bonds provide a conceptual and administrative framework to enable

private risk capital providers to invest in the activities of nonprofits. The term "impact bond" is a misnomer because such arrangements rarely involve fixed interest commitments that would subject the investee to the risk of default; payments are often defined as a function of program results and performance risk is transferred to the investor.

To the extent that investors require positive expected returns to compensate them for the time value and risk associated with their investments, part of the total envelope of social resources available for development efforts is absorbed by the financial sector. Our concern about this will depend on our assumptions about the efficiency of financial markets: if capital is efficiently priced, outcome funders value their risk reductions no less than the investors charge for absorbing them. Indeed, if we assume that there is dormant investor demand for so-called impact investments and that the expected returns from such investments are below the risk-adjusted market rates, then impact bonds might attract additional net capital towards development challenges. But adding investors to the already complex PbR arrangements can be costly; the transaction costs of joint ventures have been shown to grow exponentially with the number of contracting parties (Gong, Shenkar, Luo, & Nyaw, 2007). At the time of writing, even after the initiation of dozens of impact bonds over the last seven years amounting to hundreds of millions of dollars in transaction volume, philanthropic subsidies to the origination process remain the norm (Floyd, 2017).

*Potential for Motivation Crowding Effects*

Performance pay can crowd out the payee's intrinsic motivation of agents by undermining the presumption of mutual trust that can otherwise govern the execution of incomplete contracts (Bénabou & Tirole, 2006; Falk & Kosfeld, 2006a; Frey, 1997). Indeed, it has been argued that PbR is not an evidence-based approach because it appears to defy empirical insights on the effects of extrinsic performance incentives on intrinsic motivation (Frey, 2017). Of additional concern, there is evidence to suggest that once a social contract is damaged through the introduction of an ill-conceived pecuniary incentive, it may not be restored through the subsequent removal of the incentive (Gneezy & Rustichini, 2000).

*Potential for Gaming*

Goodhart's law holds that incentive processes can corrupt measures once they are defined as targets, as intuitively illustrated by anecdotes that bounties on pests that were meant to control them instead encouraged their breeding (Campbell, 1979; Goodhart, 1975; Vann, 2003). Short of entirely backfiring, target-setting can skew a multitasking implementer's priorities towards those activities whose success is most easily measured, dis-incentivizing ones that may be equally valuable but more difficult to measure (Holmstrom & Milgrom, 1991). It may also lead to cream-skimming, encouraging the exclusion of beneficiaries that are perceived as costly (McHugh, Sinclair, Roy, Donaldson, & Huckfield, 2013). The perverse effects of performance incentives have been widely established in public policy contexts (Carter & Whitworth, 2015; Courty & Marschke, 2008; Dixit, 2002; Jacob & Levitt, 2003).

## *CASE STUDY: THE VILLAGE ENTERPRISE IMPACT BOND*

### *Background*

Village Enterprise is a small non-profit organization that was funded in 1987 and has an annual operating expense budget between $US\$\,1.2\text{M}$ (2013) and $US\$\,2.1\text{M}$ (2016). Most of this is spent on the implementation of microenterprise programming in rural regions of Uganda and Kenya. The intervention has been filed under the *ultra-poor graduation program* label, as it shares the key aspects of its objective and approach: Village Enterprise targets the poorest households (ordinarily between the poorest quartile and the poorest half of a given village, as measured through a participatory wealth ranking and validated through a simple survey) and provides them with a seed asset as well as an intangible human capital asset (generated through an extended series of training and mentorship sessions). The theory of change holds that the intangible asset helps households maintain their newly acquired wealth and apply it to productive activities, deriving economic value from it. This is thought to generate a stream of income and sustained improvements in living standards as a result.

The program has some distinctive features. It involves the formation of both business groups (target size: three households) and savings groups (target size: 30 households). The seed asset is provided in the form of cash – more specifically, two cash injections to the business group. The program is less intensive than

many of the so-called graduation programs that share its spirit: it comes at roughly a third of the cost (in USD PPP terms) of the least costly one of those evaluated in the meta-study of Banerjee et al. (2015).

Between 2013 and 2016, the implementer was involved in a randomized trial evaluating the impacts of diverse components and variants of its program (Sedlmayr, Shah, & Sulaiman, 2018). Results suggest that the implementer succeeded in reducing poverty.

As the implementer is relatively small but has demonstrated the potential and ambition to scale up, it is an interesting case to study the process of promoting integrated poverty alleviation through PbR. A group of funders decided to support an expansion of its programming with an outcome funding commitment that would allow the implementer to reach roughly 15k households (~90k individuals). Rather than projecting with confidence that specific rates of cost-effectiveness will be achieved in the future, funders offered to reward the implementer as a function of future impacts.

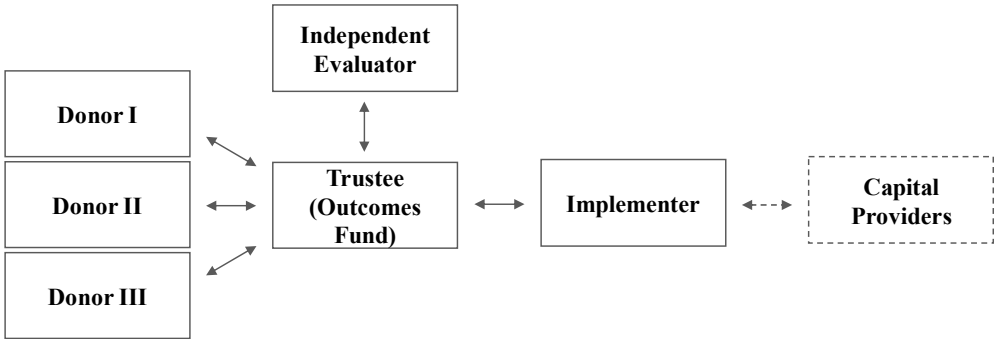
The challenge was framed as follows: "*As the deepening body of evidence points to the potential of integrated poverty alleviation interventions, priorities shift to a new challenge: how to administer them cost-effectively at growing scale? Funders recognize their own limitations in grasping the necessary success factors and challenge entrepreneurially minded development practitioners to master and demonstrate cost-effective delivery*."

***Design, Implementation, and Measurement***

The details of the challenge are as follows.[50] As illustrated in Figure 41, donor-funders contribute to a single trust (the "Poverty Alleviation Outcomes Fund") which escrows them and prepares a Payment-by-Results contract with the implementer. The implementer does not have to be fully financed at this point; it has the option to use the contractual commitment to independently raise any working capital necessary to implement the project. All contracts would remain bilateral: none would be signed by more than two parties.

[50] For a more detailed illustration, see the design memo by Instiglio (2018).

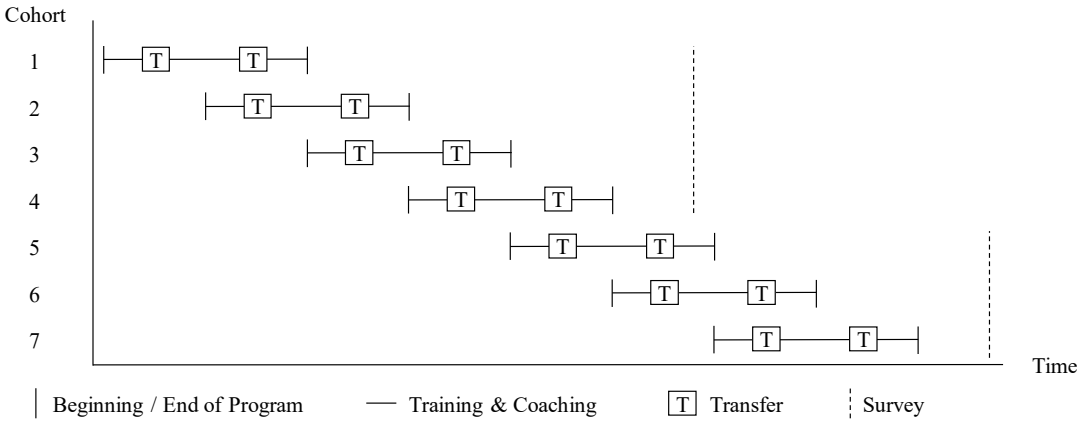
**Figure 41: Contractual Framework**




Note: Arrows depict contractual relationships.

Enough funding was initially committed to support outcome payments for seven cohorts (see Figure 42), each comprised of 28-66 villages where measurement would take place. Once financed, the implementer would generate a list of targetable villages, identify eligible households in these villages using its ordinary targeting mechanisms, and provide these data to the independent evaluator. The evaluator would randomize units into a treatment and a control group and return the list to the implementer, who would proceed to implementing the program with treatment participants and commit to not engaging with control participants any further.

**Figure 42: Implementation Sequence (Treatment Group)**




The provision of seed assets (in the form of two cash transfers) is a central feature of the program, and the evaluator would independently confirm if and exactly when each transfer is provided to each eligible

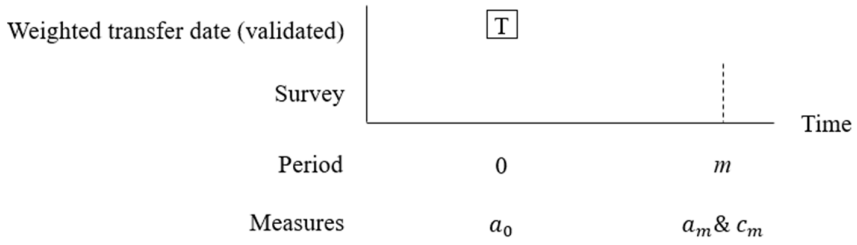
beneficiary. This involves a validation exercise: the independent evaluator assigns enumerators to be physically present at the disbursements to document all transfers.

The evaluator would conduct one household survey in treatment as well as control villages, collecting data on asset stocks and consumption flows. In consideration of a trade-off between survey costs and capital costs, cohorts 1 through 4 and 5 through 7 would be surveyed jointly.

Note that not all recipient households would receive transfers, or get surveyed, at precisely the same time; nor will all achieve the same incremental outcomes. The payment function processes impact estimates not at the level of households but on the basis of averaged values. In effect, the analysis would therefore be conducted on behalf of the representative beneficiary: a hypothetical household in the treatment group who experiences impacts corresponding to the intent-to-treat effect. Village Enterprise would be compensated for the effects experienced by this representative beneficiary, multiplied by the number of targeted beneficiaries.[51]

As described in Figure 43, period 0 is the date when the transfer is provided to the representative treatment participant. Period $m$ is when the participant answers the household survey, which takes place several months after implementation concludes for the last beneficiary in the set and approximately two years after the weighted transfer date[52] of the representative beneficiary.

**Figure 43: Measures Extracted for the Representative Beneficiary**




[51] In practice, values would be averaged across cohorts 1 through 4 and a payment calculated and disbursed for these upon conclusion of the initial survey; at the end of the final survey, values would be recalculated and payments would be trued-up.

[52] The weighted transfer date is defined as the date that corresponds to the average date of the two transfers, weighted by the two transfer amounts.

The following values[53] would be estimated for the representative participant:

$m$: The number of months elapsed between the weighted transfer date and the date of survey measurement.

$a_0$: The incremental asset stock of the representative beneficiary in period 0. This corresponds to the (average) sum of the two transfers, as established in the transfer validation data.

$a_m$: The incremental net asset position at the time of the survey, i.e., the treatment effect on asset stocks in period $m$. The underlying data would be collected through a household survey that is conducted in both treatment and control villages.

$c_m$: The incremental monthly consumption flow in the representative beneficiary household at the time of the survey, i.e., the average treatment effect on consumption in period $m$. This too is collected through the household survey.

The statistical significance of estimates would not be factored into the payment function.[54]

Using the above, a rate of change in asset impacts can be estimated. Assuming that $a_m > 0$ and $a_0 > 0$, we can calculate an estimated monthly asset growth rate:

(I) $$g = \left(\frac{a_m}{a_0}\right)^{\frac{1}{m}} - 1.$$

As incremental asset positions declined in the previous Village Enterprise trial, $g$ may well be negative. An important assumption in the forthcoming payment function will be that the growth rate of asset impacts approximates the growth rate of consumption. This assumes that the transfers are invested in tangible productive assets (such as livestock, inventory, and tools); and that the returns are consumed. It also assumes constant returns to scale – implausible across the production function but plausible at the margin.

***Payment Function***

[53] All monetary values are defined in current US dollars at time of measurement.
[54] The rationale for this is twofold: first, to simplify due diligence for any working capital providers; and second, to avoid introducing norms that would grant a systematic advantage to high-impact interventions over equally cost-effective low-cost ones.

The objective of the payment function is to create a demand schedule for outcomes that links payoffs to economic impact. The basic objective will be to compensate the implementer for the present value of consumption gains created. This compensation will be no lower than the total cash assets that the implementer transferred to the beneficiaries (a plausible lower bound on impact), and no higher than the volume of available outcomes funds (a necessary upper bound on payments).

As soon as the transfer would be validated, the incremental asset position at time 0 is known and the fund would already make payment $P_0$ to compensate the implementer for it:[55]

(II) $$P_0 = a_0$$

The choice of this early payment, based on an input-type measure, is grounded in the desire to mitigate working capital requirements and to focus the piloting of the Payment-by-Results mechanism on the intangible portion of the program, which may not be as easily scalable as the cash transfer portion. (That said, $a_0$ can be defended as an outcome measure because it constitutes a lower bound on the expected benefits of the cash asset: $a_0$ would equal its impact on consumption outcomes if it was immediately dissaved and consumed.)

The long-term results of the program (particularly those of the human capital portion, which accounts for the majority of the implementer's usual program costs) are not easily valued at the time of intervention,[56] so the parties would wait to learn if additional payments are warranted. In period $m$, Payment $P_0$ would be clawed back and through lump sum payment $P_m$ redefined to something more closely resembling the net present value of incremental consumption:

(III) $$P_m = -a_0 + c_m m + c_m x$$

In summary, the first term on the right hand side is meant to capture the value of the seed asset, which is now returned and therefore subtracted. The second term approximates the value of accumulated

[55] In practice, both seed transfers to participants and corresponding payments from the outcomes fund will be disbursed sequentially. The weighted average transfer date should be considered period 0.
[56] One creative approach might be to establish respondents' willingness to pay for human capital; see Shapiro (2017). It is however questionable if such assessments will be anywhere near accurate; see Jensen (2010).

incremental consumption that is estimated to have materialized in periods 1 through $m$. The third term approximates the value of projected incremental consumption beyond period $m$. $x$ is a multiplier that translates the incremental consumption flow into a perpetuity. By time $m$, there are three possible scenarios (A, B, and C) and corresponding multipliers:

*Scenario A: Incremental Assets Have Disappeared*

Assume (pessimistically) that by period $m$ no incremental asset position remains: $a_m \leq 0$ in this scenario. This implies that the asset which is critical to the theory of change has been consumed, and therefore does not warrant the expectation that a further stream of benefits will continue to accrue beyond period $m$. But it is possible that consumption effects are still discernible and that the estimated accumulated incremental consumption flow has already exceeded of the value of the asset. At minimum, $P_m$ accounts for these benefits, so the first two terms of *Equation III* remain intact. Therefore:

(IV) $x_A = 0$

and:

(V) $P_{m_A} = -a_0 + c_m m$

*Scenario B: Incremental Assets Have Not Fallen*

It is expected that the implementer will aim to achieve a degree of sustainability that warrants a payment in excess of $P_{m_A}$. In an ideal case, asset impacts will have not fallen at all by period $m$, implying $a_m \geq a_0$. If so, it is assumed that the stream of incremental consumption benefits will at the very least persist ($g = 0$). To value it, a periodic discount rate $\beta$ must be defined. The chosen discount rate of $10.00\%$ annualized ($\beta = 0.80\%$ compounded monthly) was selected to correspond to donors' perceived opportunity cost of capital.

Therefore,

(VI) $x_B = \frac{1}{\beta}$

and:

(VII) $P_{m_B} = -a_0 + c_m m + c_m \frac{1}{\beta}$

*Scenario C: Incremental Assets Have Diminished*

In the scenario that is considered most likely based on available evidence, asset impacts have neither fully persisted nor fallen to zero by period $m$, but are lower than the initial asset transfer has been: $a_0 > a_m > 0$. Given the theory of change, it is projected that consumption effects will also be declining in the future. As discussed above, the rate of asset growth can be measured using one validation exercise and one survey, and the rate of consumption growth is also assumed to equal rate $g$ – a negative number. Therefore:

(VIII) $x_C = \frac{1+g}{\beta-g}$

and:

(IX) $P_{m_C} = -a_0 + c_m m + c_m \frac{1+g}{\beta-g}$

*Introduction of Caps and Floors*

Extreme values and payments are possible, and both funders and the implementer have grounds to worry about tail risks. $P_{m_{floor}}$ limits the downside risk for the implementer and $P_{m_{cap}}$ ensures that payments owed will not exceed the available outcome funds. Summing up, the "final" payment function $P_m = P_{m_F}$ shows the rule that would be executed in period $m$:

(X)
IF $a_m \leq 0$ → THEN $P_{m_F} = Median\left[P_{m_{floor}}, P_{m_A}, P_{m_{cap}}\right]$

IF $a_m \geq a_0$ → THEN $P_{m_F} = Median\left[P_{m_{floor}}, P_{m_B}, P_{m_{cap}}\right]$

IF $a_0 > a_m > 0$ → THEN $P_{m_F} = Median\left[P_{m_{floor}}, P_{m_C}, P_{m_{cap}}\right]$

***Subsequent Developments***

In the project at hand, the minimum and maximum bounds that were negotiated by donors and the implementer amounted to $P_{m_{floor}} = 0$ and $P_{m_{cap}} = 265$. Donors committed a total of USD 4.28M in outcomes funds to a nonprofit trustee organization[57] and allocated additional funds to an independent evaluator. The trustee challenged the implementer to independently make any arrangements to unlock funds under the payment agreement described above.[58]

The implementer decided that it could use available working capital to implement the cash transfer portion of the program, which would be repaid quickly and could therefore be recycled across cohorts. Stating operational research purposes, it decided that it would slate two thirds of recipients to receive the traditional USD 50 transfer while one third would receive USD 150. It approached external investors with an agreement that would commit the implementer to use all external investments for the purpose of working capital needed for the non-cash components (i.e., the training and mentorship components as well as associated overhead) and to spend no less than USD 157.77 per household on this. If and once the outcomes fund was to make outcome payments $P_m$, the implementer would pass these payments to investors after subtracting and retaining for itself half of any payments that amount to rates of return in excess of 0.57% compounded monthly (7.00% compounded annually). Using this agreement, the implementer raised USD 2.325M from investors within six months.

***CONTEXTUALIZED ANALYSIS OF JUSTIFICATIONS***

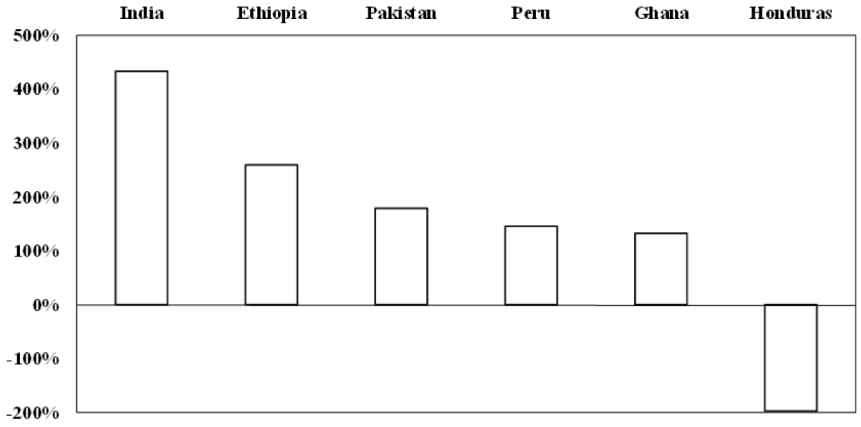
As Village Enterprise has already demonstrated cost-effective poverty alleviation in a past trial by Sedlmayr et al. (2018), and this trial was not associated with outcome based payments, it appears unnecessary to introduce financial incentives to encourage the organization to perform. In other words, it appears implausible to justify the PbR project using the principal-agent model.

We discussed that an alternative objective could be to overcome fundamental knowledge gaps. How might this apply here? The Village Enterprise program is representative of so-called ultra-poor graduation

[57] Two of the three funders transferred all resources to the trustee upfront. The third funder made pledges of future payments.
[58] One condition was that donors would not compensate the implementer for more than USD 1.2M in the form payments $P_0$ (i.e., the reimbursement of cash transfers).

programs that target the poorest households in low-income countries and offer them an integrated program involving transfers as well as customized training and mentorship in an attempt to help them establish sustainable microenterprises (Bandiera et al., 2017). Substantial research on such programs already exists. A meta-study by Banerjee et al. (2015) summarizes six randomized pilot studies of such interventions and indicates that sustained poverty relief is achievable across contexts. Even so, it is difficult to form expectations about the cost-effectiveness of future graduation-type programs, as there is heterogeneity in the results: the approach has been dramatically more cost-effective in some settings than others. Figure 44 presents benefit/cost metrics from the various evaluations that were reviewed in the meta-paper.

**Figure 44: Benefit/Cost Ratios in Six "Graduation" Sites**




Note: For underlying assumptions, consult the data source: Banerjee et al. (2015).

The review discusses this heterogeneity. In the least successful site, the transferred tangible assets depreciated at high rates, which the authors interpret as an indication that the transfer component may be a necessary condition for graduation to succeed. But they do not claim that this is the only reason for the observed heterogeneity – with good reason: clearly there may be any number of other moderators. While we can venture an educated guess about some necessary conditions – e.g., the training and mentorship components will not likely be impactful if they are not implemented, or not delivered in the right language – we cannot readily determine what conditions will be sufficient for these interventions to achieve a given effect. The critical ingredient may be tacit "know-how" that is subtle and cannot be easily defined, measured, or monitored. There are indications that the success factors are present in the Village Enterprise

program: in the past impact evaluation, rates of cost-effectiveness appeared high. In other words, the program is known to be efficacious – effective in principle.[59] But there are grounds to ask if the implementer will maintain such cost-effectiveness at growing scale. Though we do know that it is possible for graduation-type programs to be implemented effectively at meaningful scale (Bandiera et al., 2017), almost all evaluations – Village Enterprise included – took place in small-scale settings. It is reasonable to fear that the factors that moderate their impact may be correlated with the scale of implementation (Bold et al., 2013): e.g., that the fidelity and impacts may suffer with scale.

By engaging in PbR, donors were able to circumvent the need for strong assumptions about what evidence will emerge as the approach advances to greater scale. They could simply define their perceived opportunity cost of capital and left it to the implementer to deliver. The expected rate of return of 10% corresponds closely to the median internal rate of return from past investments in graduation programs (i.e. 8.50% annualized and in real terms; Banerjee et al., 2015; plus approximate USD inflation at the time of project origination.) In other words, it is a reasonable estimate of the opportunity cost of capital.

The more cost-effective the implementer, the higher its actual returns lie above expected returns. This should then lead to a greater surplus, stronger financial tailwind, and improved long-term scaling prospects. The reward function should look "as if" to incentivize the implementer to align its objectives with the benefits experienced by the ultimate beneficiaries – so "as if" the challenge was a principal-agent problem.

Prima facie, the payment function achieves this: as described in the *Appendix*, payments closely approximate the estimated present value of estimated consumption gains. All else equal: the higher the value created by the implementer, the higher the reward. There is an important wrinkle however. We must account for the fact that risk capital providers will be compensated before any residual rewards can be retained by the implementer. (As a risk capital provider cannot obtain ownership over a nonprofit implementer any more than a funder can, we can hardly treat their partnership as one and the same actor.)

[59] Clinical and public health research uses the terms "efficacy" and "effectiveness" to distinguish between basic science trials and ones grappling with at-scale delivery; see Gartlehner, Hansen, Nissman, Lohr, & Carey (2006).

Given the working capital solution that the implementer independently negotiated: once all claims are settled, what residual rewards can we realistically expect it to retain?

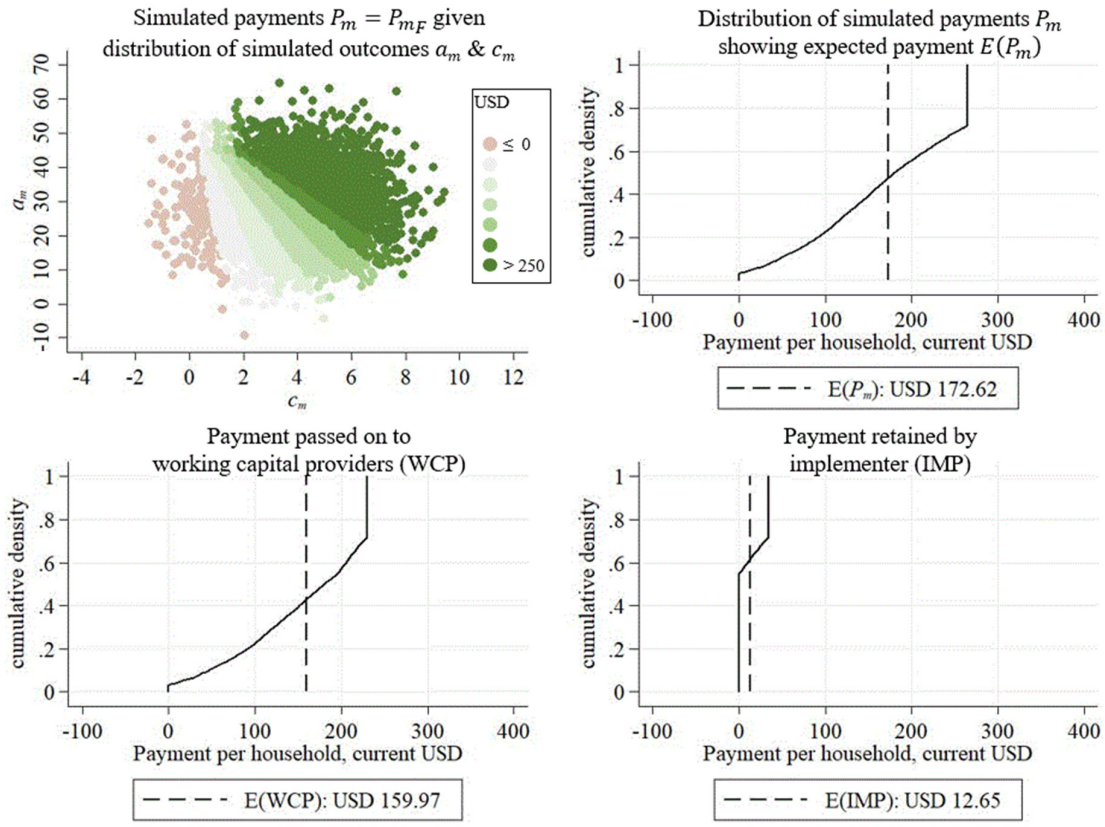
For illustration, first assume that the payment function was applied to the previous trial, using the data and estimates[60] from Sedlmayr et al. (2018): $\hat{a}_0 = 45.25$, $\hat{a}_m = 29.35$, $\hat{c}_m = 3.90$, $\hat{m} = 22$. This leads to $P_m = P_{m_F} = P_{m_C}(\hat{a}_m, \hat{c}_m) = 175.18$. Past unit costs per household (excluding the transfer portion – which was already reimbursed through payment $P_0$ – but including all local and international overhead) were estimated at USD 172.60 per household.[61] This implies that the implementer would have generated a small return of 0.07% monthly. As this lies below the negotiated threshold of 0.57%, all of it would have been passed on to the working capital providers (i.e., investors).

To be sure: even if we assume that past outcomes predict future ones, past hypothetical payouts may not equal expected payouts. In forming expectations about the future project, we should account for the fact that impact estimates will be subject to sampling error, and neither the function that makes payments to the implementer nor the function that subtracts the share of the investors is symmetric around the expected values. Let us now assume that outcomes will be generated by the processes $a_m = \hat{a}_m + \varepsilon_{a_m}$ and $c_m = \hat{c}_m + \varepsilon_{c_m}$, where $\varepsilon$ is normally distributed with a mean of zero and a standard deviation of $\hat{\varepsilon}$ (i.e., the respective errors that were associated with point estimates $\hat{a}_m$ and $\hat{c}_m$ in the previous trial; this assumes, in simplifying fashion, that sampling procedures and therefore sampling error will remain unchanged). The upper left hand side of Figure 45 uses this assumption to simulate combinations of outcomes $a_m$ and $c_m$, calculating a payment $P_m$ for each.

[60] $\hat{a}_0$ is defined as the UGX amount of the cumulative transfer to the beneficiary household, divided by what the paper defines as the UGX/USD intervention exchange rate. $\hat{a}_m = 29.35$ is the residual asset effect in the household at the time of the survey, divided by the pooled rate. $\hat{c}_m$ is the monthly consumption effect at the household level, divided by the pooled rate.

[61] Derived from the trial by Sedlmayr et al. (2018), this is the weighted programmatic cost of sub-arms A2 and B2, excluding transfers.

**Figure 45: Simulation of Outcomes and Payments $P_m$**




Note: Using data from the previous trial, the chart on the top left displays the distribution of 5,000 simulated combinations of $a_m$ and $c_m$. Each of the combinations is also color-coded to indicate the associated payment, as displayed in the legend. For a more detailed derivation of this chart, consult the Appendix. The distribution of resulting payments is visualized in simplified form in the chart on the top right, with the expected payment being the average simulated payment. The bottom left and right charts illustrate how the expected payment is split between the investors and the implementer, in each of the simulated scenarios and on average (i.e., in expectation).

The upper right hand side of Figure 45 plots the same distribution of simulated payments in the form of a cumulative density function. We can see that a small share of simulated payments is constrained by the payment floor while a larger of simulated payments is constrained by the payment cap. The probability-weighted expected payment is therefore reduced below $P_m(\hat{a}_m, \hat{c}_m) = 175.18$ to $E\big(P_m(a_m, c_m)\big) = 172.62$. This corresponds almost exactly to the estimated unit costs per household of USD 172.60, and the expected return remains just barely positive. The bottom two charts of Figure 45 show how expected

payments are split between working capital providers and the implementer. In expectation, the implementer generates a surplus of only USD 12.65 per household.[62]

Summing up: just like the principal-agent argument, the evolutionary argument for PbR requires that implementers have "skin in the game" – so end up in a stronger financial position (and in a better position to propagate) in the case of success than the case of failure. But in the studied case, once investors' absorption of up- and downsides is accounted for, the residual rewards to the implementer are modest. We are reduced to the "attention frame" – the argument that the project draws attention to results – as the most plausible justification for the project.

### *CONTEXTUALIZED ANALYSIS OF CONCERNS*

#### *Costs*

The project relies on administrative data, a simple validation exercise, and a single household survey. By relying on the validation exercise to quantify $a_0$ and on the implementer's targeting data to improve the estimates of $a_m$ and $c_m$, the project avoids the substantial costs of a baseline survey. Ultimately the optimal number of survey rounds continues to depend on statistical power considerations (McKenzie, 2012), which in turn depend on the scale of the project. It is important for the survey to collect estimates that are not downward biased through the omission of minor and exotic items. This may be achievable through a shortened commodity list design such as that described in Beegle et al. (2012), which collects key consumption items and scales to full consumption estimates based on multipliers from more comprehensive local household surveys. Unfortunately, a similar shortcut is not widely accepted for the purpose of asset measurement.

[62] The forthcoming evaluation may differ from those in the previous trial for diverse reasons beyond sampling procedures, such as changes in experimental design, estimation strategy, and context. One change that is already foreseeable is the duration of the investment. The project schedule (as illustrated in Figure 3) suggests a duration (i.e., weighted payback period) in the vicinity of 52 months. Given a minimum investment of USD 157.77 in non-cash expenses per household and a $P_{m_{cap}}$ of USD 265.00, this implies that the maximum achievable return – i.e., that which could be shared between the implementer and the investors in the very best case – is 1.00% monthly (12.68% annually) at most. After returning to investors the full principal, plus a 0.57% monthly (7.00% annual) return, plus half of the remainder, the highest possible surplus that Village Enterprise can mathematically retain in the future amounts to USD 26.74 per household.

The results-based contract is the single most complex and legally fraught transaction in the entire arrangement, and pooling resources into one outcomes fund allowed for it to be executed once as opposed to three times. As discussed, funders were not involved in the pre-financing of the production process: they effectively focused on creating demand and left it to market forces to supply the needed inputs, including working capital financing.

Recall that a critical working hypothesis was that even a small organization such as Village Enterprise would be able to effectively leverage the funders' commitments. Indeed, less than half a year after the formalization of the challenge, the implementer had indeed raised more working capital than its entire organizational budget in any previous year. The involvement of risk capital providers (i.e., private investors) give the project the central feature of a so-called impact bond. But the responsibility for raising working capital was left exclusively to the implementer, and other project stakeholders did not get involved in pre-financing. This mirrors private sector transactions, as well as a results-based financing tool called the advance market commitment (Berndt et al., 2006). While the Village Enterprise experience suggests that this may be low-cost approach to the origination of impact bonds, we also see that it leaves the implementer to independently negotiate the arrangements that define capital costs, and thereby ultimately define the incentive structure.

The capital costs obtained by the implementer appear favorable: Though the risks associated with the project may be largely uncorrelated with other asset classes and therefore well diversifiable, the negotiated rates do imply a philanthropic subsidy. That said, some form of social calculus appears to apply to a large pool of global financial assets (Bernow, Klempner, & Magnin, 2017).

***Potential for Motivation Crowding***

Assuming that the implementer is intrinsically motivated to perform, is the challenge likely to crowd out this intrinsic motivation? We should consider the mechanism through which motivation crowding is thought to work. It is a behavioral phenomenon that is triggered when a principal's actions are perceived as controlling (Frey, 2017) and cast doubt on the nonpecuniary motives or trustworthiness of the agent (Falk & Kosfeld, 2006). This control, and the presumed motivation behind it, is thought to lead to a

reduction in intrinsic motivation. (For this reason, the adverse performance effects of motivation crowding are sometimes referred to as "hidden costs of control".)

As described in the *Background* section, the project frames implementers not as morally dubious subordinates to be controlled but as pioneering entrepreneurs to be empowered. Choosing this reinforcement learning frame – as opposed to a performance incentive frame – is entirely consistent with the project's rationale. But it also appears reasonable from a behavioral perspective: one may hypothesize that abandoning the presumption of a mismatch between funder and implementer intentions helps to mitigate crowding effects.[63]

***Potential for Gaming***

There are a number of ways in which the implementer could game the system.

*Misreporting*

As the implementer collects targeting data, and this targeting data is used as a covariate in the later evaluation, the data could become tainted. This is avoided by assuring that data are shared with the independent evaluator prior to the randomized assignment (where it also serves as a stratification variable). In principle, the implementer could be tempted to manipulate respondents by coaching them to inflate or deflate survey responses depending on their assignment. To avoid this, the implementer is barred from announcing the future independent survey to any participant, and from interacting with the control group after the targeting exercise. The evaluator collects consent at the survey stage[64] and probes for breaches of protocol. There is a clause in the funding agreement that allows for payment to be withheld if breaches are established.

*Re-Balancing of Activities*

[63] More generally, if formalized conditionality is perceived as ordinary in institutional transactions while individuals rely more frequently on trust-based transactions, institutional agents (such as the implementer in question) should be less susceptible to motivation crowding from PbR than individual agents (such as the experimental subjects in the literature on motivation crowding).

[64] Minimizing the perceived association of the implementer and the independent evaluator also reduces the risk that participants interpret the independent survey as an intervention targeting exercise and independently misreport their economic status for that reason.

Might the implementer re-balance activities to focus only on what is measured? Consumption is a credible indicator of living standards among the poor and a less noisy alternative to income, partly because it can be collected through surveys with reasonably high accuracy (Deaton, 1997; Meyer & Sullivan, 2003) and partly because it is subject to consumption smoothing. It is an appropriate definition of the ultimate outcome objective.

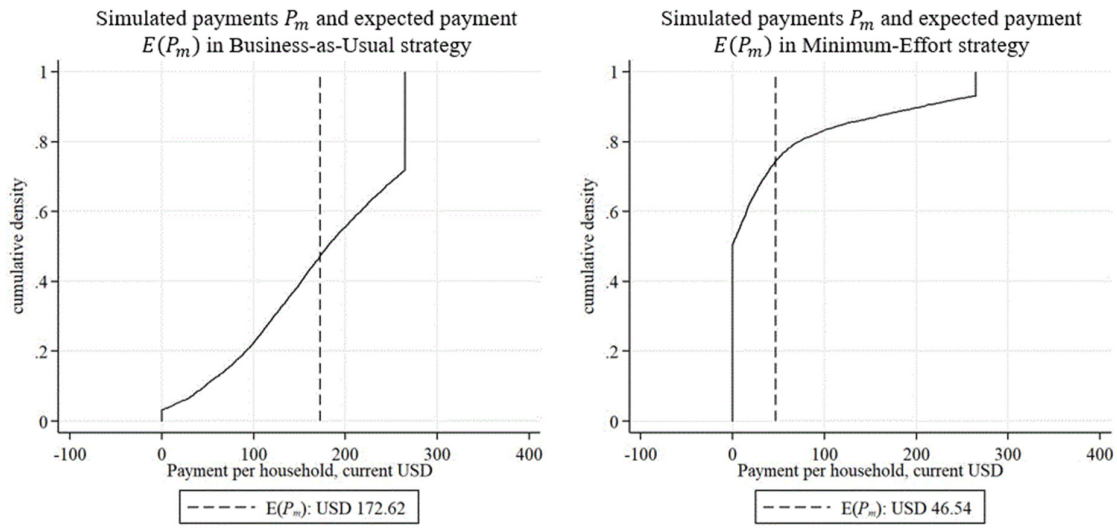
To estimate the present value of permanent consumption, modeling assumptions are required. It was assumed that trends in consumption impacts are proportional to trends in assets impacts, which is consistent with the organization's theory of change: that the maintenance of microenterprise assets defines households' long-term poverty reduction. This model can however collapse if we venture beyond the organization's theory of change. For instance, the implementer could game the system in principle by encouraging participants to dissave some tangible assets around the time of survey measurement so that inflated consumption gains temporarily materialize. The implementer could also discourage households from investing in intangibles that cannot lead to an increased consumption flow within the measurement period (say, investments in the education of young children).

*Under-Investment*

As was illustrated in Figure 45, effects will in fact be measured with error, which may well make the payment bounds binding. What we have not considered to date is that this could trigger incentive processes that distort the investment decision in the first place. To avoid detailed assumptions about the implementer's production function, let us focus on the basic intuition. Assume that the implementer had not approached investors for working capital and had neither invested in cash transfers nor in intangible extensions, and that the true impact of its activities was therefore zero. Let us call this the "Minimum-Effort" strategy. The right hand side of Figure 46 simulates the outcomes of this strategy. We can see that the lower payment bound is now very likely to be binding: in 50% of simulations, it "insures" the implementer against negative outcome measures. Meanwhile, in the 50% of cases in which sampling error goes in the implementer's favor, a payment is made although no true effect could have materialized. As already illustrated in Figure 45, the expected net payout to the implementer in a Business-as-Usual strategy amounts to only USD 12.65 per household once the investor has been compensated. But as no investors

must be compensated in the Minimum-Effort strategy, the implementer could expect to retain the full payment of USD 46.54 quantified in Figure 46.

**Figure 46: Simulation of Payments under Alternative Implementation Strategies**




Note: Using data from the previous trial, the chart on the left displays the distribution of payments resulting from of 5,000 simulations of the processes $a_m = \hat{a}_m + \varepsilon_{a_m}$ and $c_m = \hat{c}_m + \varepsilon_{c_m}$, where $\varepsilon$ is normally distributed with a mean of zero and a standard deviation of $\hat{\varepsilon}$ (i.e., the respective errors that were associated with point estimates $\hat{a}_m$ and $\hat{c}_m$ in the previous trial). The chart on the right displays the distribution of payments resulting from of 5,000 simulations of the processes $a_m = 0 + \varepsilon_{a_m}$ and $c_m = 0 + \varepsilon_{c_m}$.

More generally, the introduction of sampling error raises expected payments when true impacts are low and reduces expected payments when true impacts are high. The more the implementer invests, the lower the insurance value of the payment floor and the higher the disincentive generated by the payment cap.

*Over-Investment*

An incentive in the opposite direction could appear if we take reputational considerations into account. An implementer who wishes to build a reputation of cost-effectiveness could be tempted to provide the project with hidden subsidies, e.g. in the form of indirect support not accounted for in overhead budgets, that remain invisible to the other parties involved. This concern is elevated in the case of nonprofit implementers who can allocate any unrestricted grant funds as they please. It could render transaction data inefficient, underestimating the true social cost of achieving the outcomes. This appears difficult to avoid, or even measure, in the short run: the transparent costing of managerial and similar intangible contributions is a tall task.

### *SUMMARY*

The central policy challenge at hand does not involve information asymmetries, but fundamental knowledge gaps that make it difficult to generalize from available pilot evidence to projections of impact at scale. The potential contribution of PbR is its evolutionary justification: it has the potential to introduce a feedback mechanism between the outcomes achieved and the implementer's financial standing. If funders cannot confidently grasp the necessary conditions for impact, appraise their existence and role in new settings, and generalize from evidence that is available ex ante, they might instead leave it to the reinforcement effects of PbR to "learn" on their behalf by promoting propagation where success happens to materialize. For this process to function, payments should be tied to the ultimate outcome objective, "as if" to incentivize performance. Prima facie, the studied reward function achieves this.

However, working capital requirements led the implementer to seek a financing solution that transferred the incidence of most risks and rewards to third parties, thereby weakening the link between the outcomes achieved and the rewards retained by the implementer. Further, the implementer is a nonprofit that could subsidize its activities in diverse ways not governed by the laws of business economics: e.g., by obtaining non-market financing conditions or other forms of subsidies. The evolutionary justification for PbR would seem stronger if the implementers were strictly for-profits competing in open markets. That said, if we shift to the assumption of self-interest, other problems become pressing. A rogue implementer could take advantage sampling error to game the system, privatizing upsides and socializing downsides. In the studied arrangement, tight payment bounds make "investing little and hoping for the best" a lucrative short-term strategy in expectation. The concern did not end up materializing because the implementer's independent negotiations with investors led to an agreement that mandates a minimum investment per beneficiary household. But to mitigate this issue in any future iterations, payment bounds should be expanded. A reduction in sampling error would also help; as this can be accomplished with larger samples, the concern tends to fall somewhat with greater scale.

We argued that another potential advantage of PbR is its tendency to force funders to pay adequate attention to the opportunity cost of capital, and to force implementers to align their actions with a quest for cost-

effectiveness. While our case format cannot assess this argument comprehensively, it does illustrate in detail how this process can function: funders consult available evidence to establish the opportunity cost of capital; use this to shape their willingness to pay for outcomes; commit and escrow funds; and leave it to implementers to deliver. This speaks to an ongoing debate in the aid effectiveness space. So-called cash benchmarking establishes to what degree different interventions are able to outperform the impacts of cost-equivalent unconditional cash transfers (Center for Global Development, 2018). But in the long run, the most appropriate benchmark is not the estimated impact of some arbitrary intervention – even one that may appear as "generic" as plain cash – but the estimated impact of the best alternative use of capital. Further, the appropriate benchmark for development interventions is not one impact estimate at one arbitrary point in time, but an economic rate of return that accounts for long-term impact patterns. The studied payment function puts these principles front and center.

The idea of using PbR as a tool to help overcome knowledge gaps is a systemic one whose promise can unfold only through iteration and institutionalization. If we assume that donors' outcome challenge to the implementer will be expanded beyond the funded seven cohorts, and that the capital market conditions will remain as they have been, then we have the makings of a mechanism that would allow the implementer to scale up indefinitely, so long as impacts do not deteriorate. This is one potential answer to the challenge of scaling up graduation with fidelity. Over time, funders could challenge further organizations and adjust the required rates of return to account for improved insights about the opportunity cost of capital; in the long run, only the most cost-effective approaches would remain economically viable, whether or not they still resemble graduation. Required rates of return could be adjusted to allow for different valuations in different populations; "rate cards" might show donors' willingness to pay by outcome and setting. For such visions have a chance at realization, a number of kinks need to be worked out. In particular, reward functions should account for how funders, implementers, and working capital providers share the risks associated with sampling error.

***APPENDIX: DERIVING PAYMENT $P_m$***

Let us first derive that payment which, if made in period $m$, aligns the present value of all payments exactly with the present value of consumption impact. Let us call this the unedited payment function $P_{mU}$:

$$\text{(XI)} \quad P_0 + \frac{P_{mU}}{(1+\beta)^m} = \sum_{i=1}^{\infty} c_i \left(\frac{1}{1+\beta}\right)^i$$

Because of measurement constraints, consumption is only measured once in period $m$ and expanded across time using growth rate $g$:

$$\text{(XII)} \quad P_0 + \frac{P_{mU}}{(1+\beta)^m} = c_m \sum_{i=1}^{\infty} (1+g)^{i-m} \left(\frac{1}{1+\beta}\right)^i$$

This implies that the representative participant experiences positive incremental consumption as early as period 1. The growth of consumption impact is assumed to follow that of asset impact with constant returns to scale, growing (or declining) at the same rate into perpetuity. In period $m$, the trajectory of monthly consumption impact crosses level $c_m$. For later simplification, we now separate the consumption stream into that portion which materializes in periods 1 through $m$ and that which materializes in periods $m+1$ through $\infty$:

$$\text{(XIII)} \quad P_0 + \frac{P_{mU}}{(1+\beta)^m} = c_m \sum_{j=1}^{m} (1+g)^{j-m} \left(\frac{1}{1+\beta}\right)^j + c_m \sum_{k=m+1}^{\infty} (1+g)^{k-m} \left(\frac{1}{1+\beta}\right)^k$$

We know from *Equation II* that $P_0$ provides the implementer with the value $a_0$, so we substitute:

$$\text{(XIV)} \quad P_{mU} = -a_0(1+\beta)^m + c_m \sum_{j=1}^{m} \left(\frac{1+g}{1+\beta}\right)^{j-m} + c_m \sum_{k=\mathrm{m}+1}^{\infty} \left(\frac{1+g}{1+\beta}\right)^{k-m}$$

We can simplify to:

$$\text{(XV)} \quad P_{mU} = -a_0(1+\beta)^m + c_m \sum_{j=1}^{m} \left(\frac{1+g}{1+\beta}\right)^{j-m} + c_m \sum_{k=1}^{\infty} \left(\frac{1+g}{1+\beta}\right)^{k}$$

The third term on the right hand side represents an infinite-horizon valuation problem (Gordon & Shapiro, 1956). We can solve it as follows. Given that:

(XVI) $\sum_{k=1}^{\infty} \mu^k = \mu + \mu^2 + \cdots + \mu^{\infty}$

And:

(XVII) $\mu \sum_{k=1}^{\infty} \mu^k = \mu^2 + \cdots + \mu^{\infty}$

we can see that if $-1 < \mu < 1$, the following holds:

(XVIII) $\sum_{k=1}^{\infty} \mu^k - \mu \sum_{k=1}^{\infty} \mu^k = \mu$

Re-arranging:

(XIX) $\sum_{k=1}^{\infty} \mu^k = \frac{\mu}{1-\mu}$

Therefore, under the assumption that $-1 < \mu = \frac{1+g}{1+\beta} < 1$, we know that $\sum_{k=1}^{\infty} \left(\frac{1+g}{1+\beta}\right)^k = \frac{\frac{1+g}{1+\beta}}{1-\frac{1+g}{1+\beta}} = \frac{1+g}{\beta-g}$ and we can simplify to (XVI) to

(XX) $P_{m_U} = -a_0(1+\beta)^m + c_m \sum_{j=1}^{m} \left(\frac{1+g}{1+\beta}\right)^{j-m} + c_m \frac{1+g}{\beta-g}$

$P_{m_U}$ aligns the present value of all payments exactly with the estimated present value of estimated consumption gains.

There are a few discontents with $P_{m_U}$. First, it can give rise to outlier payments – extreme (potentially infinite) liabilities for either payers or implementers in case of extremely positive or negative results. Second, it is undefined for extreme values of $a_m$.[65] Third, it has an unwelcome property: for low estimates of $a_m$, a further reduction in $a_m$ results in higher $P_{m_U}$. (This emanates from the assumptions about the link between asset growth and consumption growth, which in the short run are not as benign as they may appear.[66]) Fourth, *Equation XX* is complex and not easy to contract upon.

[65] This is because of the assumptions that needed to be made in the process of defining a finite $P_{m_U}$; namely, that $a_m > 0$ and that $-1 < \frac{1+g}{\beta-g} < 1$.

[66] As discussed, the project assumes that all assets are productively invested and that their returns are constant to scale and consumed, so that $g$ (which is derived from asset growth rates) can approximate growth rates in consumption. So in Equation XX, all else equal, a lower $a_m$ (and lower $g$), yields lower projections of future consumption (beyond

*Equation III* assumes that for the duration of the project – so in periods 1 through $m$ – rates $g$ and $\beta$ are zero. This simplifies the first two terms of *Equation XX*, which are no longer subject to discount nor growth rates. Further, by introducing scenarios A and B, *Equation III* defines payments for extreme values of $a_m$. The result is an edited payment function labeled the edited payment function $P_{m_E}$ – a better behaved and fully defined alternative to $P_{m_U}$. It is defined as follows:

$$\text{IF} \quad a_m \leq 0 \quad \rightarrow \quad \text{THEN} \quad P_{m_E} = P_{m_A}$$

$$\text{(XXI)} \quad \text{IF} \quad a_m \geq a_0 \quad \rightarrow \quad \text{THEN} \quad P_{m_E} = P_{m_B}$$

$$\text{IF} \quad a_0 > a_m > 0 \quad \rightarrow \quad \text{THEN} \quad P_{m_E} = P_{m_C}$$

For a definition of $P_{m_A}$, $P_{m_B}$, and $P_{m_C}$, consult *Equations IV – IX*. As $P_{m_E}$ can continue to result in outlier payments, *Equation X* adds bounds that were negotiated by the funders and the implementer to remove tail risks, resulting in the final payment function $P_{m_F}$, as described in *Equation X*.

period $m$) but higher estimates of accumulated consumption (in periods 1 through $m$). For very low levels of $a_m$, the latter effect outweighs the former, so a further reduction in $a_m$ increases the implied net present value of consumption and therefore payments.

# *CONCLUSION*

## *FINDINGS, RECOMMENDATIONS, AND QUALIFICATIONS*

Let us return to the questions that were laid out in the introduction of the thesis.

**Question 1. Can the graduation approach be simplified? More specifically, can the intangible component be cut, distilled, or eliminated without adverse consequences?**

Like Berge et al. (2015), Chowdhury et al. (2017), and Banerjee et al. (2018), Chapter 1 found that combining transfers with intangibles (in the form of training and/or mentorship) was more impactful than providing transfers alone. In our study, the pure cash transfer arm received proportionally higher transfers to account for the savings from eliminating the intangible components; this strengthens the relative appeal of the integrated program. Ample caution is however warranted. First, Karlan et al. (2015) and Fiala (2018) do not find that intangibles increased the impacts of transfers. Second, realized costs differed somewhat from budgeted costs; the overhead expenses associated with randomized implementation were higher than expected, and it is appropriate to allocate an outsized proportion of overhead costs to the more complex (i.e., the integrated) program variant. The integrated and the cash-only variants were therefore only *approximately* cost-equivalent. Third, cash transfer recipients responded to the mid- and endline surveys at higher rates than participants in the integrated program, and our findings are not robust to extreme assumptions about the socioeconomic nature of differential attritors. Fourth, there is a choice of alternative plausible counterfactuals, and the estimated impacts depend on this choice. Finally, we are concerned about the possibility of differential misreporting: while it is normal and widespread to collect outcomes in the form of self-reported survey measures even from respondents who had previously also been targeted for support on the basis of other self-reported survey measures, our results raise the question if this might lead to strategic responses.

Consistent with Banerjee et al. (2018), we found that a savings intervention alone does not account for the success of the integrated program, and consistent with Karlan et al. (2017) we found that it did not even lead to a detectable increase in savings. But households that were trained on the creation and management of savings groups did tend to report higher microenterprise activity and improved community perceptions.

Productive cash inflows are higher with borderline significance and point estimates of impacts on consumption are higher as well (though not significantly so). Overall, we see no basis to recommend the elimination of the savings component.

We also attempted a more radical approach to simplify the integrated intervention by distilling, with the help of a psychologist, the extensive intangible (training and mentorship) intervention into a light-touch psychological one. We found that while adding this light-touch intervention to cash transfers tended to manipulate psychological outlook and increase investment in productive assets, it did not increase consumption (indeed, point estimates are negative). We find no basis to recommend the substitution of training and mentorship components with this intervention. Caution is warranted here as well: details of intervention structure and implementation context undoubtedly matter. Unlike us, Campos et al. (2017) find that a psychologically inspired training program (aimed at promoting personal initiative among microentrepreneurs in Togo) outperformed traditional business training for microentrepreneurs.

Summing up, we identify no specific opportunity to simplify the integrated approach without sacrificing impact. We recommend that graduation implementers eliminate program components only in the context of further evaluation. Our evidence provides some support for efforts that promote the graduation approach, including by scaling up intangible extensions in unconditional cash transfer programs. But we urge policymakers not to presume that the impacts found in pilot studies will necessarily replicate in scaled-up contexts. Continued evaluation and flexibility remain important.

**Question 2. How might the transfers and the intangible components interact?**

At first sight, the high impacts of the integrated microenterprise program in Chapter 1, alongside the low impacts of plain cash transfers, appear to vindicate the presumption of the implementer (and the graduation approach more generally) that transfers and intangibles should be provided in conjunction because they are complementary. That said, as we did not test the intangible component without the transfers, we cannot specifically establish this complementarity; some training and coaching activities might have been independently impactful. In Chapter 2 we did investigate one potential mechanism that could have established complementarity: we tested the hypothesis that transfers create cognitive bandwidth among the

poor and thereby facilitate the cultivation of intangible assets. Though this would suggest that the joint impact of the interventions is higher when training and mentorship take place *after* a transfer, we did not find this to be so. Unlike Mani et al. (2013) and like Carvalho et al. (2016), we did not find that cash inflows altered cognitive performance in the first place.

If a decision-maker is forced to choose between implementing a cognitively demanding task with the poor just before or just after a cash transfer, we weakly recommend doing so after - but based only on statistically insignificant point estimates, and only on the assumption that doing so is no more costly than implementing in the opposite order.

**Question 3. When PbR is used to scale up integrated poverty alleviation programs, what features should the grant agreements have?**

Chapter 3 argued that PbR has the potential to address the central epistemic challenge we are facing in the scale-up of the integrated graduation approach, as it may introduce selection pressures that promote the propagation of successful implementers and program structures (even where the reasons for success may not be well understood). We used a demonstration case to argue that widespread concerns about transaction costs can be addressed through deliberate design features (specifically, through the initial pooling and escrowing of outcomes funds), and that motivation crowding effects may be preventable as well (specifically, through a communication strategy that is grounded in behavioral insights). But describing the reward function in detail also highlighted difficulties. It is challenging to tie rewards to evidence of poverty alleviation in practice because (a) credible impact measurement comes with sampling error, weakening the link between impact and reward (and worse, creating opportunities for gaming); (b) the incidence of monetary rewards may not ultimately lie with implementers, but with their working capital providers; and (c) nonmonetary (e.g., reputational) rewards come into play that can be hard to measure and calibrate.

We support continued experimentation with PbR in the poverty alleviation space. It will be important to tighten the link between impacts and rewards, which could be partly achieved by expanding payment bounds and reducing sampling error. We recommend that outcome funders frame their communication surrounding PbR in a way that does not imply a mismatch in incentives, but simultaneously recommend

designing reward functions "as if" to incentivize performance. All else equal, a reward function that more closely links rewards to ultimate outcome objectives is both better at incentivizing impact and at selecting for it.

### *AREAS FOR FUTURE RESEARCH*

Like most empirical findings, ours would benefit from replication in different contexts. In addition, a number of questions have emerged:

(1) **Does the graduation approach in fact lose impact at scale?** Much of our work was built on the presumption that the impacts of integrated graduation programs will suffer with greater scale and in weaker institutional contexts. Is this borne out? We are aware of one ongoing large-scale randomized evaluation of the graduation approach in partnership with governments across Africa's Sahel region (Bossuroy et al., forthcoming); many more would be useful.

(2) **What is the full potential of the behavioral design of cash transfers?** While we failed to establish that engaging with cash transfer recipients in a psychological exercise was impactful, the impacts in any given setting will depend on many details of both intervention structure and context. Further research is currently happening on the psychological engagement with cash transfer recipients (Bossuroy et al., forthcoming; Datta, Martin, & Rawlings, forthcoming; Orkin et al., forthcoming) and may come to different conclusions.

(3) **How does the theory of scarcity fare under further replication?** While Mani et al. (2013) and Ong et al. (2019) provide empirical support, Carvalho et al. (2016) and our findings do not. Further studies might help illuminate under what conditions the theory does and does not hold.

(4) **When poverty alleviation interventions are targeted on the basis of survey data, does this increase strategic misreporting?** It is known that being surveyed can introduce bias by making certain options more salient (Zwane et al., 2011). Another potential source of bias is that respondents may under-report their economic status in the hope of increasing the odds of eligibility for possible future benefits. Though this phenomenon has been considered and not well corroborated (Baird & Özler, 2012; Moore et al., 2000), strategic misreporting – especially differential misreporting by

treatment arm – would pose a major risk to many contemporary poverty evaluations. It would be useful to study under what conditions targeting exercises cause respondents to later misreport their economic status.

(5) **How to make a poverty reward function fully incentive-compatible?** Be it "so as" or "as if" to create incentives, outcome funders should aim to create a tight link between the outcomes of an intervention and the rewards that allow the implementer (and approach) to propagate. An improved reward function would remain incentive-compatible in the face of sampling error and a wide range of working capital arrangements. Ideally it would also account for intangible rewards, such as the formation of reputational capital.

(6) **How best to project trends in consumption impacts from limited data?** We argued that valuation models should not presume that short-term consumption impacts will necessarily persist. This assumption creates an uneven playing field, inflating the benefits of interventions that maximize short-term consumption over those that encourage investment. We attempted to project long-term consumption trends as a function of asset formation, using intervention data plus one household survey. This relied on strong assumptions about the nature of investment and consumption. Can this assumption be empirically justified? Can it be improved upon?

(7) **How to improve the precision of asset and consumption measurement?** As the sampling error associated with consumption and asset measurement created major difficulties for the studied PbR arrangement, what techniques might help reduce this error (given a fixed evaluation budget)? A relevant contribution by McKenzie (2012) describes how the statistical power of randomized evaluations in the development sector can be optimized by trading off the number of respondents against the number of survey rounds. Another contribution by Beegle et al. (2012) illustrates opportunities to simplify consumption measurement, at least for a subset of respondents. It would be useful for many purposes, including PbR, to expand upon this literature. For instance, under what conditions might it be sensible to collect multiple consumption and asset proxy measures in a single survey round?

(8) **Can the behavioral design of outcome-based funding agreements in fact prevent motivation crowding?** We argued that by framing outcome-based transactions in a way that does not imply a mismatch in incentives, it may be possible to disable the behavioral mechanism that causes motivation crowding. This hypothesis could be empirically studied through relatively simple lab experiments.

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