CSAE Working Paper WPS/2017-15-2

Cash-Plus: Poverty Impacts of Transfer-Based Intervention Alternatives

24 January 2018

Richard Sedlmayr ⸸ Anuj Shah † Munshi Sulaiman ‡

Can programmatic extensions such as training and mentorship enhance the economic impact of cash transfers, or do they needlessly absorb resources that program recipients could allocate more meaningfully by themselves? Using a randomized trial, we evaluate a program that targets poor Ugandans and offers them an integrated package comprised of lump sum transfers, coaching, and training on microenterprise development as well as savings group formation. We assess its impact and that of its savings component, as well as the impacts of much simplified program variants: one intervention variant that is limited to lump sum cash transfers and another that expands upon transfers using a light-touch behavioral intervention component. The results support the notion that integrated development interventions are sensible poverty reduction tools.

Keywords: graduation, microenterprise, cash transfers, behavioral design

JEL Codes: O12, O22, O35, I38

Authors: ⸸ University of Oxford. † University of Chicago. ‡ Save the Children. RS and MS co-designed the trial. AS took responsibility for the design of the behavioral intervention. MS supervised field work. RS designed and conducted the analysis and wrote the paper. Correspondence: richard.sedlmayr@sant.ox.ac.uk

Acknowledgments: This study could not have materialized without the extensive contributions of Rachel Proefke. Sam Gant effectively led the local piloting and roll-out of the behavioral intervention. Other critical contributions were made by Dianne Calvi, Caroline Bernadi, Konstantin Zvereff, Winnie Auma, AJ Doty, Celeste Brubaker, and the team at Village Enterprise; by Dustin Davis, Agne Pupienyte, Harrison Pollock, Paola Elice, Samuel Rosenow, and the team at Innovations for Poverty Action; and by the team at the Independent Research and Evaluation Cell at BRAC Uganda. We benefitted from relevant conversations with Richard Tugume, Kate Orkin, Rob Garlick, Johannes Haushofer, Natalie Quinn, Berk Özler, Paul Sparks, Juan Camilo Villalobos, Stefan Dercon, Michael Kremer, Dean Karlan, and the participants of the Buffett Institute Global Poverty Research Lab's Gathering on Financial inclusion and Social Protection.

Transparency and Replicability: Data, cleaning code, analysis code, scripts, surveys, and other supplementary materials are archived on the Open Science Framework (osf.io/mzrkx). The study was registered on the Registry for International Impact Evaluations (ID 52bb3799ccf6a).

Disclaimer: The impacts presented here may differ from those that will be measured in the context of a future impact bond with Village Enterprise for many reasons, including program design, research design, general equilibrium effects, service provider performance, and diverse other factors including random chance.

INTRODUCTION

Motivation

Much development assistance takes decisions on behalf of those it aims to serve. Take the growing class of integrated poverty alleviation programs that target poor households in low-income countries and provide them with a package of livestock and/or lump sum transfers, as well as training and mentoring. Such programs have been presented under different labels—including microenterprise development, livelihood development, or ultra-poor graduation—and may differ in some design features. But they operate on the shared theory that an inflow in assets will enable beneficiaries to establish micro-enterprises, and that training and mentorship will prepare them to maintain the assets and derive benefits from them over time. Implicit in this theory is the belief that making some investments on behalf of beneficiaries—especially in their human capital—helps improve outcomes.

Skeptics may point out that development practice has a long history of paternalistically misallocating resources by transacting without the substantive involvement of those it purports to serve (Easterly, 2007). Why not give beneficiaries expanded agency over program resources – say, by expanding the monetary transfer portion of the program and allowing beneficiaries to invest as they see fit? If investment choices made by the poor differ from those envisioned by development practitioners, it may be because their preferences are different (Das, Do, & Ozler, 2005).

Of course, if we interpreted these investment decisions as revealing the preferences of well-informed and rational agents in functioning markets, it is hard to see a case for restricting choice; but there are some grounds to question if these are appropriate assumptions. The markets for human capital cannot be characterized as fully functional (Stiglitz, 1989), and transfers are unlikely to achieve optimal outcomes in the presence of market failures.[1] By defining a set of activities that is tailored to the expected needs of

[1] Transfer programs that target entire communities have also repeatedly failed to achieve their objectives; see Casey, Glennerster, & Miguel, 2012; Humphreys, Sanchez de la Sierra, & Windt, 2012.

beneficiaries, and by delivering it presumptively and at scale, Village Enterprise may be providing a valuable service that would be impossible or exceedingly costly for beneficiaries to procure on the open market. Even if such services were available and reasonably priced, people might underinvest in human capital if they are uninformed (Jensen, 2010), inattentive (Hanna, Mullainathan, & Schwartzstein, 2014), or time inconsistent (O'Donoghue & Rabin, 1999). It has also been demonstrated that the investment decisions of transfer recipients are highly malleable through seemingly trivial interventions, such as the labeling of the transfer (Benhassine, Devoto, Duflo, Dupas, & Pouliquen, 2015), which questions the strength of revealed preference analysis in such contexts.

One principle should be broadly acceptable to both advocates of integrated programs and advocates of light-touch ones: when program variants that expand the agency of the poor achieve even the stated objectives of development practitioners better than more restrictive program variants do, then such an expansion is warranted. Indeed, it has been suggested that the impacts of unconditional cash transfers can serve to benchmark the performance of development investments (Blattman & Niehaus, 2014).

Can elements of integrated poverty alleviation programs indeed be stripped out without adverse consequences for cost-effectiveness on key performance metrics? Existing evaluations of integrated program variants have demonstrated important economic improvements (Bandiera et al., 2017; A. Banerjee et al., 2015; Abhijit Banerjee, Duflo, Chattopadhyay, & Shapiro, 2016; Blattman et al., 2016), but plain unconditional cash transfers have also demonstrated impacts on important markers of economic development (Baird, McIntosh, & Özler, 2011; Haushofer & Shapiro, 2016). If integrated programs can do without training and mentoring, this insight could be easily implemented in the context of existing development practice.

The insight would also be important from the perspective of delivery science. Generalizing from past evaluation results calls for an awareness of the contextual factors that moderated the effects in the original settings, and of their role in the new and different settings (Cartwright & Hardie, 2012; Deaton, 2010). One

such factor might be the quality of implementation, especially that of components involving a major variable "human element" such as training and mentoring. If it correlates negatively with the scale of implementation, pilot settings will yield inadequately optimistic policy predictions (Bold, Kimenyi, Mwabu, Ng'ang'a, & Sandefur, 2013; Pritchett & Sandefur, 2013). Given that past evaluations took place in modestly scaled contexts of nonprofit programs, there are reasons to be concerned that integrated poverty alleviation programs may no longer work when they grow very large – say, get consistently adopted by governments. In the light of such concerns, a reduction in the complexity of interventions should be welcome: all else equal, a simpler intervention (say, one with fewer training and mentorship sessions) will tend to be delivered with greater fidelity.

Programmatic Context

Village Enterprise is a nonprofit organization that implements microenterprise programs in Uganda and Western Kenya. Its core program has parallels to the interventions studied in Banerjee et al. (2015) in that it uses a participatory targeting process as well as a proxy means test to identify the poorest households and then provides one of their representatives with a combination of transfers, mentorship, and training. However, the Village Enterprise program has several distinguishing features. It is relatively short in duration, with training sessions taking four months, mentorship engagement taking nine months, and the overall program concluding 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 their business activities as partnerships with other households (target size: three households). The program also establishes village-level savings groups (target size: thirty households) that provide basic deposit 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. (That said, the program does include a training session on environmental conservation that is not widespread in other poverty relief

programs.) Coaching is run by designated business mentors and focused specifically on matters of micro-enterprise administration. The transfer component of the program is delivered not in the form of physical assets, but cash. Transfers are made to the business partnership, as opposed 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).

Research Framework

Our research aimed to deepen insights on several questions, all of which serve to speak to the broader challenge of delivering integrated poverty alleviation programs effectively and at scale.

One line of inquiry aims to establish the impacts of alternative program variants. On the one hand, we evaluate an integrated program that provides a package of transfers, training, and mentorship; on the other hand, we evaluate a dramatically simplified program that monetizes the cost of training and mentorship and thereby maximizes the resources transferred to participants in the form of cash. Based on the evidence base of so-called graduation programs, which are more intensive but similar in spirit (Bandiera et al., 2017; A. Banerjee et al., 2015), we expected that the integrated program variants program would orient the productive activities of poor households towards microenterprise administration and lead to sustained improvements in markers of economic as well as subjective well-being. Meanwhile, based on previous work by Fafchamps, McKenzie, Quinn, & Woodruff (2012), we expected that providing unconditional cash transfers would tend to relatively lower initial investment in productive assets, leading to higher short-term consumption but lower long-term consumption.

Another line of inquiry involves marginal extension components that may help alter the cost-effectiveness of alternative variants. In the microenterprise program variant, we evaluate the savings group component;

at the time of program design, evidence on such interventions was modest (Gash & Odell, 2013), and expected that savings groups would alter measures of financial inclusion but not more fundamental standards of living. In the cash transfer program variant, we explore a light-touch extension that could be implemented with minimal constraints on participant agency. It has been suggested that targeting mental constructs, such as aspirations, can have economic impacts (Bernard, Dercon, Orkin, & Taffesse, 2014). Indeed, a large-scale development intervention that disbursed cash upon business plan submission turned out to yield remarkable poverty alleviation effects (Blattman, Fiala, & Martinez, 2014). We hypothesized a causal interaction: in the words of Lybbert & Wydick (2016), that addressing "internal" constraints may be especially impactful at times when more tangible interventions overcome "external" ones. We therefore set out to evaluate the impact of a behavioral feature that added goal-setting and plan-making to the transfers.

We then directly benchmark integrated microenterprise and cash transfer variants against each other; while to date there is experimental evidence on both intervention variants, there is little to no research comparing them in a given setting (Sulaiman, Goldberg, Karlan, & de Montesquiou, 2016). Further, we investigate spillover effects; while these are not a central subject of the analysis, they help in the selection of appropriate counterfactuals.

In the light of a vivid debate about threats to the validity of insights in empirical social science, we are compelled to address two concerns that are relevant to our research. One concern is that much economic research may not be adequately powered (Ioannidis, Stanley, & Doucouliagos, 2017). Superficially, aspects of our work are susceptible to this – some more so than others. For example, the cash transfer arm had access to fewer implementation resources than the microenterprise arms. (The evaluation was designed in the light of operational needs and constraints: selected insights—e.g., on the impact of removing savings modules from the microenterprise program—were expected to be directly actionable for the implementer, while others—e.g., on the impact of adding a psychological intervention to a cash transfer program—were further removed from the current program and called for dedicated evaluation resources.) Sample sizes

differ across arms, and so does the probability of false negatives. However, the appropriate standards for detectable effects also vary, and it is fundamentally uncertain what some appropriate thresholds may be. For instance, when it comes to the comparison between the transfer and microenterprise program variants, costs were budgeted to be roughly equivalent and it was reasonable to expect that effects would be roughly equivalent as well. Experimentation remains useful: readers might put little weight on the null hypothesis, but interpret results in the light of their prior expectations.

A second concern is that researchers can be incentivized to drift towards analytical choices that deliver significant, compelling, or otherwise welcome results, raising the risk that these turn out to be spurious (Miguel et al., 2014; Nosek et al., 2015). In the case at hand, the breadth of the research design and data set provides ample opportunity to engage in data mining and cherry-picking. One tool that has been proposed to curb these concern is the registration of a so-called pre-analysis plan. But this comes with costs (Olken, 2015), especially to less experienced researchers who struggle to appropriately specify their analysis in the abstract. We explore an alternative approach. After conducting only an undetailed registration at the outset of the trial and leaving open many degrees of freedom, we try to curtail this freedom by ceding central aspects of the analysis specification to model selection algorithms. On those dimensions where model selection algorithms are not typically used (specifically, on the operationalization of variables), we attempt to ground our choices in a transparent process through the use of so-called specification curves (Simonsohn, Simmons, & Nelson, 2015).

STUDY DESIGN

Sampling, Eligibility, and Assignment

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 each of these villages, Village Enterprise

independently conducted a participatory wealth ranking exercise, followed by a quantitative means test using progress-out-of-poverty (PPI) survey data, to validate eligibility.

A sense of the economic status of eligibles can be gained from *Table 1*. It appears that Village Enterprise successfully targets people whose consumption lies USD PPP 1.90 per capita per day. Our measures indicate the majority of consumption is not derived from income earned in the form of cash inflows from productive activities, which suggests that households derive a significant share of consumption from subsistence or assistance. (Note however that income measures are notoriously difficult to measure in low-income contexts and especially prone to under-reporting; see Deaton, 1997, and Meyer & Sullivan, 2003).

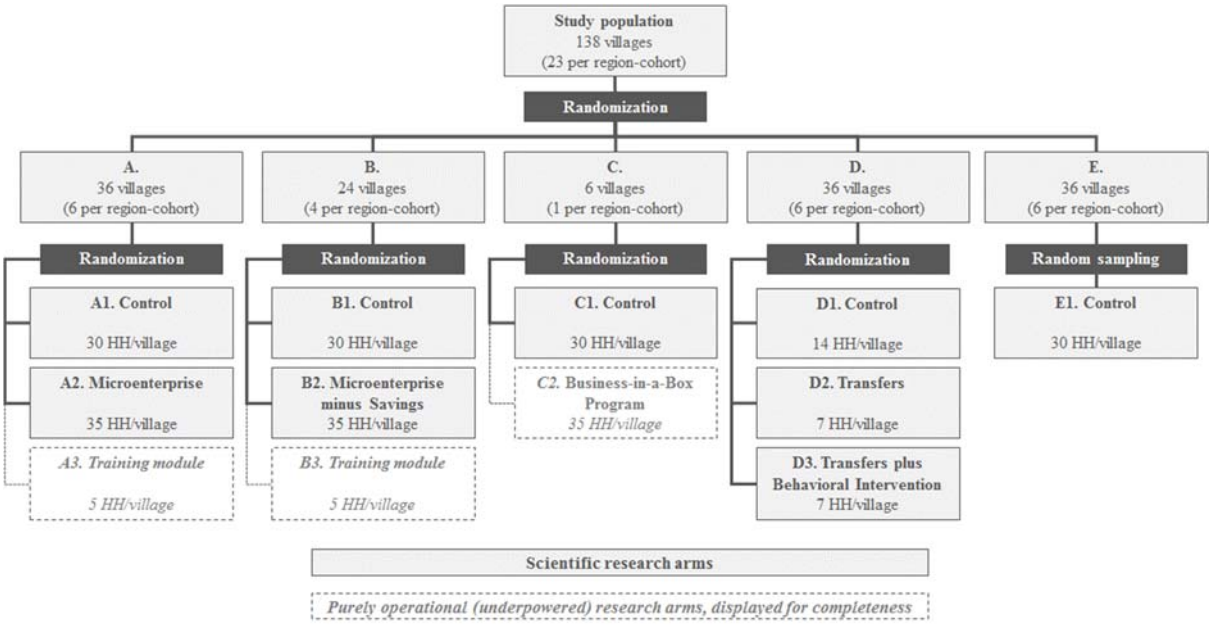
Table 1: Economic Status of Eligibles at Baseline (per capita)

Item[1]	UGX[2]	USD[3]	USD (2016 PPP)[4]
Food & Beverage Consumption	480,197	190.55	451.74
Recurring Consumption	73,306	29.09	68.96
Infrequent Consumption	61,111	24.25	57.49
Total Consumption	**624,072**	**247.65**	**587.09**
Livestock Assets	46,786	18.57	44.01
Durable Assets	46,475	18.44	43.72
Net Financial Position	1,321	0.52	1.24
Total Assets	**98,623**	**39.14**	**92.78**
Net Cash Inflows from Farming	840	0.33	0.79
Income from Other Self-Employment	66,325	26.32	62.39
Income from Paid Employment	94,949	37.68	89.32
Total Productive Cash Inflows[5]	**184,625**	**73.26**	**173.68**

Notes:
(1) As data are derived from baseline survey, they are contingent on study recruitment and survey consent. All flow values are annualized. All items are calculated in accordance with analysis procedures presented below. As these winsorize each outcome individually at the 95% level, sub-composites do not add up to totals. For a more detailed definition of items, see publicly archived code.
(2) Current Ugandan Shillings at time of baseline.
(3) Current US dollars, transformed using exchange rates at baseline. For a discussion of exchange rates, consult the endnotes.
(4) US dollars adjusted to 2016 constant Purchasing Power Parity levels.
(5) Total productive cash inflows exclude income elements in the form of in-kind revenues, in-kind expenses, inflows from non-productive activities (such as remittances or transfers), and accruals.

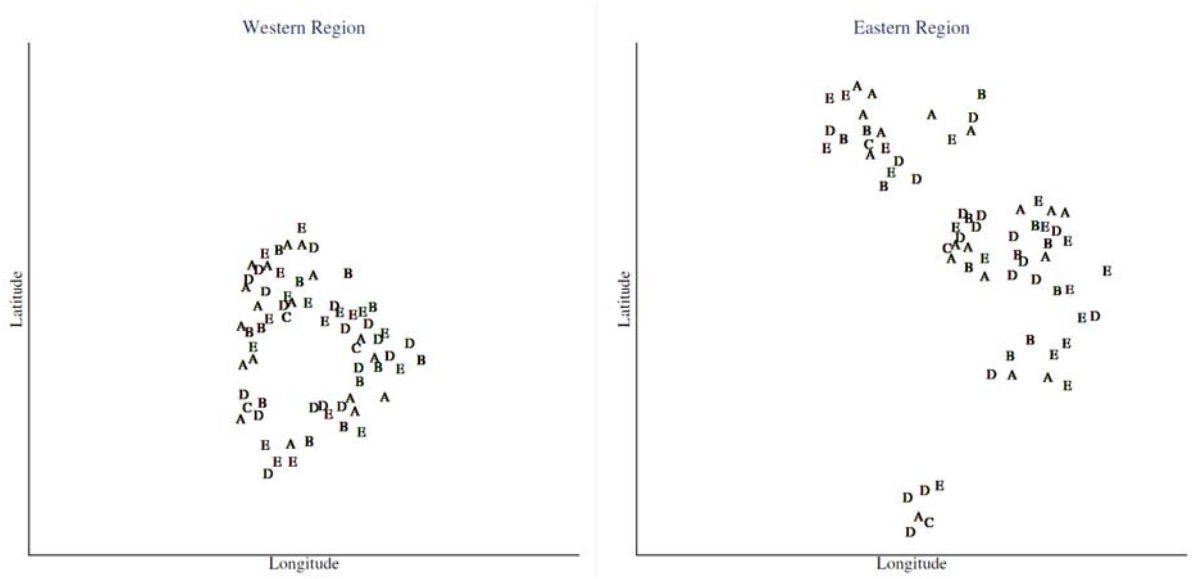
During the PPI survey process, Village Enterprise identified a representative for each household. The resulting list was shared with the research team for randomization. Within each region, three equally sized region-cohorts of 23 villages each were formed, resulting in six region-cohorts. As displayed in *Figure 1*, the randomization was stratified by region-cohort and assigned villages at random to one of five arms, labeled A-E.

Figure 1: Assignment by Arm and Sub-Arm



Eligible participants within each village were further randomly allocated to sub-arms. In 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 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 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 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 E-type villages, 30 households were assigned to controls (E1). *Figure 2* displays the geographic distribution of villages by arm and region.

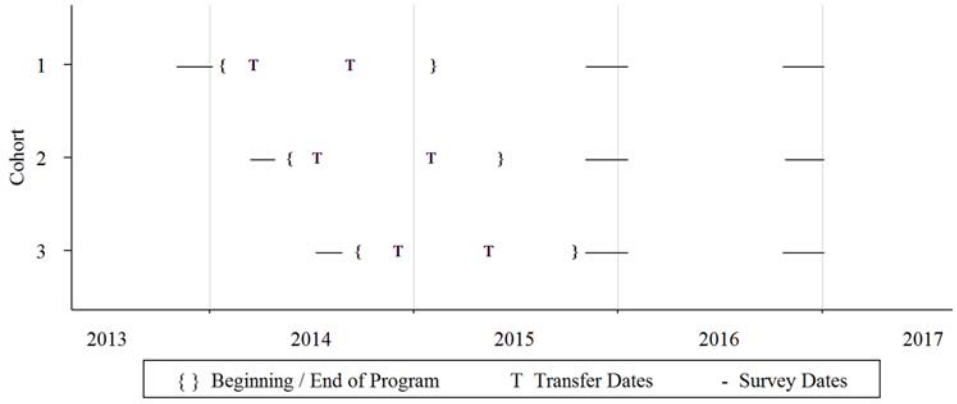
Figure 2: Village Assignment by Arm and Region



Note: Each axis corresponds to 0.9 degrees latitude / longitude.

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 survey were formally recruited into the study. As displayed in *Figure 3*, baseline survey and program implementation were staggered by cohort.

Figure 3: Assignment of Cohorts over Time



Intervention Design and Costing

The standard microenterprise program (sub-arm A2) was the routine program of Village Enterprise, composed of training, transfers, and mentorship. All trainings were administered by a dedicated intervention leader. The training component constituted sixteen 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 lump sum cash transfer of nominal UGX 240k 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 initiated after the first transfer and continued at a monthly frequency.

At the outset of the trial, the direct and replicable cost of the microenterprise program was budgeted at USD 140 (current dollars). Note that budgeted costs differed from incurred costs, partly because of efficiency losses associated with the need to follow scientific protocol. Note also that cost numbers are highly sensitive to assumptions about exchange rates and indirect cost allocation. For a discussion of exchange rate assumptions used in this paper, consult the endnotes.[1] For an illustration of the cost structure of alternative sub-arms, using a retrospective analysis of incurred costs as quantified in financial reports, consult *Table 2*. This displays the intervention field activities, quantifies the time costs of each, and uses relative time intensity of activities to assign costs from internal financial reports to the sub-arms ("activity-based costing"). Only the transfer component is quantified differently (based on its nominal value at intervention time.)

Table 2: 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, USD							
Field delivery costs		5.42	35.88	29.21	37.13	7.93	10.43
Cash transfers		-	45.92	45.92	45.92	119.40	119.40
Subtotal		*5.42*	*81.80*	*75.13*	*83.06*	*127.33*	*129.83*
Other Ugandan program costs		13.05	86.33	70.27	89.34	19.07	25.10
Subtotal		*18.47*	*168.14*	*145.40*	*172.40*	*146.40*	*154.93*
Int'l program & overhead costs		9.72	64.32	52.36	66.57	14.21	18.70
Grand total		**28.20**	**232.46**	**197.76**	238.97	**160.61**	**173.63**
Cost per household, UGX							
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 full accounting of 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) With the exception of cash transfers, all totals are based on internal financial reports of Village Enterprise. Table uses exchange rates at intervention time (see endnotes for a discussion of exchange rates).
(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.

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 Village Enterprise.

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 its activity structure flows into the cost allocation of the other arms. (Sub-arms A3 and B3 were similarly operational in nature; their incremental cost of delivery was however negligible).

Sub-arm D2 involved only unconditional cash transfers. Unlike in the microenterprise program variants, payments were provided not to three-member businesses but to individual 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 as equivalent to the direct cost of the microenterprise program, minus the lowest share of non-transfer costs that was identified in the benchmarking of independent cash transfer delivery initiatives (i.e., 7.4%).

Sub-arm D3 expanded upon the cash transfers described in sub-arm D2 using a light-touch behavioral intervention that attempted to distill relevant literature and evaluate the incremental impact of goal-setting, plan-making, and complementary psychological approaches in a cash transfer program. The intervention was comprised of three sessions, including (a) an introductory discussion alongside the voucher provision

(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 components were derived from literature on mental contrasting and implementation intentions (Gollwitzer, 1999; Oettingen, 2000). Participants also completed self-affirmation exercises to address some of the stigma of poverty and to promote the belief that their goals were achievable (Hall, Zhao, & Shafir, 2014). Participants were asked to think about peers who had been successful, and about ways that they could follow their peers' examples. This was motivated by work on role models (Lockwood & Kunda, 1997) as well as other work on the power of social norms (Cialdini & Trost, 1998) and 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 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 personal consumption and goal pursuit.

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).

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. However, diverse further measures on nutrition, education, health, decision-making, cognitive performance, and community life were also of interest.

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). Cognitive baseline measurement was not successful in the first cohort, and cognitive data collection was abandoned after the baseline. The available data can be gleaned from the survey forms, data sets, and code, all of which are publicly archived except as noted in the *Appendix on Data and Measures*.

EMPIRICAL STRATEGY

Strategy for Poverty Outcomes

As mentioned in the introduction, we start the analysis process with the classification of "choice dimensions" that we expect to be important determinants of the outcomes. We will then use combinations of choice dimensions to establishing a universe of plausible results to derive inferences from. For illustration purposes, consider the following model:

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

Here, y_{ijF} is the per-capita outcome in household i in village cluster 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 β.

'Tests' are defined as alternative combinations of outcomes y and treatment assignments T. Each test has a substantively different interpretation. Choice dimensions here include the following:

(1) **Definition of outcomes**. In defining poverty outcome y, we present each of the three primary financial outcomes (consumption, assets, and cash inflows) in the form of one total composite as well as three sub-composites.

(2) **Definition of outcome rounds**. We define alternative follow-ups F as the first follow-up (midline); the second follow-up (endline); and, following McKenzie (2012), a pooled average value.

(3) **Definition of comparisons**. In defining T, we evaluate six impacts:

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

[b] those of the microenterprise program (both with and without the savings group component) by comparing the set of sub-arms A2 and B2 to untreated controls, which will be selected in the analysis process;

[c] those of the cash transfer program (both with and without the behavioral intervention component) by comparing the set of sub-arms D2 and D3 to untreated controls, which will be selected in the analysis process;

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

[e] those of the behavioral intervention component, conditional on the cash transfer program variant, 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.

This implies 12×3×6 = 216 alternative tests with substantively different interpretations. For each test, there are numerous plausible specification alternatives that may change results but not their substantive interpretation. Some choice dimensions involve those made in course of model selection, e.g.:

(1) **Use of baseline values**. The aforementioned model, which controls for the baseline measure y_B, is not the only plausible approach. Alternatively, one might subtract baseline data from follow-up data and estimate differences in differences, or leave it out of the estimation process altogether.

(2) **Use of socioeconomic covariates**. The available selection of measures to populate set X_{ijB} is large, but the choice can be reduced to 'selecting none' or 'selecting some set'. One plausible set might

involve five socioeconomic baseline characteristics, selected using a selection algorithm such as least angle regression (Efron et al., 2004).

(3) **Use of fixed effects**. The term α_j implies the use of cluster fixed effects. A plausible alternative would be to define α as a constant.

Other choice dimensions relate to the operationalization of variables from the data, e.g.:

(1) **Outlier adjustment**. As the data set is not cleared of outliers and poverty measures are sensitive to them, some adjustment is required. To avoid introducing an attenuating bias, it is most sensible to adjust each combination of y and T separately. But there is discretion in the appropriate level – for instance, one might 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).

(2) **Definition of counterfactual set in controlled comparisons.** As defined above, comparisons [b] and [c] compare a treatment group with controls. But there are different plausible definitions of controls: one might 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 would circumvent adjustments for cluster robustness, with benefits for statistical power, and selecting only control villages would minimize susceptibility to possible bias emerging from within-village spillovers. The third option is a compromise between power and unbiasedness. An appropriate assessment of trade-offs is difficult without data.

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

Multiplying the 216 tests with 2×3×2 alternative models and 3×3×3 alternative operationalizations would yield a total of 69,984 combinations.

However, not every specification choice 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]; this removes 4/9 of conceivable estimates. 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"); this removes 7/20 of conceivable estimates. Third, the use of any valuation other than the respondent's is only appropriate for measures with commodity character (removing 1/3 of conceivable estimates). 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.

To limit the number of applicable specifications, we will rely on model selection processes (for an overview, see Burnham & Anderson, 2010). Specifically, we will use the universe of 16,848 specifications to identify the model that has the strongest support from the data. (Note that as cluster fixed effects are not applicable in specifications involving comparison sets that were randomized at the cluster unit, we allow for a separate model here. Note also that we limit the model selection exercise to poverty outcomes—i.e., consumption, assets, cash inflows composites, and the three sub-composites as displayed in *Table 1*.)

We will calculate the Akaike information criterion (AIC) associated with each estimate and associated specification. AIC quantifies the strength of a model, conditional on the data; we prefer specifications with low AIC, indicating both explanatory strength and parsimony. However, as we do not want to end up with different models for each test, we seek a normalized measure of proportional support which may later be averaged across tests. Following Burnham & Anderson (2004) we define a set of specifications 1 through

K (which in our case corresponds to all specifications within a given test) and compute[2] Akaike weights w_s:

$$\text{(IV)} \qquad w_s = \frac{exp(-0.5AIC_s)}{\sum_{k=1}^{K} exp(-0.5AIC_k)}$$

We will conduct this procedure for all 216 tests, then average specification weights to arrive at model weights that are applicable across tests. The model with the highest weight is most likely to be the best model, given all available data. 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.

It is not standard to extend such model selection processes to issues of variable operationalization; this step therefore involves elevated discretion. To ground it in a transparent process, we build on Simonsohn, Simmons, & Nelson (2015) by developing "specification curves" that visually present the results of a universe of plausible specifications behind a given test.

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 follow-up rounds) across six comparison groups. To account for multiple inference, we control for the false discovery rate (Benjamini & Hochberg, 1995), reporting minimum q values following the method used in Anderson (2008). We apply these adjustments across all estimates within a given comparison group and outcome class. This definition corresponds to our definition of the individual hypothesis.

Strategy for Other Outcomes

For all outcomes other than poverty composites and sub-composites, we will present two specifications. The first is the most basic regression specification; the second is the aforementioned preferred specification. The preferred specification is derived from the aforementioned model selection process for poverty

[2] To avoid the exponentiation of extreme values, the computation $w_s = \frac{exp(-0.5(AIC_s - AIC_{\min}))}{\sum_{k=1}^{K} exp(-0.5(AIC_k - AIC_{\min}))}$ is used in practice, where $AIC_{\min}$ is the lowest measured Akaike criterion in a given set.

outcomes, but does not feed back into this process. (We wish to limit such interdependence to avoid a scenario where the estimates that serve as inputs for cost-effectiveness calculations might be tipped by more exploratory analyses.)

We will apply specifications 1 and 2 to all measures including individual level and binary outcomes. The latter are transformed through the use of logistic regression, and estimates are presented as odds ratios.

As before, we will apply false discovery adjustments for each comparison group and outcome class separately: beyond poverty outcomes, these outcome classes include psychological, nutritional, employment, schooling, savings/loans, health, and community related outcomes. In summary, each table presented in the *Appendix of Tables* corresponds to our definition of an individual hypothesis, and is the subject of a separate false discovery rate adjustment.

RESULTS

Balance Checks, Participant Flow, and Attrition

Table 3: Covariate Balance

Baseline measure	Treatment sub-arms	Control sub-arms	p value	N
HH size	5.96	5.88	0.336	5,774
Age of HH Head	43.01	43.16	0.734	5,575
HH Head's years of schooling	5.32	5.32	0.949	4,586
HH Head is female	28.54%	28.52%	0.989	5,763
HH Head is monogamously married	56.79%	56.14%	0.622	5,763
HH Head is literate	46.69%	46.82%	0.922	5,763
HH has iron roof	26.49%	25.57%	0.432	5,774
HH has mud walls	39.92%	40.25%	0.798	5,774
HH has earth floor	96.78%	96.63%	0.761	5,774
HH has sanitary toilet / latrine	41.39%	40.49%	0.494	5,774
HH uses wood as main cooking fuel	98.61%	98.04%	0.102	5,774
HH uses electric light	2.04%	1.96%	0.819	5,774
HH owns its home	88.00%	87.61%	0.651	5,774
All HH members have two pairs of clothes	61.31%	61.76%	0.724	5,774
All HH members have a pair of shoes	23.39%	23.41%	0.987	5,774

Notes:
- Data are derived from baseline data, so are post baseline attrition.
- The first three variables are continuous (representing averages) and the others are binary (representing proportions).
- p values are derived from simple regression differences. Logistic regression is applied in the case of binary dependent variables.
- Intent-to-treat assignment T is coded to the value one among all households receiving any form of direct treatment (set A2∪B2∪D2∪D3) and to the value zero among all households receiving none (set A1∪B1∪C1∪D1∪E1).
- Standard errors are not adjusted for cluster robustness.

Table 3 presents balance checks on the baseline measures that are subsequently considered as covariates in

applicable specifications. Treatment and control sub-arms are well balanced, with no significant differences emerging on any baseline measure. The first element of *Table 4* ("available participant slots") displays the assignments that were depicted in *Figure 1*. As discussed, only participants who had been successfully baselined 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. As some heterogeneity in attrition rates across arms is apparent in *Table 4*, a test of the significance of differential attrition between treatment and counterfactual groups in the different comparison sets is presented in *Table 5*. Indeed, comparison sets [c] and [f] are consistently afflicted by differential attrition; for these, we will follow the trimming procedures proposed by Lee (2009) in order to put bounds on the treatment effects, repeating the trimming procedures individually for each test. This procedure will be limited to poverty outcomes.

Table 4: Participant Flow

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%

Note:
(1) Attrition is defined as the share of baseline survey participants for whom the corresponding follow-up survey was unsuccessful.

Table 5: Evidence of Differential Attrition by Comparison Set

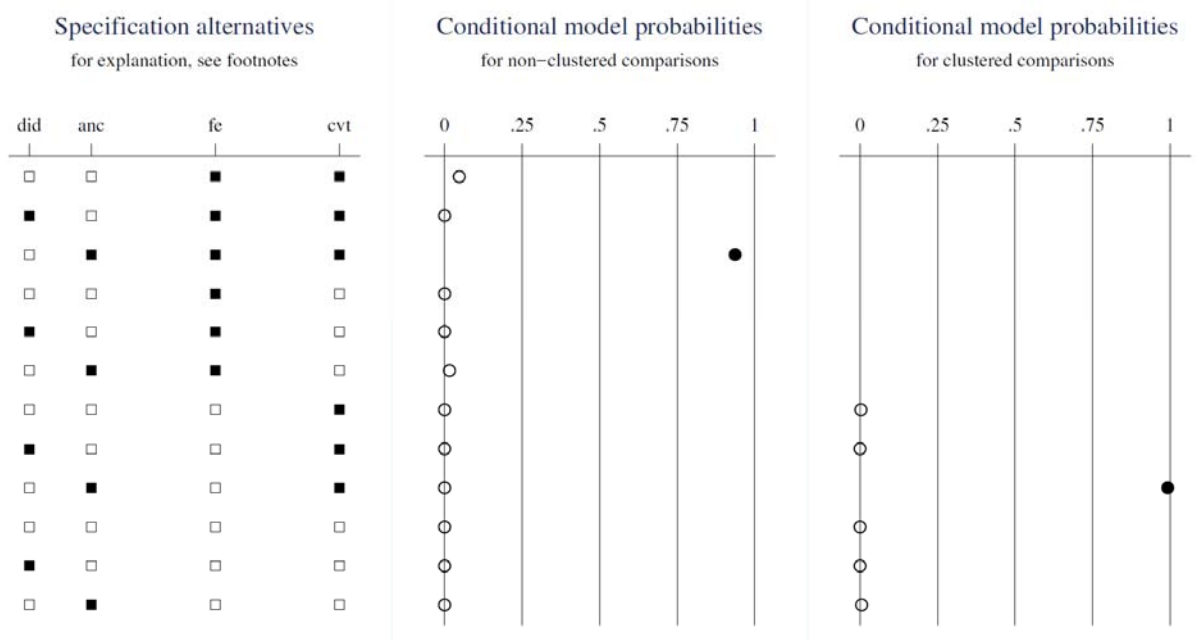
Comparison		Midline							Endline						
		Treatment			Counterfactual				Treatment			Counterfactual			
		Surveyed	Attrited	Odds	Surveyed	Attrited	Odds	p value	Surveyed	Attrited	Odds	Surveyed	Attrited	Odds	p value
[a]		2,164	165	0.076	933	62	0.066	0.530	2,113	216	0.102	915	80	0.087	0.386
[b]	[i]	1,819	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:
- Comparison groups are formed to estimate the impact [a] of spillovers, [b] of the microenterprise program (both with and without the savings group component), [c] of the cash transfer program (both with and without the behavioral component), [d] of the savings group component conditional on the microenterprise program variant, [e] of the behavioral component conditional on the cash transfer program variant, and [f] of the microenterprise program over the cash transfer program. Counterfactual type [i] implies the use of within-village controls, [ii] the use of between-village controls, and [iii] the use of all available controls. For more information, see the chapter on *Empirical Strategy*.
- 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].

Specification Selection

As discussed in the chapter on *Empirical Strategy*, the specification process involves model selection and variable operationalization. We start the model selection process by assigning equal prior probabilities to each model within each test; calculating the Akaike information criterion for each of the 16,848 estimates; and using these inputs to calculate Akaike weights for each estimate. Averaging these weights across clustered and non-clustered tests, we find the model probabilities presented in *Figure 4*. 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 comparisons, it additionally prescribes the use of cluster fixed effects: here, the specification presented in equation (I) turns out to be the preferred one. (We repeat the model selection procedure using the Bayesian Information Criterion instead of the Akaike Information Criterion, following Clyde, 2003, and Hoeting et al., 1999, and arrive at the same results.)

Figure 4: Specification Selection



Note:

- The first two columns define the use of baseline data. Symbol ■ indicates that the choice applies. "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 3. The selection process is repeated for each test.
- The preferred specification is defined as the one with the highest conditional probability, and is highlighted through symbol ●.

In order to select operationalizations, we assess the sensitivity of results to different assumptions before settling on what we consider to be the most appropriate ones. We attempt to ground this in a transparent process. In *Figures A14* and *A18* of the *Appendix of Specification Curves*, we can see that 99% winsorization leaves questionable data points in place, meanwhile, we cannot see a case for winsorizing below the 95% level.

We proceed to the definition of valuation rules. Beyond the estimates that were already recoded through winsorization, we cannot visually determine which rule is most appropriate. To select that which is most representative of all specifications, we generate mean standardized effects for each test, subtract these from

all individual estimates to generate error terms, and select the rule that minimizes squared errors. This prescribes that we value all commodity type goods in each wave using the local median prices of the region.

The most difficult choice involves the selection of appropriate controls (i.e., the counterfactuals in comparisons [b] and [c], which aim to establish the impact of microenterprise and cash programs). Based on available literature, we expected to establish no evidence of spillovers, which would have enabled the use within-cluster comparison groups and avoid the clustering of standard errors. In the aggregate, as displayed in *Table 7*, there is no significant evidence of spillovers, though point estimates are consistently negative. Consulting the more detailed *Table A1* in the *Appendix of Tables*, we can see that in some sub-composites and waves, negative spillovers are borderline significant. We can also visualize spillovers in the *Appendix of Specification Curves*; the more pronounced they are, the less evenly distributed estimates will be among the specification alternatives. More specifically, where within-village spillovers are large and negative, within-village control groups (specification "wtn") will yield the highest results. Indeed, in the case of cash transfer programs, we see indications of negative spillovers in the cash groups on all dimensions – consumption (*Figure A3*), assets (*Figure A9*), as well as productive cash inflows (*Figure A15*). Within-village counterfactuals would therefore be biased, and a shared counterfactual is needed. Relying only on between-village counterfactuals would damage power excessively, and the share of questionable controls is small. Following Banerjee et al. (2015), we opt to use all available controls.

We are now left with a single preferred specification rule that is again summarized in *Table 6*. We arrive at 216 estimates for poverty outcomes (36 per test); these are consolidated in *Table 7* and presented in more detail in *Tables A1-A6* of the *Appendix of Tables*.

Table 6: Summary of Analyses

		Test					
	Comparison set	[a] Impact of Spillovers	[b][iii] Impact of Microenterprise Programs	[c][iii] Impact of Transfer Programs	[d] Impact of Savings Component	[e] Impact of Behavioral Component	[f] Microenterprise over Transfer Programs
Sub-arm							
A1	Control	Treatment	Counterfactual	C			
A2	Microenterprise		T		T		T
B1	Control	T	C	C			
B2	Microenterprise minus Savings		T		C		T
C1	Control	T	C	C			
D1	Control	T	C	C			
D2	Transfers			T		C	C
D3	Transfers plus Behavioral Intervention			T		T	C
E1	Control	C	C	C			
Preferred Specification	Clustered standard errors (cls)	■	■	■	■	□	■
	Difference-in-Differences (did)	□	□	□	□	□	□
	ANCOVA (anc)	■	■	■	■	■	■
	Cluster fixed effects (fe)	□	□	□	□	■	□
	Socioeconomic covariates (cvt)	■	■	■	■	■	■
	95% winsorization (w95)	■	■	■	■	■	■
	99% winsorization (w99)	□	□	□	□	□	□
	Self-reported unit valuation (own)	□	□	□	□	□	□
	Medial local unit valuation[(1)] (loc)	■	■	■	■	■	■
	Within-village comparison (wtn)	□	□	□	□	■	□
	Between-village comparison (btw)	■	□	□	■	□	■
	Lee Bounds (differential attrition trim)	□	□	■	□	□	■

Notes:

- The columns at the bottom of the table define specification features; symbol ■ indicates that the choice applies. Where two columns are displayed, three alternatives are available; the third column is not displayed; a third choice applies whenever the other two do not apply.
- Column "cls" shows if the regressions adjust errors for cluster robustness. As it is defined by the counterfactual selection, it is not an independent choice dimension and included for illustration purposes only.
- For a discussion of columns "did", "anc", "fe", and "ctv", consult the footnote of *Figure 4*.
- The next two columns define operationalization 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 dimension (90% winsorization) exists. In no case did this turn out to be the preferred choice operationalization.
- 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. In no case did this turn out to be the preferred operationalization. (1) Note that some classes of goods (such as medical expenditures or jewelry assets) are too heterogeneous to allow for a sensible unit valuation across households; for such categories, only the respondent's own valuation is used. When aggregated with other measures that use another valuation rule, the latter valuation rule is displayed. See publicly archived code for further details.
- The final two columns define the choice dimension pertaining to the counterfactual selection. "wtn" implies a comparison [i] within villages, and "btw" implies a [ii] between-village comparison. A third choice, involving [iii] the use of all available counterfactuals, applies when neither of the other choices does. This is the case in comparison sets [b] and [c].

Table 7: Summary of Impacts on Poverty Outcomes (UGX values per capita)

			Conservatively Trimmed Estimate			Untrimmed Estimate			*Aggressively Trimmed Estimate*		
Comparison set			*Consumption*	*Assets*	*Cash Inflows*	Consumption	Assets	Cash Inflows	*Consumption*	*Assets*	*Cash Inflows*
[a]	Spillovers	Coefficient				-16,462	-3,640	-8,069			
		Error				18,915	6,789	9,273			
		p value				0.386	0.593	0.386			
		q value				1.000	1.000	1.000			
[b][iii]	Microenterprise Program	Coefficient				26,061	16,343	13,483			
		Error				11,248	5,449	6,747			
		p value				0.022 **	0.003 ***	0.048 **			
		q value				0.055 *	0.021 **	0.087 *			
[c][iii]	Transfer Program	Coefficient	*-48,001*	*287*	*-35,716*	-17,141	15,852	-8,453	*-6,417*	*18,420*	*-992*
		Error	*17,043*	*7,044*	*8,649*	19,679	8,397	11,740	*20,379*	*8,516*	*11,704*
		p value	*0.006* ***	*0.968*	*0.000* ***	0.385	0.061 *	0.473	*0.753*	*0.032* **	*0.933*
		q value	*0.011* **	*0.368*	*0.001* ***	0.580	0.132	0.599	*0.802*	*0.083* *	*0.878*
[d]	Savings Component (contingent on Microenterprise Variant)	Coefficient				8,833	-5,917	20,208			
		Error				21,944	9,048	11,007			
		p value				0.689	0.516	0.071 *			
		q value				1.000	1.000	0.506			
[e]	Behavioral Component (contingent on Transfer Variant)	Coefficient				-24,982	19,283	-5,154			
		Error				29,279	11,479	17,309			
		p value				0.394	0.094 *	0.766			
		q value				1.000	0.451	1.000			
[f]	Microenterprise vs Transfer Program	Coefficient	*33,190*	*-4,528*	*4,526*	46,294	-831	12,983	*79,796*	*16,065*	*39,236*
		Error	*23,372*	*9,798*	*11,352*	22,429	9,627	11,309	*19,282*	*7,126*	*9,032*
		p value	*0.159*	*0.645*	*0.691*	0.042 **	0.931	0.254	*0.000* ***	*0.026* **	*0.000* ***
		q value	*0.476*	*1.000*	*1.000*	0.193	1.000	1.000	*0.001* ***	*0.013* **	*0.001* ***

Notes:

- The table is based on data from the pooled follow-ups. For full results (including by survey round), consult the *Appendix of Tables*. False discovery rate adjustments that form the basis of q values are calculated on the sets of results from these tables.
- Trimming procedures for comparison sets that are afflicted by differential attrition will follow the procedures outlined in Lee (2009). We define an aggressive trim as that which results in a higher estimate, which may either involve trimming observation from the bottom of the treatment group or from the top of the comparison group.
- Comparison groups are formed to estimate the impact [a] of spillovers, [b] of the microenterprise program (both with and without the savings group component), [c] of the cash transfer program (both with and without the behavioral component), [d] of the savings group component conditional on the microenterprise program variant, [e] of the behavioral component conditional on the cash transfer program variant, and [f] of the microenterprise program over the cash transfer program. Counterfactual type [i] implies the use of within-village controls, [ii] the use of between-village controls, and [iii] the use of all available controls. For more information, see the chapter on *Empirical Strategy*.
- Standard errors are adjusted for cluster robustness in so-called clustered comparisons, i,e: [a], [b][iii], [c][iii], [d], and [f].

Impacts of Microenterprise Program

Table 7 shows impact on annual consumption amounting to UGX 26k per capita when pooling across survey rounds. This appears to be driven predominantly by gains in food and beverage consumption, which is corroborated by nutritional impacts: *Table A14 (see Appendix of Tables)* demonstrates strong evidence of improvements in food security (i.e., a reduction in the household food insecurity access score) as well as increases in dietary diversity. No meaningful impacts emerge on other health related outcomes (*Table A38*).

Table 7 also shows clear evidence of gains in asset stock, estimated at UGX 16k per capita. To put this in the context of the original transfer: given an average household size of six individuals and ignoring possible measurement gaps, the initial gain in per capita asset positions as a consequence of the transfer had been UGX 20k per capita among microenterprise participants. The gains in asset stock appears to be driven predominantly by growth in livestock ownership. *Table A32* breaks the household's financial position into its constituent components to explore if the modesty of these effects can be explained by the netting of savings and loans. Indeed, there are indications that both increase, but in no event do the individual estimates exceed one dollar (current USD) per capita.

Income effects appear to be driven by cash inflows from self-employment activities; no significant income effects emerge from paid employment. *Table A20* indicates that paid labor tends to fall, consistent with the conjecture that poverty reduction disincentives the pursuit of low-quality employment opportunities (Bandiera et al., 2017). No significant effects emerge on the number of income sources, suggesting that the program neither causes significant diversification nor specialization. We do not observe meaningful impacts on schooling outcomes (*Table A26*).

Table A8 lays out psychological outcomes. We see strong evidence of gains in subjective well-being, which unlike most other effects in this study tend to grow over time. We further see gains in self-reported status,

as well as the psychological composite index. *Table A44* indicates some improvements in trust and the degree of integration people perceive with their communities.

Impacts of Cash Transfer Programs (relative to Controls and Microenterprise)

As shown in *Table 7*, estimated asset effects of the cash transfer program are positive, in the vicinity of those estimated for the microenterprise intervention. Note however that the initial asset transfer was substantially higher in this program, at roughly UGX 35k per capita on average; we can infer that asset positions diminished at higher rates in the cash transfer group. This indicates that transfer recipients either consumed their newly received resources at higher rates or experienced higher rates of asset depreciation. Contrary to expectations, consumption estimates are markedly negative among cash transfer program beneficiaries, as shown in Tables *A3* and *A6* (see *Appendix of Tables*). Unlike in the microenterprise program, no encouraging signals emerge on psychological and nutritional outcomes. Consistent with Banerjee, Hanna, Kreindler, & Olken (2015), the results do not appear to be driven by a disincentive among cash transfer recipients to work: in fact, we see pronounced increases in self-reported labor force participation (*Table A21*). It appears that households used cash transfers in part to pay back loans, though in absolute terms the amounts are negligible (*Table A33*). Some positive tendencies emerge in the domain of school attendance and enrolment (*Table A27*).

Overall, results are substantially less encouraging than those of the microenterprise program: for reasons we cannot fully explain, transfer recipients appeared to derive less economic value from their assets than microenterprise beneficiaries did. The data are inconsistent with the conjecture that cash transfer participants could have become "lazy". They are more consistent with the beliefs of the program implementer: that left to themselves – without training and mentorship – beneficiaries struggled to make productive investments, maintain them, and derive sustained value from them. This statement must be caveated. The point estimates of the cash arm are puzzling and could warrant some suspicion. Pure transfer recipients could have strategically adjusted their self-reported economic status downward: having received less of a coherent narrative about the program and its justifications and objectives, they might have been

more likely to independently form a false belief that the surveyors of the evaluation team were involved with targeting beneficiaries. (This is purely speculative: there are no indications other than the results, and these could also be reconciled with other patterns – say, strong positive short-term dissaving choices and consumption effects that had dissipated as early as the first follow-up survey.) Also recall that *Table 5* indicated that study participants in cash transfer groups attrited at lower rates than respondents in the control and microenterprise groups; *Table 7* puts bounds on the effects in the light of this differential attrition, and no discoveries about cash transfers are robust to this.

Impacts of Savings Group Component (Conditional on Microenterprise Program Variant)

Reviewing *Table 7* and all relevant ones from the *Appendix of Tables*, we see parallels emerging with previous work of Karlan et al. (2017). We do not detect impacts on consumption nor total net asset positions. Surprisingly, *Table A34* suggests that even monetary asset positions are entirely unaffected: the expectation that savings groups would alter measures of financial inclusion was not borne out.[3] We do however see indications that savings groups can alter the structure of income sources, and appear to be especially conducive to non-farm microenterprise activity. We also see some indications of improvement in the standing of women in Table *A46*.

Impacts of Behavioral Intervention Component (Conditional on Cash Transfer Program Variant)

Table A5 suggests that short-term consumption effects are negative; meanwhile, the behavioral intervention seemed to alter the investment patterns of cash transfer recipients, leading to increased livestock investments. We again see some indications that income from paid employment falls, and *Table A29* seems to suggest that children may have started to work less; however, no effects on schooling outcomes are

[3] 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.

discernible. We see indications of gains in subjective well-being and diverse other psychological outcomes, with a strong signal on respondents' sense of pride (*Table A11*).

DISCUSSION

This study detects few meaningful positive impacts from plain cash transfers – partly because sample conditions and differential attrition led to broad confidence intervals, but also because point estimates on important markers of poverty are low. We gain elevated confidence in impacts of the integrated microenterprise intervention program. Here, key poverty outcomes are clearly significant, robust to multiple inference adjustments, and corroborated by consistent signals on subjective well-being and nutrition. Cost-effectiveness appears high: the cost of the microenterprise program, as incurred by Village Enterprise over the course of the roll-out, amounted to roughly UGX 101k per capita under very conservative assumptions (e.g., including fully loaded programmatic and overhead expenses incurred outside of Uganda). The scale of consumption effects, at roughly UGX 26k per year, implies a payback period below 4 years. Accounting additionally for the residual asset stock of UGX 16k, it comes closer to 3 years. In other words, break-even was plausibly achieved not far beyond the measurement period. Emerging insights on the impacts of marginal components (both with regards to savings group formation and psychological engagement) might advance cost-effectiveness further; however, point estimates are also consistent with a possible attenuation in poverty effects over time, so we are not able to speak confidently to the sustainability of gains.

The mechanism through which the integrated poverty program worked remains difficult to pin down. We see that the psychological condition of beneficiaries improved but cannot make compelling statements about mediation (Green, Ha, & Bullock, 2010). A simple behavioral intervention was able to achieve a somewhat similar profile on psychological and asset effects, but the same consumption effect patterns did not follow. Overall, the results support the notion that extensions to cash transfers can help beneficiaries get more value out of their newly acquired assets. It also supports the more specific belief of the implementer that an integrated package, designed with multiple presumed constraints in mind, cannot

simply be stripped of its components without adverse consequences. How such a package might be effectively delivered at very large scale remains an important and open question.

REFERENCES

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

Baird, S., McIntosh, C., & Özler, B. (2011). Cash or condition? Evidence from a cash transfer experiment. *Quarterly Journal of Economics*, *126*(4), 1709–1753. https://doi.org/10.1093/qje/qjr032

Bandiera, O., Burgess, R., Das, N., Gulesci, S., Rasul, I., & Sulaiman, M. (2017). Labor Markets and Poverty in Village Economies. *Quarterly Journal of Economics*, 811–870. https://doi.org/10.1093/q je/q jx003

Banerjee, A., Duflo, E., Chattopadhyay, R., & Shapiro, J. (2016). The Long term Impacts of a "Graduation" Program: Evidence from West Bengal. *Working Paper*, (September).

Banerjee, A., Duflo, E., Goldberg, N., Karlan, D., Osei, R., Pariente, W., … Udry, C. (2015). A multifaceted program causes lasting progress for the very poor: Evidence from six countries. *Science*, *348*(6236). https://doi.org/10.1126/science.1260799

Banerjee, A., Hanna, R., Kreindler, G., & Olken, B. A. (2015). *Debunking the Stereotype of the Lazy Welfare Recipient: Evidence from Cash Transfer Programs Worldwide* (Faculty Research Paper Series No. RWP15-076).

Benhassine, N., Devoto, F., Duflo, E., Dupas, P., & Pouliquen, V. (2015). Turning a shove into a nudge? A "labeled cash transfer" for education. *American Economic Journal: Economic Policy*, *7*(3), 1–48. https://doi.org/10.1257/pol.20130225

Benjamini, Y., & Hochberg, Y. (1995). Controlling the false discovery rate: a practical and powerful approach to multiple testing. *Journal of the Royal Statistical Society B*. https://doi.org/10.2307/2346101

Bernard, T., Dercon, S., Orkin, K., & Taffesse, A. S. (2014). The Future in Mind: Aspirations and Forward-Looking Behaviour in Rural Ethiopia. *The World Bank Working Paper Series*, (April), 48. https://doi.org/http://dx.doi.org/10.2139/ssrn.2514590

Blattman, C., Fiala, N., & Martinez, S. (2014). Generating Skilled Self-Employment in Developing Countries: Experimental Evidence from Uganda. *The Quarterly Journal of Economics*, *129*(2), 697–752. https://doi.org/10.1093/qje/qjt057

Blattman, C., Green, E. P., Jamison, J., Christian, M., Annan, J., Carlson, N., … Peck, R. (2016). The Returns to Microenterprise Support among the Ultrapoor: A Field Experiment in Postwar Uganda. *American Economic Journal: Applied Economics*, *8*(2), 35–64. https://doi.org/http://dx.doi.org/10.1257/app.20150023

Blattman, C., & Niehaus, P. (2014). Show them the money. *Foreign Affairs*.

Bold, T., Kimenyi, M., Mwabu, G., Ng'ang'a, A., & Sandefur, J. (2013). Scaling-up What Works: Experimental Evidence on External Validity in Kenyan. *Centre for Study of African Economies Conference*, *44*, 1–47.

Burnham, K. P., & Anderson, D. R. (2004). Multimodel inference: Understanding AIC and BIC in model selection. *Sociological Methods and Research*, *33*(2), 261–304. https://doi.org/10.1177/0049124104268644

Burnham, K. P., & Anderson, D. R. (2010). *Model Selection and Multi-Model Inference: A Practical Information-Theoretical Approach*. New York: Springer. https://doi.org/10.1016/j.ecolmodel.2003.11.004

Cartwright, N., & Hardie, J. (2012). *Evidence-Based Policy: A Practical Guide to Doing It Better*. Oxford University Press. Retrieved from https://books.google.com/books?id=SwU8LgEACAAJ

Casey, K., Glennerster, R., & Miguel, E. (2012). Reshaping Institutions: Evidence on Aid Impacts Using a Preanalysis Plan. *The Quarterly Journal of Economics*, *127*(4), 1755–1812. https://doi.org/10.1093/qje/qje027

Clyde, M. (2003). Model Averaging. In S. J. Press & S. Chib (Eds.), *Subjective and Objective Bayesian Statistics*. Hoboken, NJ, USA: John Wiley & Sons, Inc. https://doi.org/10.1002/9780470317105

Das, J., Do, Q.-T., & Ozler, B. (2005). Reassessing Conditional Cash Transfer Programs. *The World Bank Research Observer*, *20*(1), 57–80. https://doi.org/10.1093/wbro/lki005

Deaton, A. (1997). *The analysis of household surveys: A microeconometric approach to development policy*. Washington DC: World Bank Publishing Group. https://doi.org/doi:10.1596/0-8018-5254-4

Deaton, A. (2010). Instruments, Randomization, and Learning about Development. *Journal of Economic Literature*, *48*(2), 424–455. https://doi.org/10.1257/jel.48.2.424

Easterly, W. (2007). *The White Man's Burden: Why the West's Efforts to Aid the Rest Have Done So Much Ill and So Little Good*. Penguin Group.

Efron, B., Hastie, T., Johnstone, I., Tibshirani, R., Ishwaran, H., Knight, K., … Tibshirani, R. (2004). Least angle regression. *Annals of Statistics*, *32*(2), 407–499. https://doi.org/10.1214/009053604000000067

Fafchamps, M., McKenzie, D., Quinn, S., & Woodruff, C. (2012). Female Microenterprises and the Fly-paper Effect: Evidence from a Randomized Experiment in Ghana, 1–58. Retrieved from papers2://publication/uuid/39924231-0289-43F2-8C99-4339C761C628

Gash, M., & Odell, K. (2013). The Evidence-Based Story of Savings Groups: A Synthesis of Seven Randomized Control Trials. *The SEEP Network*, (September). Retrieved from http://trickleup.org/wp-content/uploads/2016/05/2013_SEEP_Evidence_Based_Savings_Groups.pdf

Gollwitzer, P. M. (1999). Implementation intentions. *American Psychologist*, *54*(7), 493–503. https://doi.org/10.1177/0146167207311201

Green, D. P., Ha, S. E., & Bullock, J. G. (2010). Enough Already about "Black Box" Experiments: Studying Mediation Is More Difficult than Most Scholars Suppose. *The Annals of the American Academy of Political and Social Science*, *628*(1), 200–208. https://doi.org/10.1177/0002716209351526

Hanna, R., Mullainathan, S., & Schwartzstein, J. (2014). Learning through noticing: Theory and evidence from a field experiment. *Quarterly Journal of Economics*, *129*(3), 1311–1353. https://doi.org/10.1093/qje/qju015

Haushofer, J., & Shapiro, J. (2016). The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya. *Quarterly Journal of Economics*. https://doi.org/10.1093/qje/qjw025

Hoeting, J. J. A., Madigan, D., Raftery, A. E. A., & Volinsky, C. T. (1999). Bayesian model averaging: A tutorial. *Statistical Science*, *14*(4), 382–401. https://doi.org/10.2307/2676803

Humphreys, M., Sanchez de la Sierra, R., & Windt, P. Van Der. (2012). *Social and Economic Impacts of Tuungane*.

Ioannidis, J. P. A., Stanley, T. D., & Doucouliagos, H. (2017). THE POWER OF BIAS IN ECONOMICS RESEARCH*. *The Economic Journal*, *127*(605), F236–F265. https://doi.org/10.1111/ecoj.12461

Jensen, R. (2010). The (Perceived) Returns to Education and the Demand for Schooling. *Quarterly Journal of Economics*, *125*(2), 515–548. https://doi.org/10.1162/qjec.2010.125.2.515

Karlan, D., Savonitto, B., Thuysbaert, B., & Udry, C. (2017). Impact of savings groups on the lives of the poor. *Proceedings of the National Academy of Sciences*, *114*(12), 3079–3084. https://doi.org/10.1073/pnas.1611520114

Lee, D. S. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *Review of Economic Studies*, *76*(3), 1071–1102. https://doi.org/10.1111/j.1467-937X.2009.00536.x

Lybbert, T. J., & Wydick, B. (2016). Hope as Aspirations, Agency, and Pathways: Poverty Dynamics and Microfinance in Oaxaca, Mexico. *NBER Working Paper Series*, 33. https://doi.org/10.1093/wbro/lkt007

McKenzie, D. (2012). Beyond baseline and follow-up: The case for more T in experiments. *Journal of Development Economics*, *99*(2), 210–221. https://doi.org/10.1016/j.jdeveco.2012.01.002

Meyer, B. D., & Sullivan, J. X. (2003). *Measuring the Well-Being of the Poor Using Income and Consumption* (NBER Working Paper Series No. 9760). Retrieved from http://www.nber.org/papers/w9760

Miguel, E., Camerer, C., Casey, K., Cohen, J., Esterling, K. M., Gerber, a, … Van der Laan, M. (2014). Social science. Promoting transparency in social science research. *Science (New York, N.Y.)*, *343*, 30–1. https://doi.org/10.1126/science.1245317

Nosek, B. A., Alter, G., Banks, G. C., Borsboom, D., Bowman, S. D., Breckler, S. J., … Yarkoni, T. (2015). Promoting an open research culture. *Science*, *348*, 1422–25. https://doi.org/10.1126/science.aab2374

O'Donoghue, T., & Rabin, M. (1999). Doing it now or later. *American Economic Review*, *89*(1), 103–124. https://doi.org/10.1257/aer.89.1.103

Oettingen, G. (2000). Expectancy Effects on Behavior Depend on Self-Regulatory Thought. *Social Cognition*, *18*(2), 101–129. https://doi.org/10.1521/soco.2000.18.2.101

Olken, B. A. (2015). Promises and Perils of Pre-Analysis Plans. *Journal of Economic Perspectives*, *29*(3), 61–80. https://doi.org/10.1257/jep.29.3.61

Pritchett, L., & Sandefur, J. (2013). Context Matters for Size: Why External Validity Claims and Development Practice Don't Mix. *Journal of Global Development*, *4*(2), 161–197. https://doi.org/10.2139/ssrn.2364580

Simonsohn, U., Simmons, J. P., & Nelson, L. D. (2015). Specification Curve: Descriptive and Inferential Statistics on All Reasonable Specifications. *Mimeo*. Retrieved from http://ssrn.com/abstract=2694998

Stiglitz, J. (1989). Markets, Market Failures, and Development. *The American Economic Review*, *79*(2), 197–203. Retrieved from http://www.jstor.org/stable/1827756

Sulaiman, M., Goldberg, N., Karlan, D., & de Montesquiou, A. (2016). Eliminating Extreme Poverty: Comparing the Cost-Effectiveness of Livelihood, Cash Transfer, and Graduation Approaches. *Access to Finance Forum. Reports by CGAP and Its Partners*, *11*(11).

ENDNOTES

[1] All mathematical procedures 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:

- 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).
- 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).
- 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).
- 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).
- 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).
- 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 of Specification Curves

January 24, 2018

List of Figures

Notes on the Interpretation of Specification Curves

The specification curves display 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 resoluts total composite outcomes pooled across follow up rounds are displayed. Each figure comes with three charts:

"Specification Alternatives" Chart:
This chart highlights alternative specification details.

- Columns define specification features. A filled symbol indicates that the column feature applies, while a blank symbol indicates that it does not. Where two columns are displayed, 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.
- Column **cls** shows if the regressions adjust errors for cluster robustness. As this choice applies to all so-called clustered comparison sets, and never applies to so-called non-clustered comparison sets, it is not an independent choice dimension (unlike all other columns), and is included for illustration purposes only.
- The next 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 socioeconomic baseline covariates from all those listed in the paper. The selection process is repeated for each test.
- The next 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 colums 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. Note that some classes of goods (such as medical expenditures or jewelry assets) are too heterogeneous to allow for a sensible unit valuation across households; for such categories, only the respondent's *own* valuation is used. When aggregated with other measures that use use another valuation rule, the latter valuation rule is displayed. See publicly archived code for further details.
- The final two columns define the choice dimension pertaining to the counterfactual selection. Note that alternatives are only applicable in comparison sets [a] and [b]. **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.

"Estimates" Chart:
These display estimated treatment effects, presented in standardized terms (i.e., in terms of standard deviations of the control group). All numbers are per capita, and flow numbers are annualized. The preferred specification, identified in the paper, is highlighted through a black (as opposed to a hollow) marker.

"p values" Chart:
Specifications and treatment effects are ordered in ascending order of p values. The preferred specification is again highlighted.

Figure A1: Impact of Spillovers on Consumption

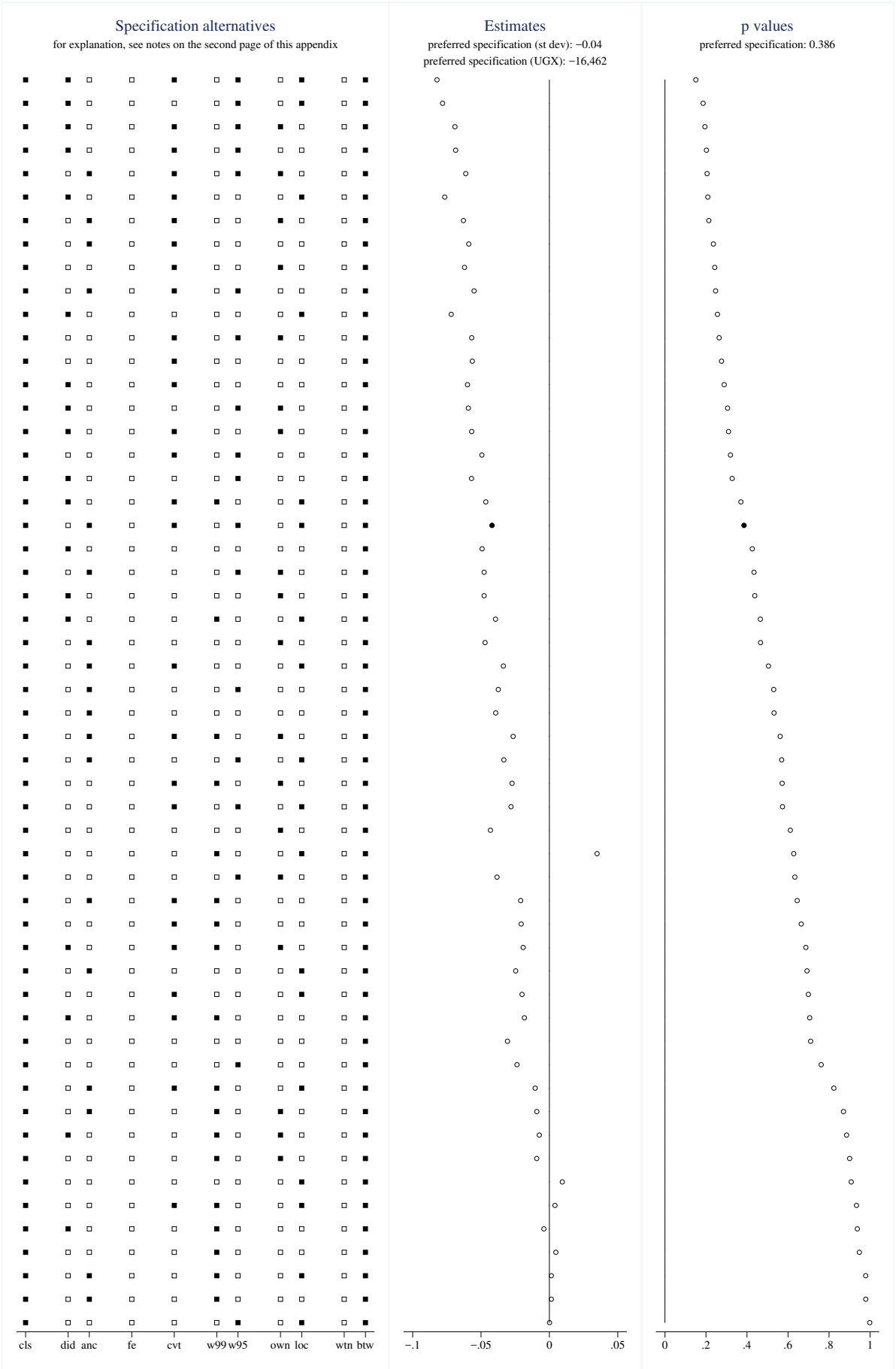


Figure A2: Impact of Microenterprise Programs on Consumption

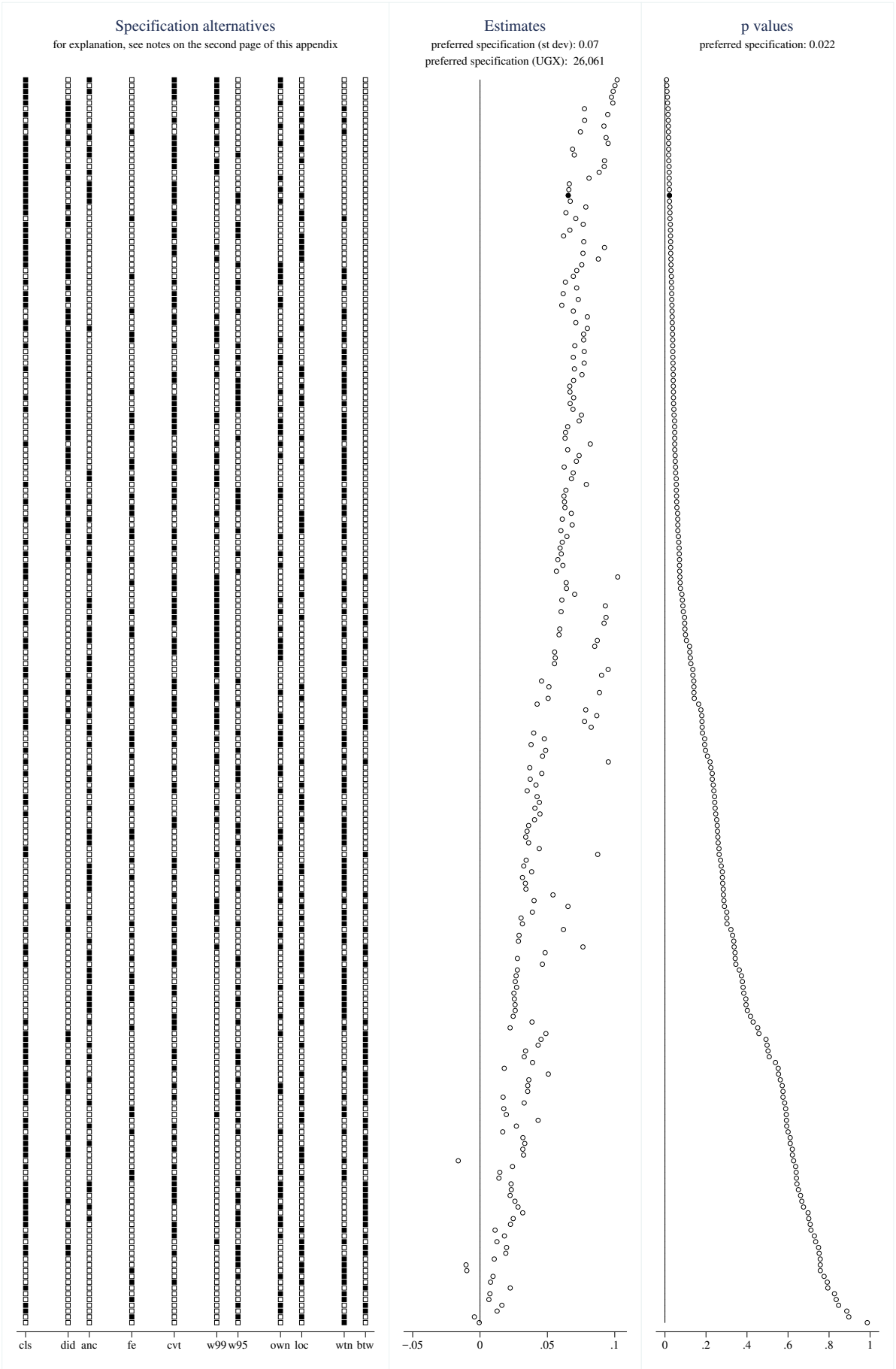


Figure A3: Impact of Cash Transfer Programs on Consumption

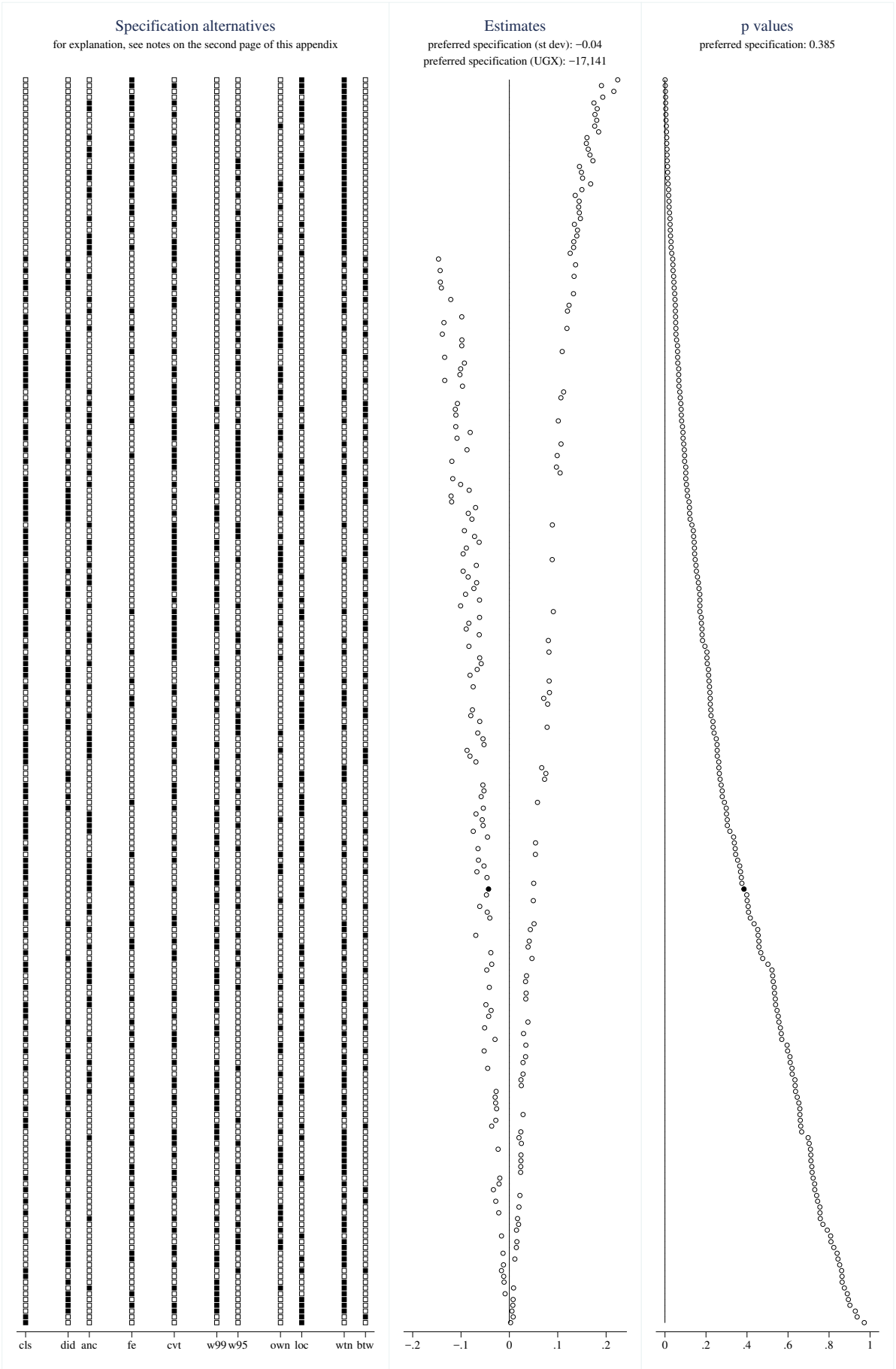


Figure A4: Impact of Savings Group Component (Conditional on Microenterprise Program Variant) on Consumption

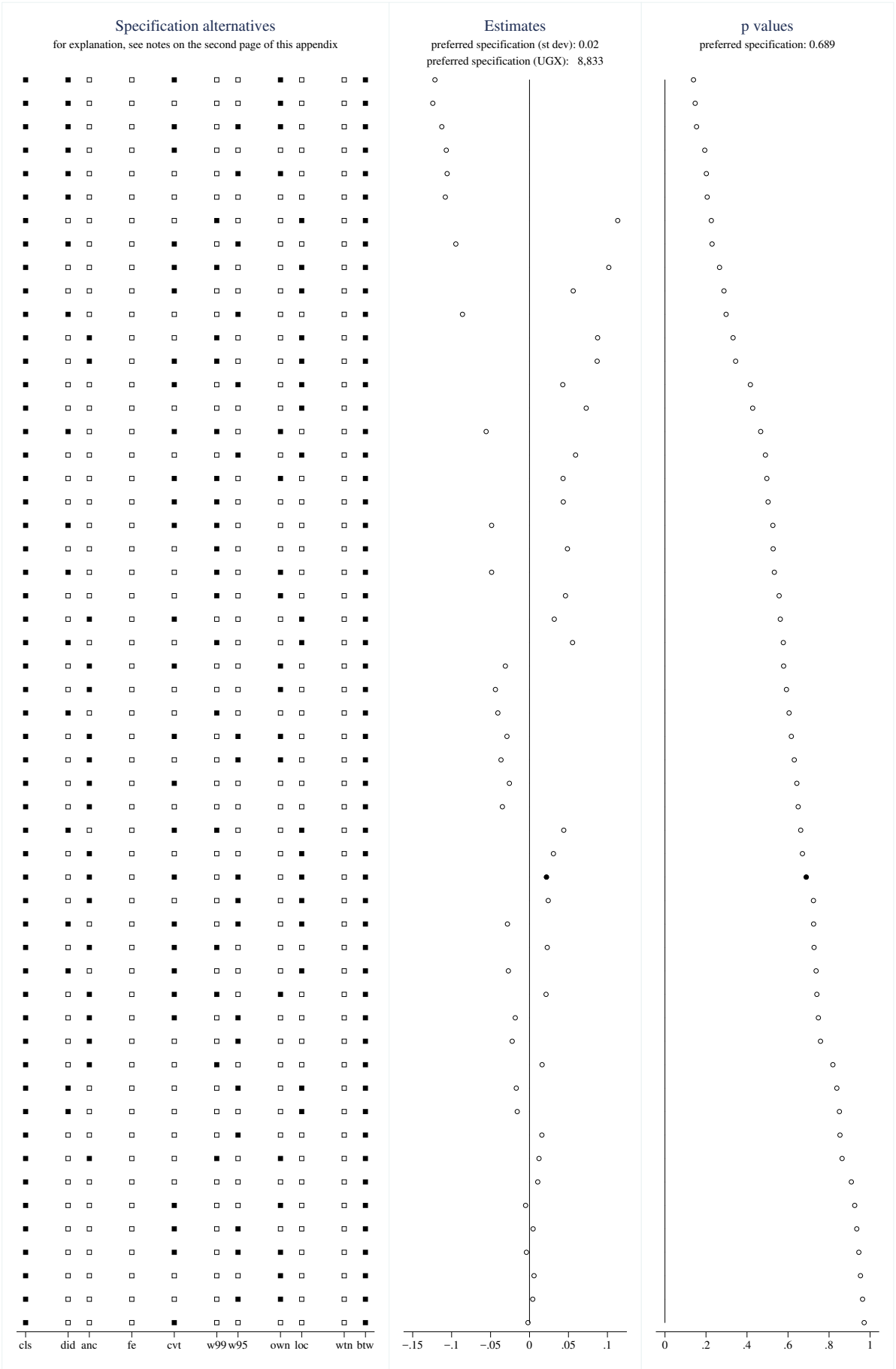


Figure A5: Impact of Behavioral Intervention Component (Conditional on Cash Transfer Program Variant) on Consumption

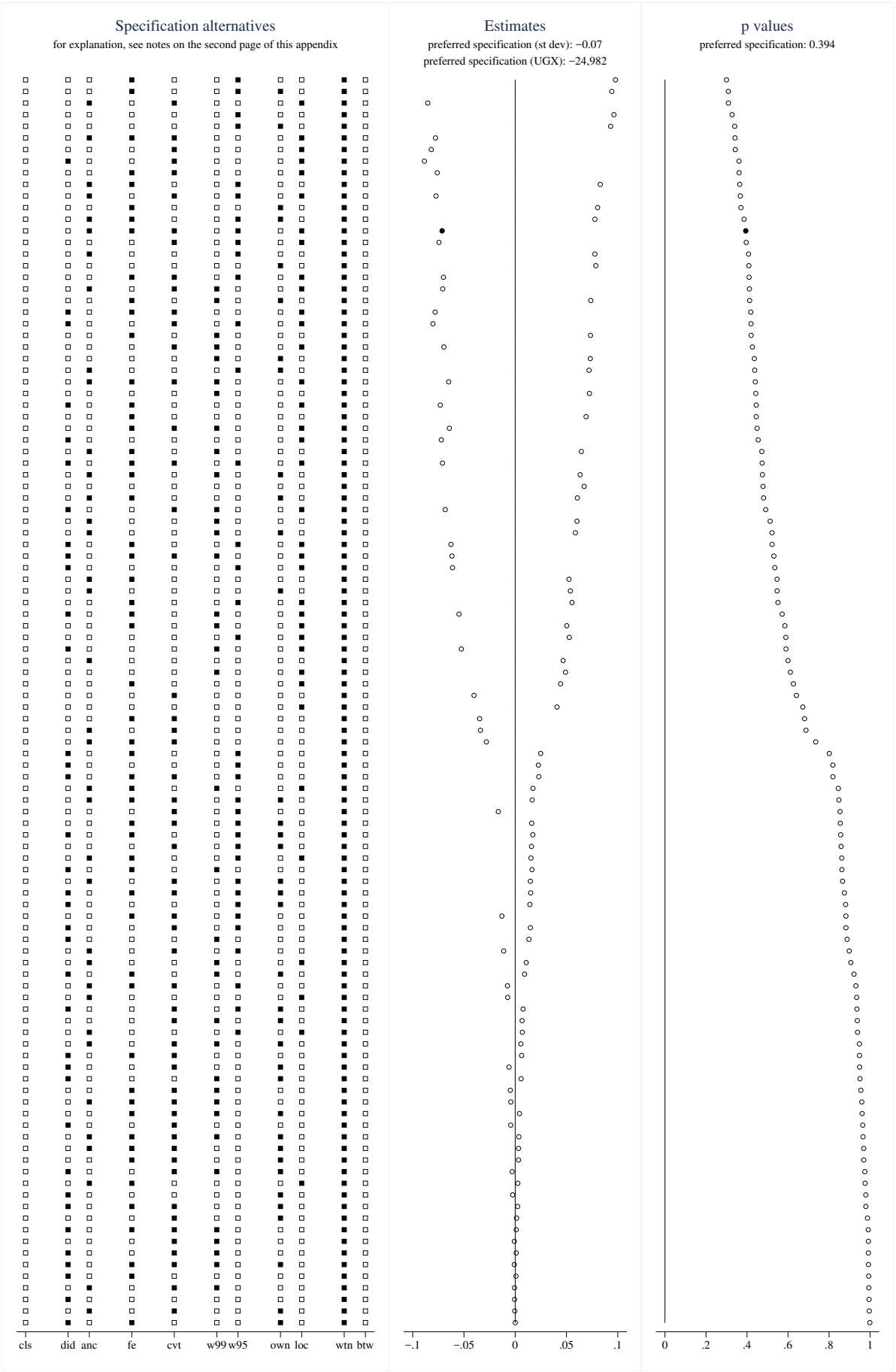


Figure A6: Impact of Microenterprise Programs vs Cash Transfer Programs on Consumption

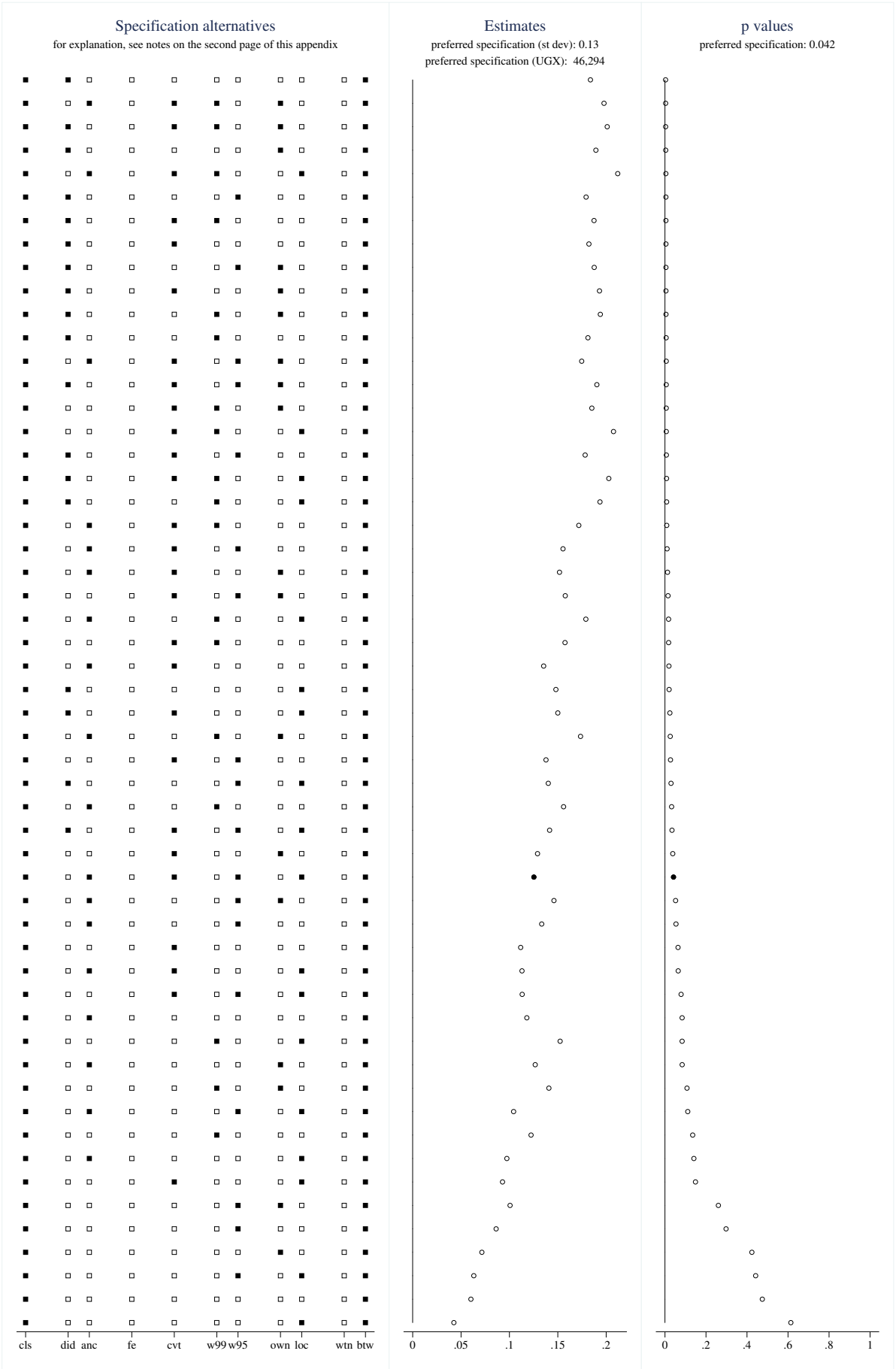


Figure A7: Impact of Spillovers on Assets

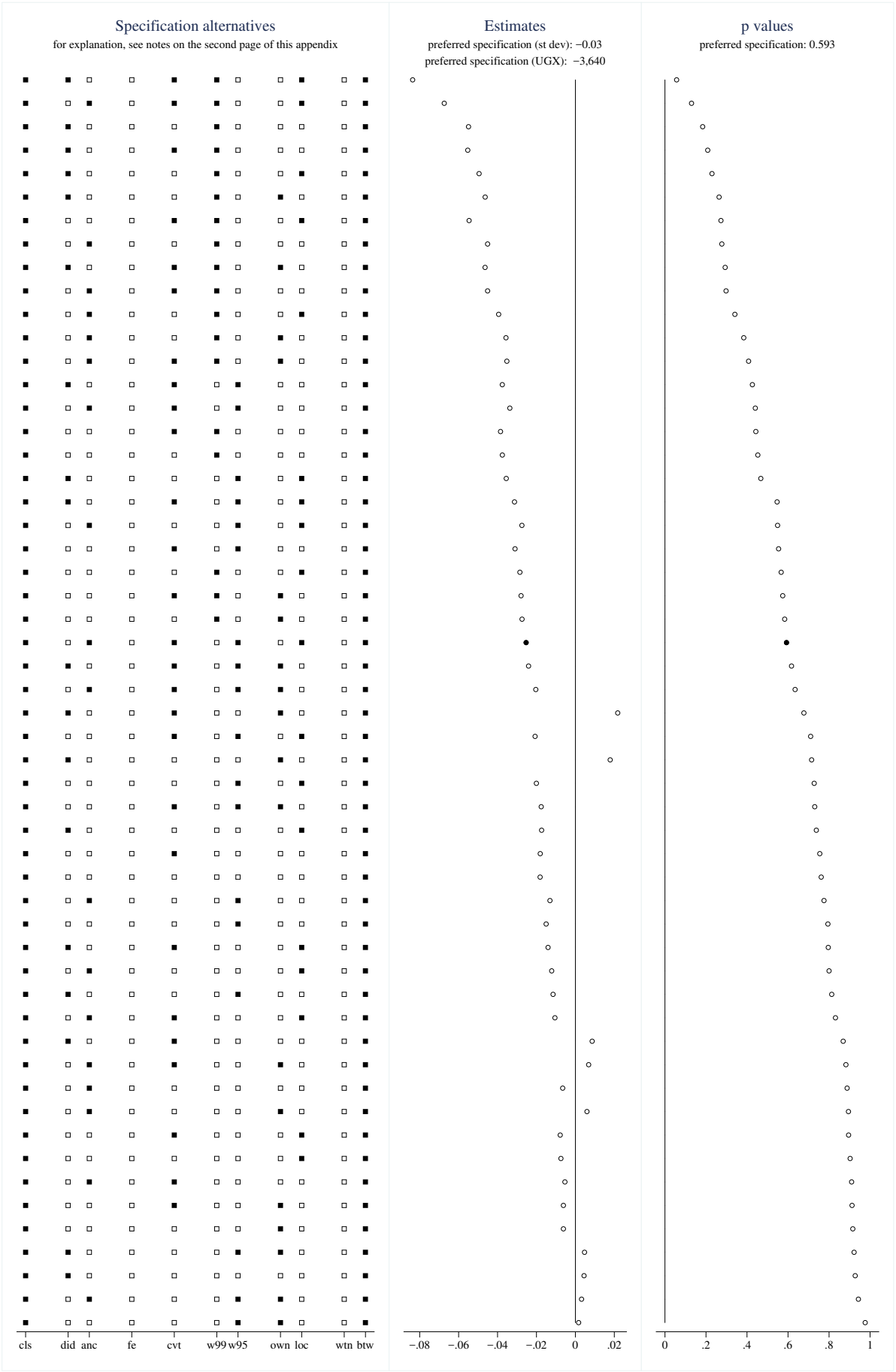


Figure A8: Impact of Microenterprise Programs on Assets

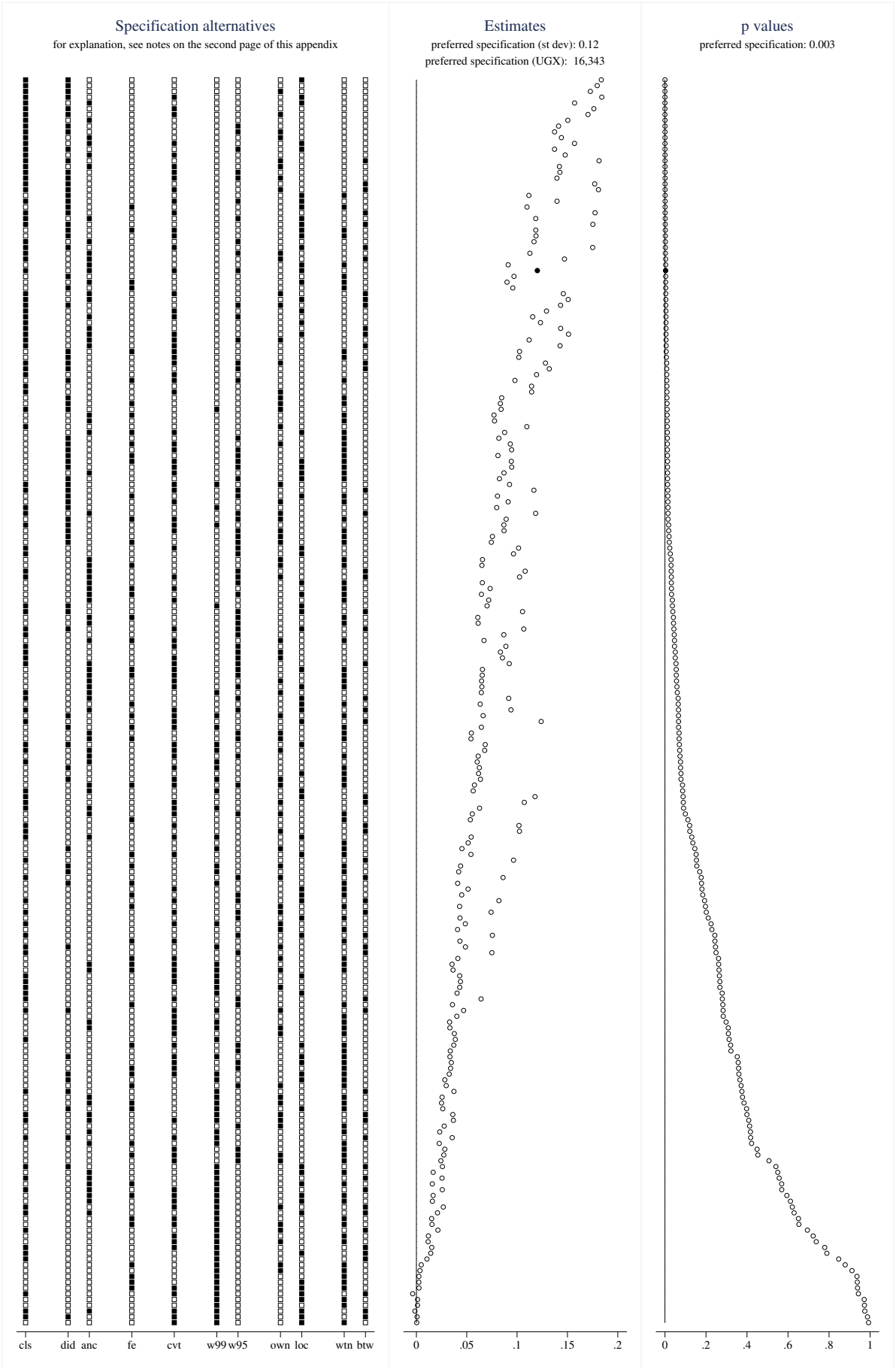


Figure A9: Impact of Cash Transfer Programs on Assets

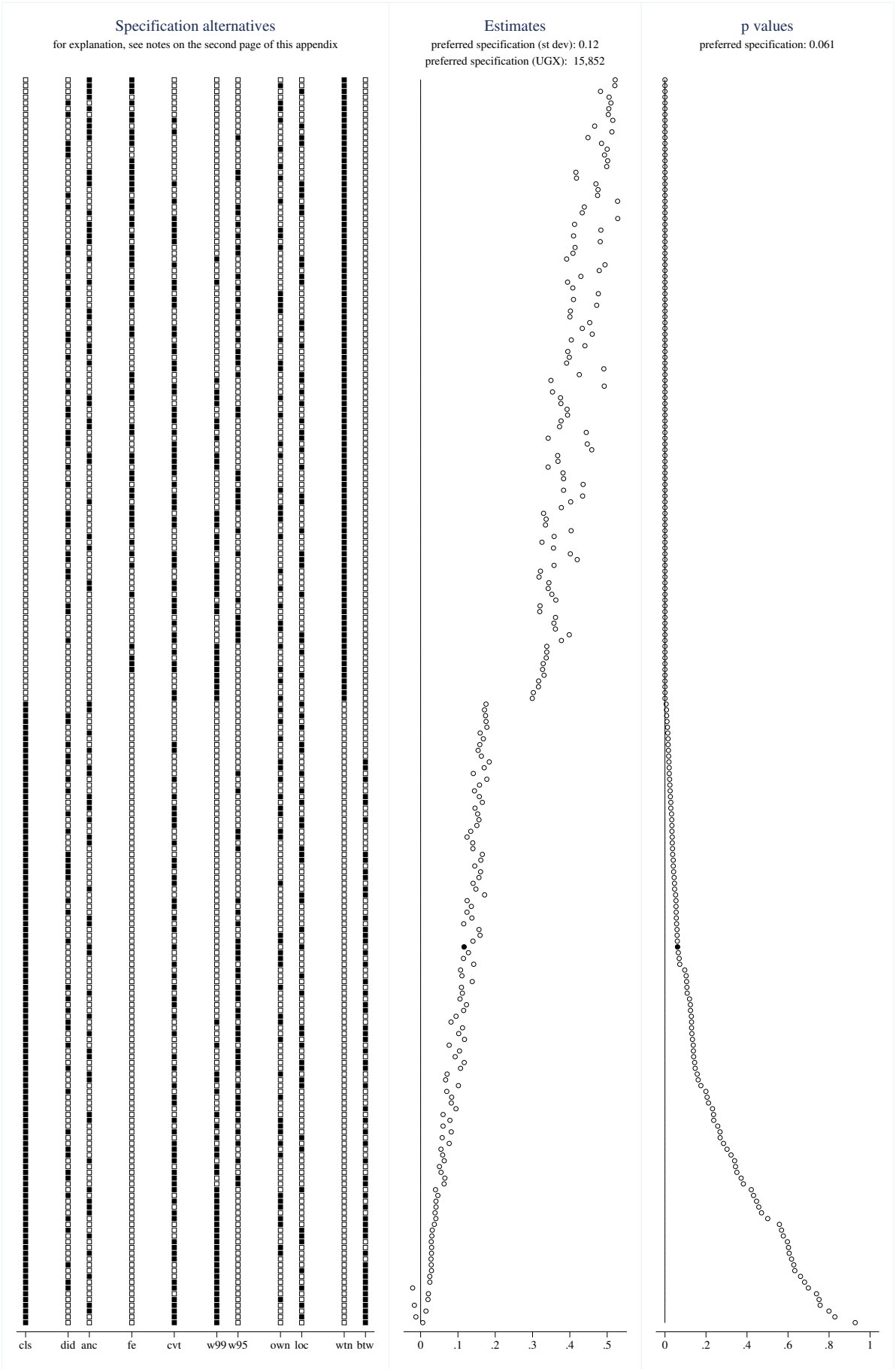


Figure A10: Impact of Savings Group Component (Conditional on Microenterprise Program Variant) on Assets

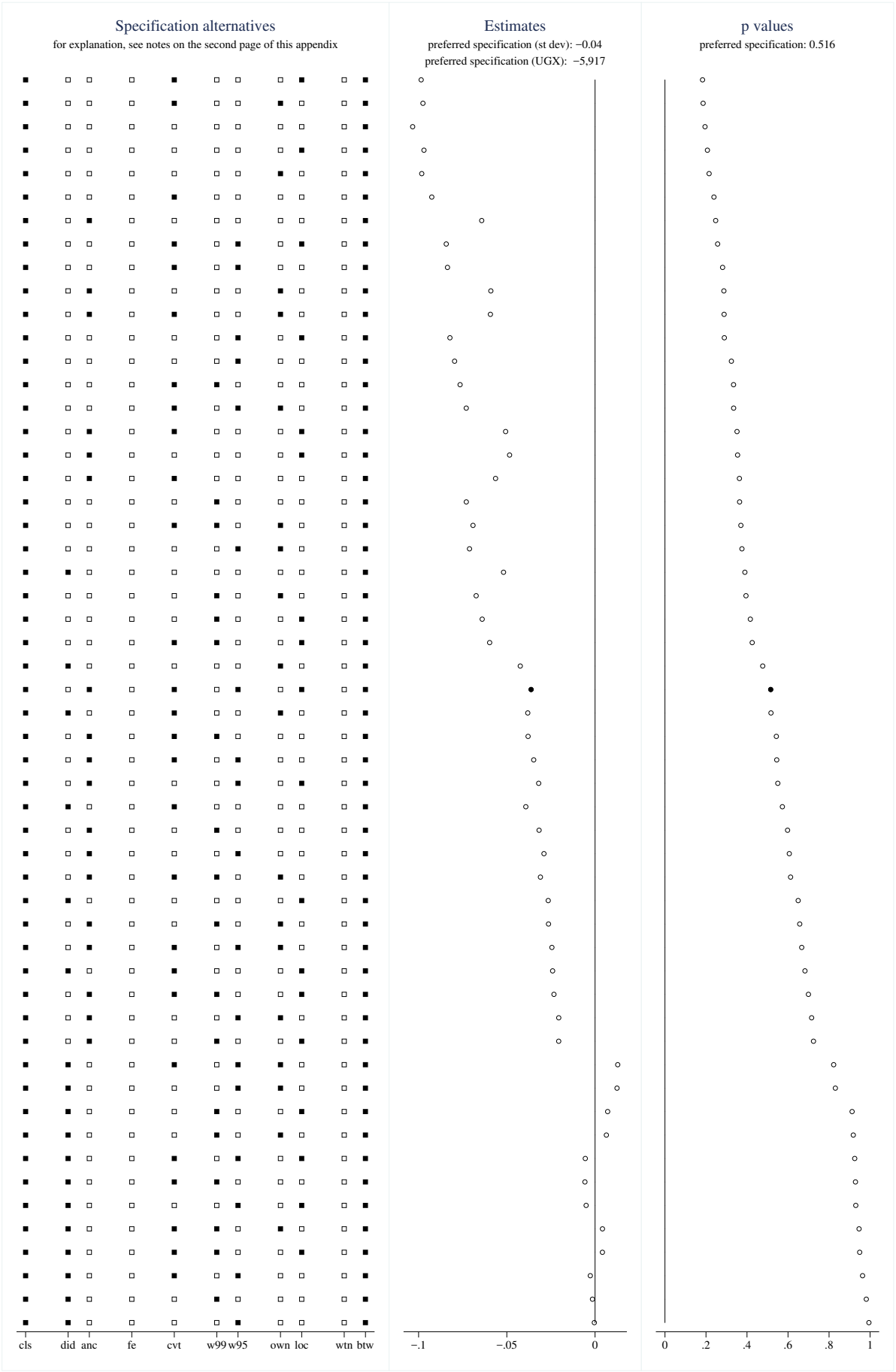


Figure A11: Impact of Behavioral Intervention Component (Conditional on Cash Transfer Program Variant) on Assets

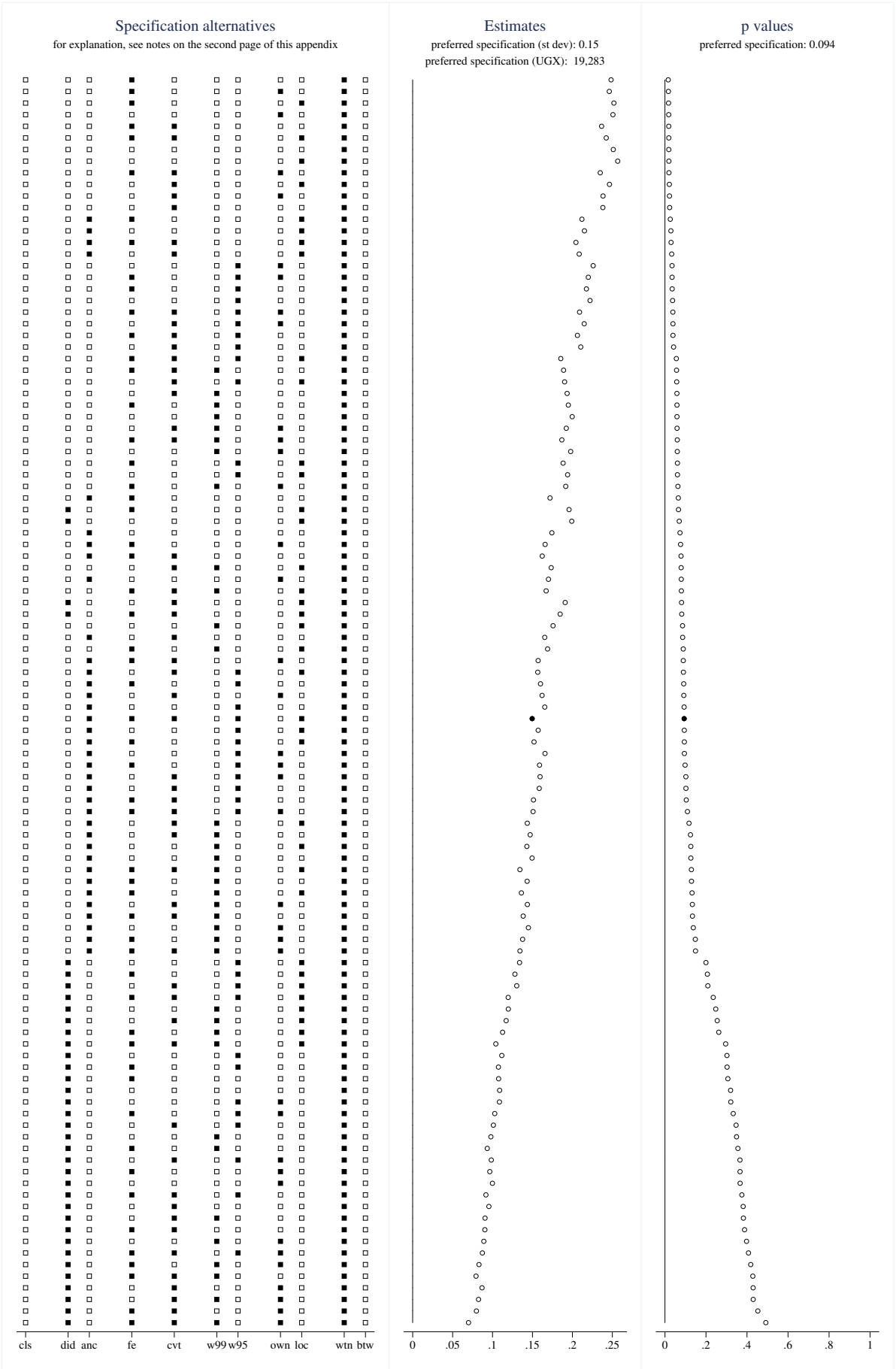


Figure A12: Impact of Microenterprise Programs vs Cash Transfer Programs on Assets

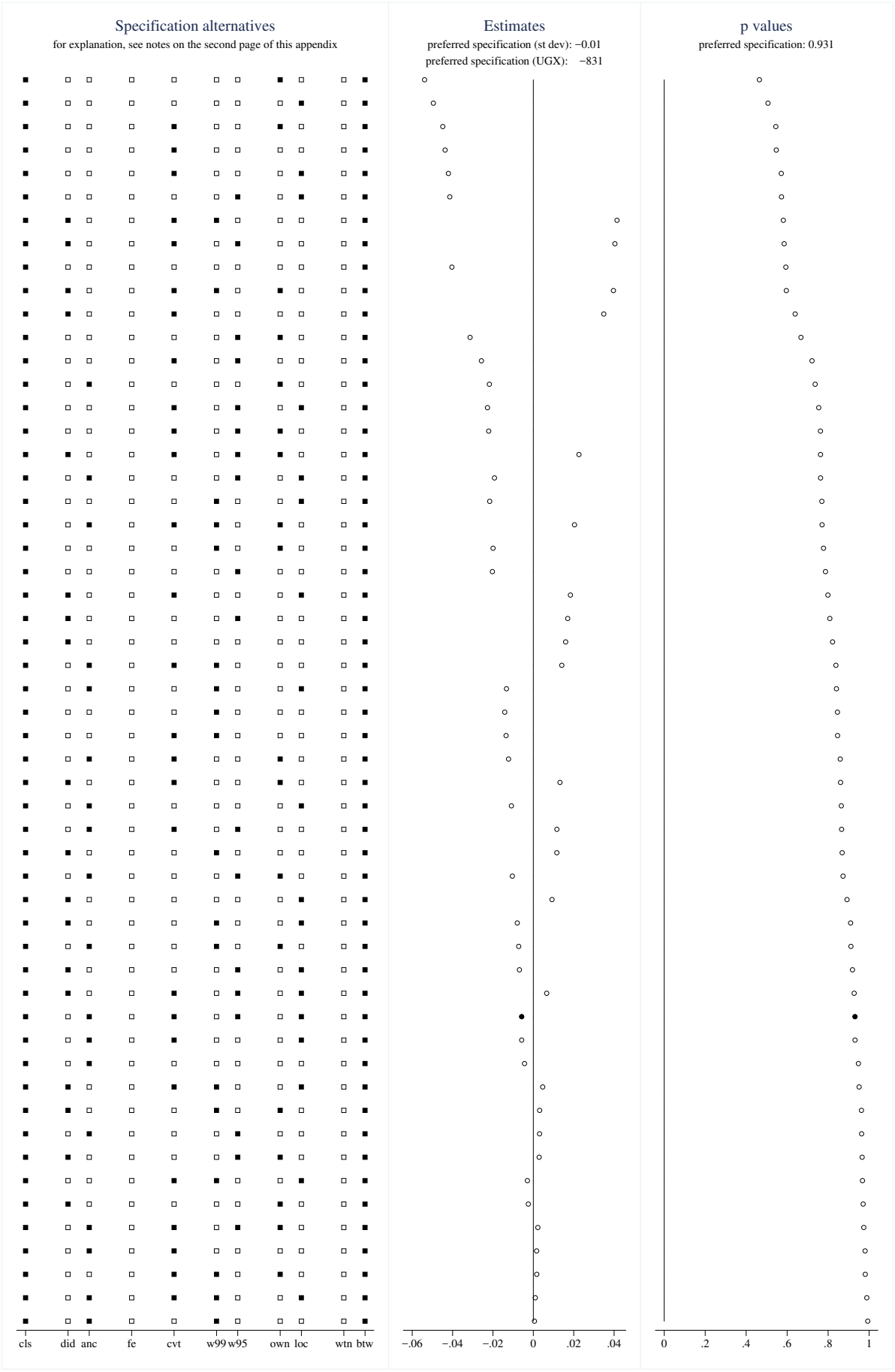


Figure A13: Impact of Spillovers on Productive Cash Inflows

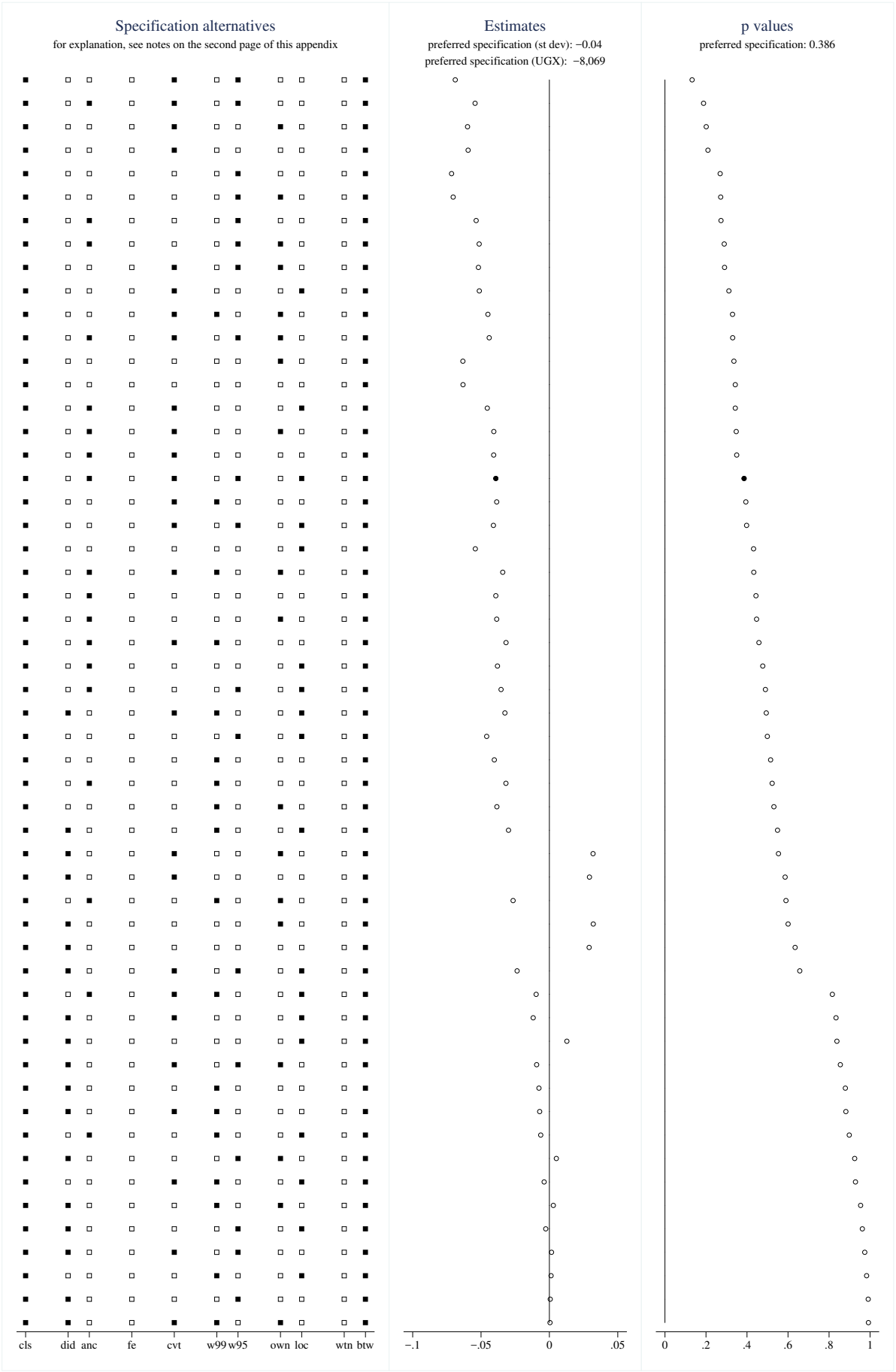


Figure A14: Impact of Microenterprise Programs on Productive Cash Inflows

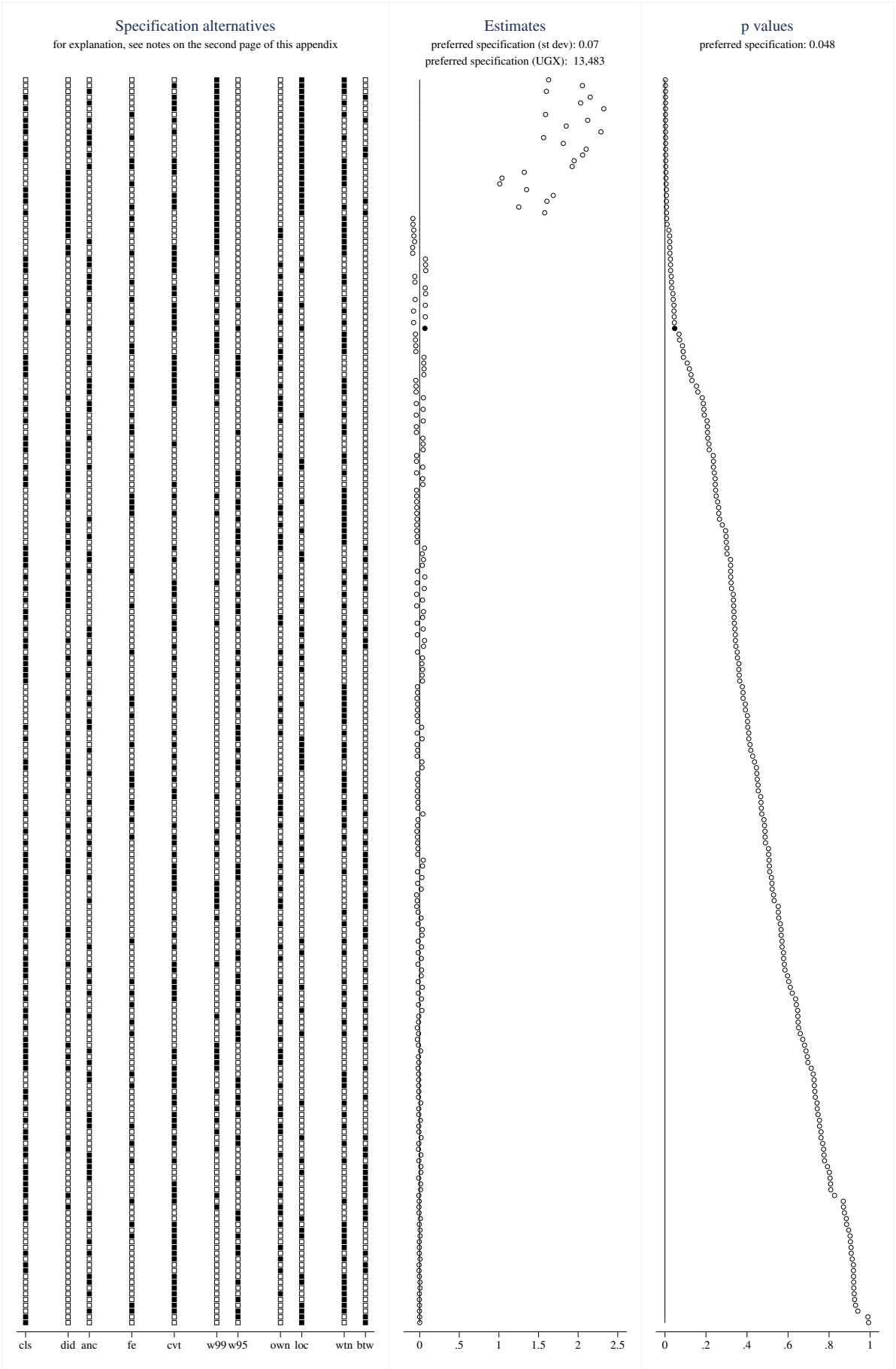


Figure A15: Impact of Cash Transfer Programs on Productive Cash Inflows

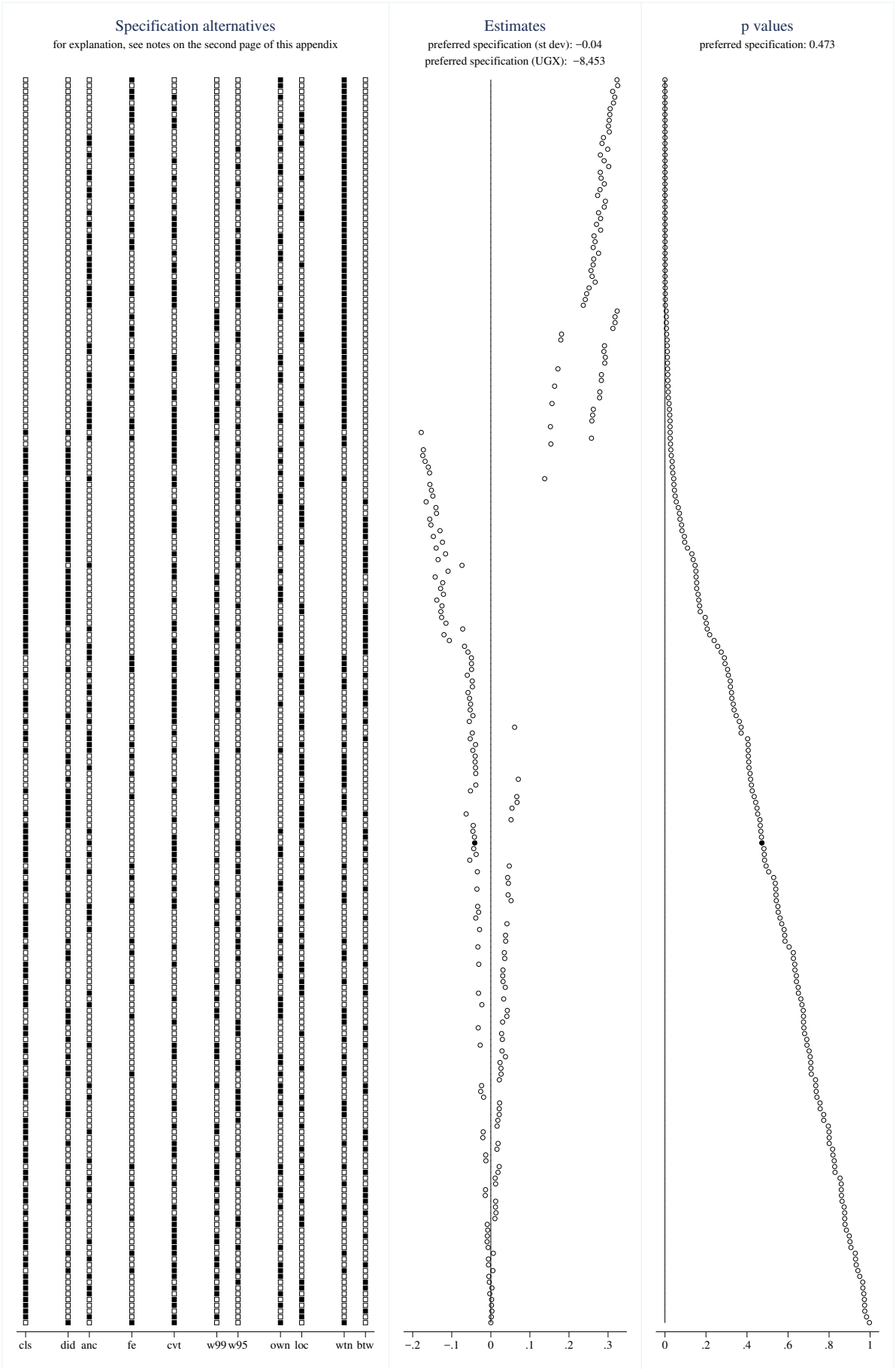


Figure A16: Impact of Savings Component (Conditional on Microenterprise Program Variant) on Productive Cash Inflows

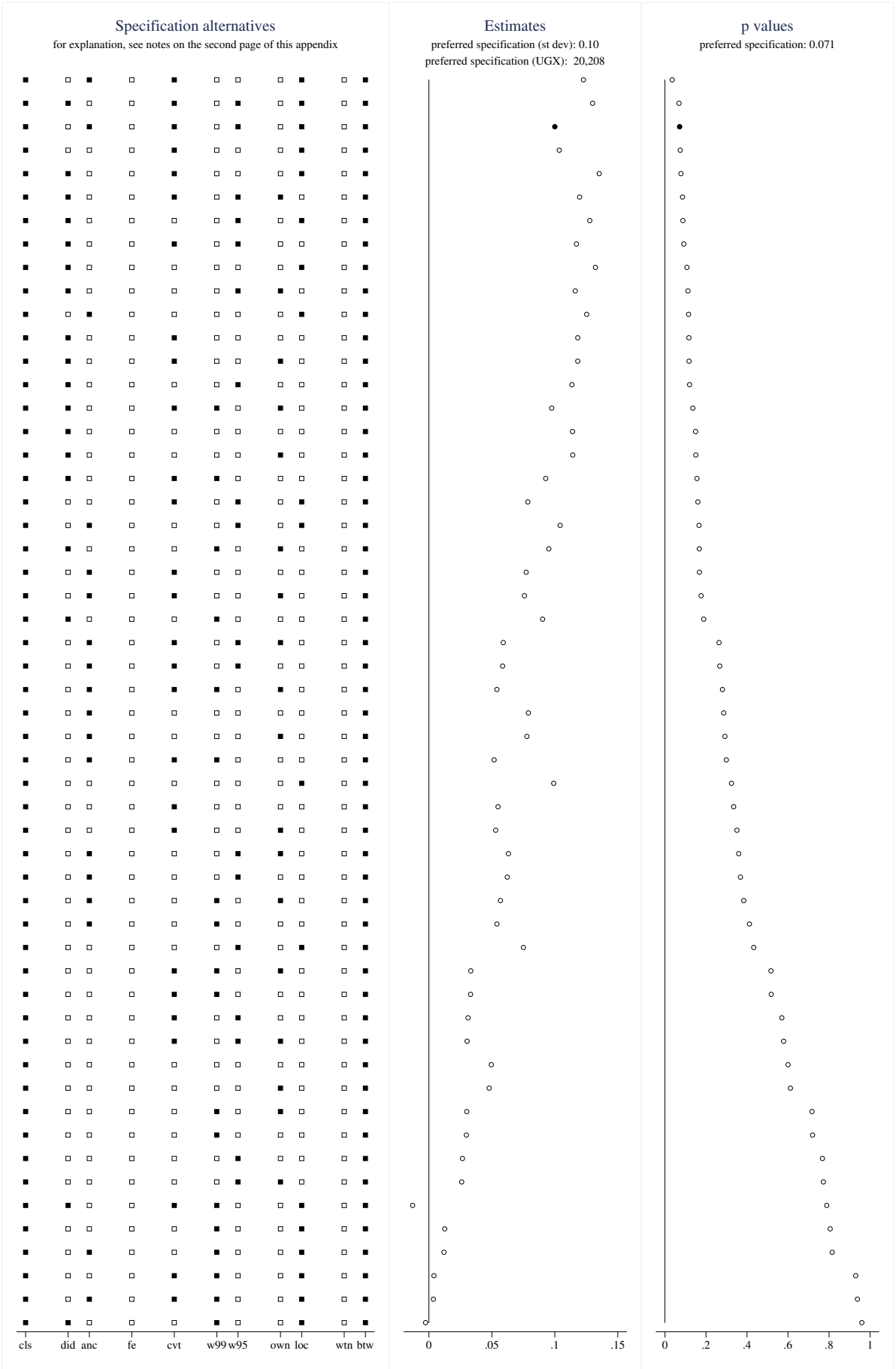


Figure A17: Impact of Behavioral Intervention (Conditional on Cash Transfer Program Variant) on Productive Cash Inflows

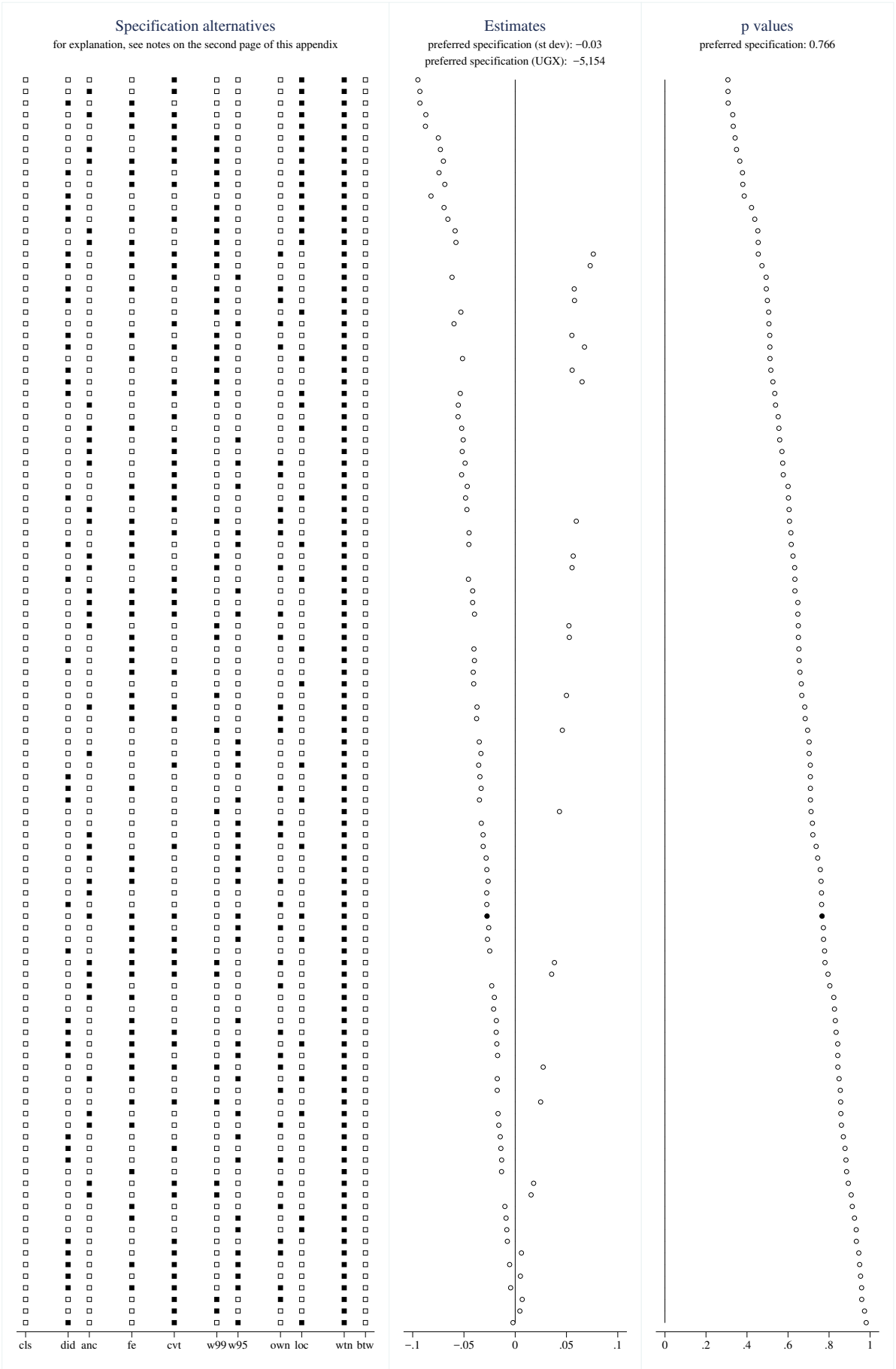
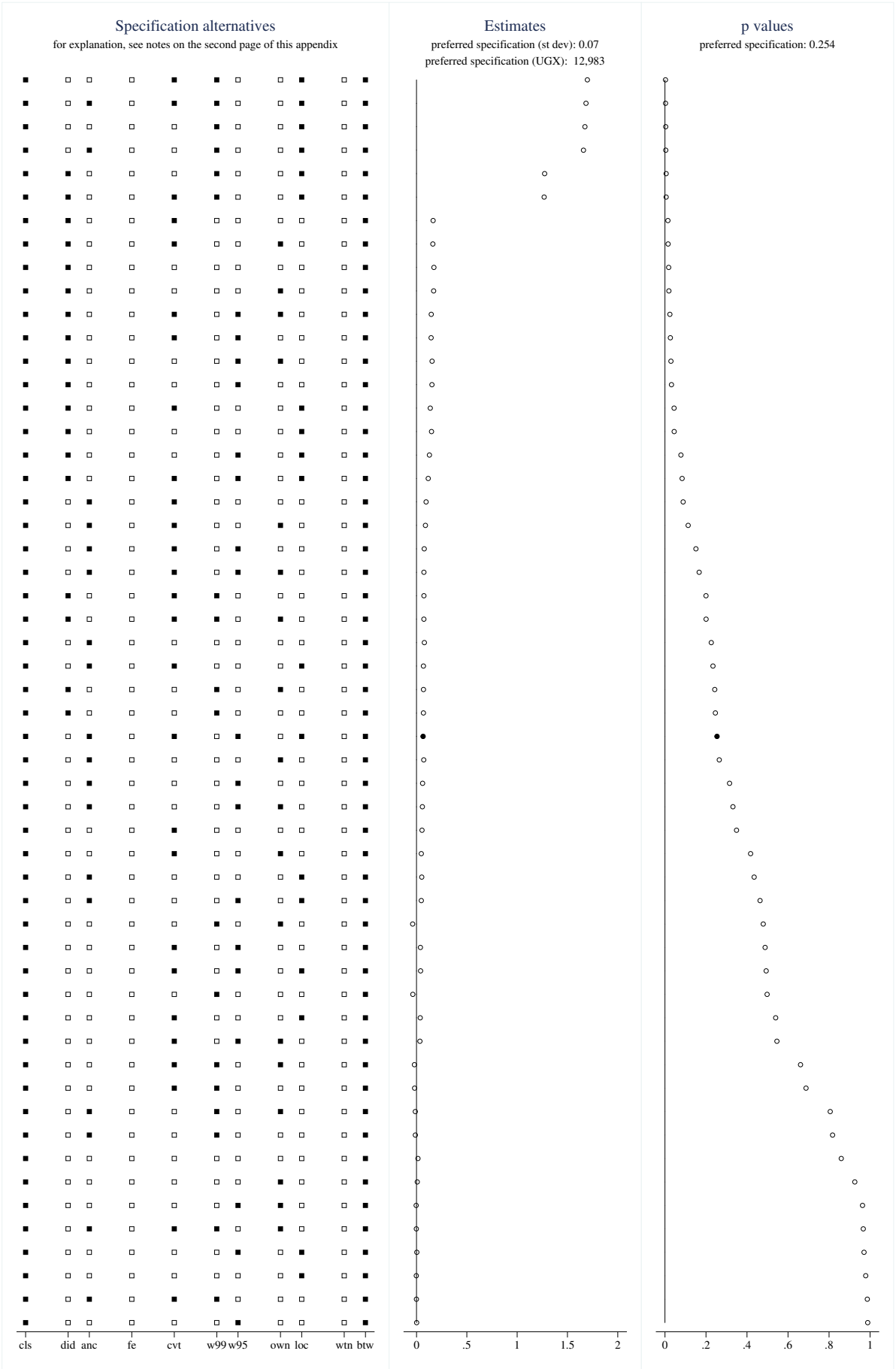


Figure A18: Impact of Microenterprise Programs vs Cash Transfer Programs on Productive Cash Inflows



Appendix of Tables

January 24, 2018

List of Tables

Table A1: Impact of Spillovers on Poverty Indicators

Follow-up Round	First	Second	Pooled	First	Second	Pooled	First	Second	Pooled
	Total Consumption			Total 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
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
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
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
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

Table A2: Impact of Microenterprise Programs on Poverty Indicators

Follow-up Round	First	Second	Pooled	First	Second	Pooled	First	Second	Pooled
	Total Consumption			Total 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
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
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
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
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

Table A3: Impact of Cash Transfer Programs on Poverty Indicators

Follow-up Round	First	Second	Pooled	First	Second	Pooled	First	Second	Pooled
	Total Consumption			Total 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
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
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
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
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

Table A4: Impact of Savings Group Component (Conditional on Microenterprise Program Variant) on Poverty Indicators

Follow-up Round	First	Second	Pooled	First	Second	Pooled	First	Second	Pooled
	Total Consumption			Total 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
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
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
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
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

Table A5: Impact of Behavioral Intervention Component (Conditional on Cash Transfer Program Variant) on Poverty Indicators

Follow-up Round	First	Second	Pooled	First	Second	Pooled	First	Second	Pooled
	Total Consumption			Total 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
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
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
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
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

Table A6: Impact of Microenterprise Programs vs Cash Transfer Programs on Poverty Indicators

Follow-up Round	First	Second	Pooled	First	Second	Pooled	First	Second	Pooled
	Total Consumption			Total 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
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
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
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
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

Table A7: Impact of Spillovers on Psychological Indicators

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:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{pjB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1 and to the value zero in set E1.
- Errors are adjusted for cluster robustness.

Table A8: Impact of Microenterprise Programs on Psychological Indicators

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{pjB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.

- Errors are adjusted for cluster robustness.

Table A9: Impact of Cash Transfer Programs on Psychological Indicators

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set D2$\bigcup$D3 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Errors are adjusted for cluster robustness.

Table A10: Impact of Savings Component (Conditional on Microenterprise Program Variant) on Psychological Indicators

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{pjB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.
- Errors are adjusted for cluster robustness.

Table A11: Impact of Behavioral Intervention (Conditional on Cash Transfer Program Variant) on Psychological Indicators

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called non-clustered comparisons, which is $y_{ijF} = \alpha_j + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, α_j defines cluster fixed effects; y_{ijB} is the is the baseline value of the dependent variable (included only when available); and X_{pjB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.

Table A12: Impact of Microenterprise Programs vs Cash Transfer Programs on Psychological Indicators

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set D2$\bigcup$D3.

- Errors are adjusted for cluster robustness.

Table A13: 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{pjB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1 and to the value zero in set E1.
- Errors are adjusted for cluster robustness.

Table A14: 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	0.107	0.001 ***	0.107	0.023 **	0.104	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	0.104	0.023 **	0.131	0.057 *	0.113	0.023 **
	N	4,916	4,906	4,811	3,815	5,073	4,021

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{pjB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Errors are adjusted for cluster robustness.

Table A15: Impact of Cash 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set D2$\bigcup$D3 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Errors are adjusted for cluster robustness.

Table A16: Impact of Savings Component (Conditional on Microenterprise Program Variant) 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{pjB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.
- Errors are adjusted for cluster robustness.

Table A17: Impact of Behavioral Intervention (Conditional on Cash Transfer Program Variant) 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	0.815	0.815	1.000	1.000	0.815	0.815
	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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called non-clustered comparisons, which is $y_{ijF} = \alpha_j + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, α_j defines cluster fixed effects; y_{ijB} is the is the baseline value of the dependent variable; and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.

Table A18: Impact of Microenterprise Programs vs Cash 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set D2$\bigcup$D3.
- Errors are adjusted for cluster robustness.

Table A19: Impact of Spillovers on Employment Activity

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:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1 and to the value zero in set E1.
- Logistic regression is applied in all cases. As all outcomes are binary, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A20: Impact of Microenterprise Programs on Employment Activity

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Logistic regression is applied in all cases. As all outcomes are binary, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A21: Impact of Cash Transfer Programs on Employment Activity

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set D2$\bigcup$D3 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Logistic regression is applied in all cases. As all outcomes are binary, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A22: Impact of Savings Component (Conditional on Microenterprise Program Variant) on Employment Activity

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.
- Logistic regression is applied in all cases. As all outcomes are binary, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A23: Impact of Behavioral Intervention (Conditional on Cash Transfer Program Variant) on Employment Activity

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called non-clustered comparisons, which is $y_{ijF} = \alpha_j + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, α_j defines cluster fixed effects; y_{ijB} is the is the baseline value of the dependent variable; and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2.
- Logistic regression is applied in all cases. As all outcomes are binary, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A24: Impact of Microenterprise Programs vs Cash Transfer Programs on Employment Activity

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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set D2$\bigcup$D3.
- Logistic regression is applied in all cases. As all outcomes are binary, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A25: 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:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1 and to the value zero in set E1.
- Logistic regression is applied in the case of the first two outcomes, which are binary. Because of these outcomes, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A26: 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the one zero among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Logistic regression is applied in the case of the first two outcomes, which are binary. Because of these outcomes, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A27: Impact of Cash 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set D2$\bigcup$D3 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Logistic regression is applied in the case of the first two outcomes, which are binary. Because of these outcomes, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A28: Impact of Savings Component (Conditional on Microenterprise Program Variant) 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.
- Logistic regression is applied in the case of the first two outcomes, which are binary. Because of these outcomes, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A29: Impact of Behavioral Intervention (Conditional on Cash Transfer Program Variant) 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called non-clustered comparisons, which is $y_{ijF} = \alpha_j + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, α_j defines cluster fixed effects; y_{ijB} is the is the baseline value of the dependent variable; and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2.
- Logistic regression is applied in the case of the first two outcomes, which are binary. Because of these outcomes, no pooled follow-up round is created.

Table A30: Impact of Microenterprise Programs vs Cash 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for individual i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set D2$\bigcup$D3.
- Logistic regression is applied in the case of the first two outcomes, which are binary. Because of these outcomes, no pooled follow-up round is created.
- Errors are adjusted for cluster robustness.

Table A31: 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:

- All numbers are reported in current Ugandan Shillings per capita. Flow variables (consumption and income) are yearly. Totals are not equal to the sum of sub-composites because they are winsorized and estimated separately. For further information on sensitivities, see corresponding specification curve figure.
- Estimates pertain to coefficient β in the preferred specification for so-called clustered comparisons. The applicable model is defined as $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F; y_{ijB} is the is the baseline value of the dependent variable; and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1 and to the value zero in set E1.
- Errors are adjusted for cluster robustness.

Table A32: 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

Notes:

- All numbers are reported in current Ugandan Shillings per capita. Flow variables (consumption and income) are yearly.
- Estimates pertain to coefficient β in the preferred specification for so-called clustered comparisons. The applicable model is defined as $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F; y_{ijB} is the is the baseline value of the dependent variable; and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Errors are adjusted for cluster robustness.

Table A33: Impact of Cash 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

Notes:

- All numbers are reported in current Ugandan Shillings per capita. Flow variables (consumption and income) are yearly.
- Estimates pertain to coefficient β in the preferred specification for so-called clustered comparisons. The applicable model is defined as $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F; y_{ijB} is the is the baseline value of the dependent variable; and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set D2$\bigcup$D3 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Errors are adjusted for cluster robustness.

Table A34: Impact of Savings Component (Conditional on Microenterprise Program Variant) 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

Notes:

- All numbers are reported in current Ugandan Shillings per capita. Flow variables (consumption and income) are yearly.
- Estimates pertain to coefficient β in the preferred specification for so-called clustered comparisons. The applicable model is defined as $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F; y_{ijB} is the is the baseline value of the dependent variable; and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.
- Errors are adjusted for cluster robustness.

Table A35: Impact of Behavioral Intervention (Conditional on Cash Transfer Program Variant) 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

Notes:

- All numbers are reported in current Ugandan Shillings per capita.
- Estimates pertain to coefficient β in the preferred specification for so-called non-clustered comparisons. The applicable model is $y_{ijF} = \alpha_j + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, α_j defines cluster fixed effects; y_{ijB} is the is the baseline value of the dependent variable; and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set D3 and to the value zero in set D2.

Table A36: Impact of Microenterprise Programs vs Cash 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

Notes:

- All numbers are reported in current Ugandan Shillings per capita. Flow variables (consumption and income) are yearly.
- Estimates pertain to coefficient β in the preferred specification for so-called clustered comparisons. The applicable model is defined as $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F; y_{ijB} is the is the baseline value of the dependent variable; and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set D2$\bigcup$D3.
- Errors are adjusted for cluster robustness.

Table A37: 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:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1 and to the value zero in set E1.
- Errors are adjusted for cluster robustness.

Table A38: 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.
- Errors are adjusted for cluster robustness.

Table A39: Impact of Cash 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set D2$\bigcup$D3 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.

- Errors are adjusted for cluster robustness.

Table A40: Impact of Savings Component (Conditional on Microenterprise Program Variant) 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.
- Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.
- Errors are adjusted for cluster robustness.

Table A41: Impact of Behavioral Intervention (Conditional on Cash Transfer Program Variant) 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called non-clustered comparisons, which is $y_{ijF} = \alpha_j + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, α_j defines cluster fixed effects; y_{ijB} is the is the baseline value of the dependent variable (included only when available); and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.

Table A42: Impact of Microenterprise Programs vs Cash 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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set D2$\bigcup$D3.

- Errors are adjusted for cluster robustness.

Table A43: Impact of Spillovers on Community 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
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

Table A44: Impact of Microenterprise Programs on Community 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
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

Table A45: Impact of Cash Transfer Programs on Community 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
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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set D2$\bigcup$D3 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.

- Errors are adjusted for cluster robustness.

Table A46: Impact of Savings Component (Conditional on Microenterprise Program Variant) on Community 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
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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set A2 and to the value zero in set B2.

- Errors are adjusted for cluster robustness.

Table A47: Impact of Behavioral Intervention (Conditional on Cash Transfer Program Variant) on Community 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
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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called non-clustered comparisons, which is $y_{ijF} = \alpha_j + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, α_j defines cluster fixed effects; y_{ijB} is the is the baseline value of the dependent variable (included only when available); and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set A1$\bigcup$B1$\bigcup$C1$\bigcup$D1$\bigcup$E1.

Table A48: Impact of Microenterprise Programs vs Cash Transfer Programs on Community 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
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

Notes:

- Estimates pertain to coefficient β. Specification 1 uses the model $y_{ijF} = \alpha + \beta T_{ij} + \varepsilon_{ij}$, where y_{ijB} is the outcome in question for household i in cluster j during survey round F. Specification 2 applies the preferred specification for so-called clustered comparisons, which uses model $y_{ijF} = \alpha + \beta T_{ij} + \gamma y_{ijB} + \delta X_{ijB} + \varepsilon_{ij}$; here, y_{ijB} is the is the baseline value of the dependent variable (included only when available) and X_{ijB} is a set of five baseline covariates selected for each given test using least angle regression.

- Intent-to-treat assignment T is coded to the value one among households in set A2$\bigcup$B2 and to the value zero in set D2$\bigcup$D3.

- Errors are adjusted for cluster robustness.