Aspiring to a Better Future: Can a Simple Psychological Intervention Reduce Poverty?*

Kate Orkin, Robert Garlick, Mahreen Mahmud,
Richard Sedlmayr, Johannes Haushofer, Stefan Dercon

December 12, 2024

Abstract

Do higher aspirations for the future motivate people living in poverty to make long-term investments? Do their aspirations increase when economic conditions improve? To answer these questions, we run a 415-village field experiment with 8,300 women living in poverty in rural Kenya. We design an 80-minute workshop to help people set higher aspirations and plan to achieve them. We cross-randomise this with large unconditional cash transfers. The workshop substantially raises aspirations, labour supply, investment, revenue, and living standards 17 months later, relative to a placebo workshop. Increases in aspirations are the most likely mechanism to explain the economic effects. Cash transfers also raise aspirations, which might help to explain why transfers increase labour supply and investment. We conclude that aspirations respond to both economic and psychological interventions, contribute to investment decisions and living standards, and are important considerations for development policy.

*Orkin: University of Oxford (kate.orkin@bsg.ox.ac.uk); Garlick: Duke University (robert.garlick@duke.edu); Mahmud: University of Exeter (m.mahmud@exeter.ac.uk); Sedlmayr: The Agency Fund (richard@agency.fund); Haushofer: Stockholm University (johannes.haushofer@su.se); Dercon: University of Oxford (stefan.dercon@economics.ox.ac.uk). The pre-analysis plans, questionnaires, and intervention materials are posted at https://www.socialscienceregistry.org/trials/996. We thank GiveDirectly, the Busara Centre for Behavioural Economics; Innovations for Poverty Action; and staff at the University of Oxford. Pat Olvera and Rita Wachera produced videos with the research team and Will Le, Stephanie Palla, Michael Cooke and Miriam Laker oversaw the GiveDirectly collaboration. We thank Charles Amuku, Paul Brimble, Rosie Chiang, Axel Eizmendi Larrinaga, Sam Fishman, Marta Grabowska, Michala Iben Riis-Vestergaard, Mungai Mwangi, Geetika Nagpal, Anasuya Narasimhan, Gabor Nyeki, Emma Riley, Giulio Schinaia, Victoria Schreitter, Catherine Thomas, and John Walker for outstanding research assistance. We thank Nava Ashraf, Jon de Quidt, Doug Gollin, Erica Field, Sarojini Hirshleifer, Supreet Kaur, Travis Lybbert, Karen Macours, Edward Miguel, Paul Niehaus, Gautam Rao, Imran Rasul, Emma Riley, Frank Schilbach, Jessi Streib, Duncan Thomas, Chris Udry, Michael Walker, Bruce Wydick and many seminar participants for thoughtful input as well as Natalie Quinn for collaboration on the consumption module. The project is funded by Fidelity Charitable, the UKRI GCRF Accelerating Achievement for Africa's Adolescents Hub, the Bill and Melinda Gates Foundation, Good Ventures, the JPAL PPE Initiative, and a donation from John Collenette. This study has ethics approval from Oxford (protocol # SSD/CUREC1A/BSG C1A_16-002), Maseno (protocol # MSU/DRPI/MUERC/00196/15) and Duke (protocol # 2018-0020) universities.

1 Introduction

Despite progress in poverty reduction globally, some people and communities struggle to accumulate assets and raise their earnings. While lack of access to capital, education and markets matter, recent evidence suggests psychological or "internal" constraints may inhibit people living in poverty from taking up work or investment opportunities, despite potentially high returns.[1] Researchers from multiple disciplines have theorised one possible internal constraint: people living in poverty may lack opportunities to learn "the capacity to aspire" (Appadurai, 2013): to set higher long-term aspirations to improve their socio-economic position and plan concrete steps to achieve them. They may not encounter institutions or traditions encouraging them to strive and plan for a better future or be less likely to encounter role models who have succeeded. They may set lower aspirations than they are able to attain, hence limiting their economic investments, and entrenching them in poverty.[2]

We test these ideas by providing causal evidence both that boosting aspirations among people living in poverty can increase investment and improve living standards, and that alleviating poverty can raise aspirations. We run a 415-village field experiment with 8,300 women living in poverty in rural Kenya. In some villages, we induce variation in aspirations through a unique workshop that teaches participants simple techniques to set higher aspirations and take actions to achieve them. In other villages, we induce variation in poverty by offering large unconditional cash transfers. Both the workshop and the cash transfers substantially raise aspirations, monetary and time investments, revenue, and proxies for living standards – consumption, non-land asset wealth and revenue – after 17 months. These results are consistent with theories proposing that people's aspirations are important contributors to their investment decisions and living standards and that aspirations respond to both economic and psychological interventions.

We establish these findings using a four-step argument. First, we show that *lower aspirations are associated with lower living standards and, conditional on living standards, lower investment in productive activities*. To show this, we census 32,000 households in 415 villages in rural Western Kenya, randomly sample 8,300 households classified as living in poverty on a proxy means test, and survey the primary adult woman in each household.[3] We measure respondents' aspirations as the levels of assets, income, and children's education they *would like their household to reach* a decade or more in the future. Aspirations are typically higher than, but highly correlated with, their expectations for their future and largely uncorrelated with the other psychological characteristics

[1] For example: Angelucci & Bennett (2021), Alan et al. (2019), Ashraf et al. (2022), Baranov et al. (2020), Barker et al. (2022), Bhat et al. (2022), Blattman et al. (2017), Campos et al. (2017), Ghosal et al. (2020), McKelway (2021).

[2] This idea appears in anthropology (Appadurai, 2013; Rapport, 2016), sociology (Willis, 1977; Wilson, 1987), and economic theory on the role of aspirations, neighbourhoods and role models in persistent poverty (Benabou, 1996; Dalton et al., 2016; Durlauf, 1996; Genicot & Ray, 2017; Lybbert & Wydick, 2018).

[3] Section 2 explains why we targeted women for interventions and therefore for our sample.

that we measure: the ability to plan for and implement actions to achieve desired outcomes (e.g. self-efficacy or locus of control), time and risk preferences, and mental health.[4] Aspirations are more strongly associated with living standards, as well as with monetary and time investment, conditional on living standards, than these other psychological characteristics. These associations are consistent with the theoretical ideas above but may not capture causal relationships.

Hence, second, *we develop a novel light-touch intervention to raise aspirations and provide causal evidence that this can substantially raise investment and living standards among households living in poverty.* Our intervention is a workshop lasting roughly 80 minutes, administered by fieldworkers in small groups. Participants watch a video about the life stories of two local fictional female role models who improve their economic position by applying three psychological techniques: setting higher aspirations; defining concrete, immediate steps to achieve them; and anticipating obstacles.[5] They then do facilitated exercises to learn to use these techniques. We study the workshop's impacts using the first two arms of the field experiment. The workshop (experimental arm 1) has large and statistically significant effects on five of six prespecified measures of investment and living standards after 17 months, relative to a placebo workshop containing the same information but without the life stories or psychological techniques (arm 2). It increases expenditure on productive inputs by 22% of the placebo group mean and labour supply by 5%. Households have higher living standards: consumption increases by 4%, non-land asset value by 6% and revenue by 11%. The workshop raises an index combining these six outcomes by 0.11 standard deviations. To achieve these changes, households save more and accumulate more small, non-lumpy assets. They invest mainly in non-farm enterprises, supplying more labour, spending more on inventory and enterprise assets, and adopting new business practices, consistent with research finding "slack" in this sector and region (Egger et al., 2022). Effects vary little by baseline wealth or other dimensions in prespecified and machine learning-based heterogeneity analysis, showing the workshop is effective even for poorer households. The workshop is highly cost-effective relative to many development interventions: it pays for itself in terms of raised consumption expenditure after 17 months.

Third, we show it is likely *the workshop operates at least in part through the mechanism of boosting women's aspirations and expectations for their household's future economic position.* It substantially increases prespecified measures of aspirations and expectations 17 months later, driven more by an increase in education aspirations than asset or income aspirations. It is unlikely it worked through other plausible psychological mechanisms studied in economics which we measure.

[4]We explain the choice of aspirations and expectations measure in Section 3.1 Our empirical results are robust to replacing aspirations with expectations or to measuring aspirations as the level respondents would like to reach minus their beliefs about their current level.

[5]The term "psychological techniques", originating from the medical and psychology literature, refers to the "active ingredients" of interventions, such as goal-setting and anticipating obstacles (Michie et al., 2013).

It has no effects on self-efficacy, the main measure psychologists use to capture the ability to plan for and implement actions to achieve desired outcomes (Conroy & Hagger, 2018; Kwasnicka et al., 2013); nor on locus of control, growth mindset, grit, time or risk preferences, or mental health. And it is unlikely it worked by providing specific information about possible investment activities because we compare the workshop to a placebo video and exercises containing exactly the same information – images and descriptions – about economic opportunities, but without the inspiring life stories and psychological techniques. The workshop does not affect beliefs about returns to specific economic activities nor lead to mimicry of activities in the videos. We also show evidence against effects being driven by experimenter demand. We show these results are consistent with a simple model of reference-dependent utility, where either aspirations (Dalton et al., 2016; Genicot & Ray, 2017; Lybbert & Wydick, 2018) or expectations (Kőszegi & Rabin, 2006, 2007) proxy for reference points. Exceeding (missing) one's reference point for future consumption increases (decreases) utility. A higher reference point, induced by the workshop, causes higher current investment to finance higher future consumption and meet the reference point, matching the treatment effects we observe.

Fourth, we show that *improvements in economic conditions can raise aspirations for people living in poverty*. We offer participants in some villages large, lump-sum cash transfers of 2,237 USD PPP and placebo workshops (experimental arm 3), and compare them to those offered only placebo workshops (arm 1). Transfers are provided by the NGO GiveDirectly, are roughly 60% of mean annual household consumption, and offer a major opportunity for households to make investments and improve their living standards. The transfers not only raise labour supply, spending on productive inputs, revenue, consumption, and assets. They also substantially raise aspirations and expectations.[6] Interpreted through our model, these results suggest that cash transfers can raise reference points for future consumption – proxied by aspirations – and hence shift current expenditure toward investment to finance higher future consumption. These results raise the possibility that cash or other resource transfers can shift economic outcomes through both conventional wealth effects and psychological effects.

These improvements in economic conditions can raise aspirations enough that they no longer limit investment. To show this, we offer participants in some villages both the cash transfer and the aspirations workshop (experimental arm 4). This combined intervention and the cash transfer plus placebo workshop (arm 3) have nearly equal, positive, substantial effects on aspirations and expectations and on economic outcomes relative to the placebo workshop alone (arm 1). This suggests that cash transfers' positive effects on aspirations and hence on investment may crowd out the investment-promoting effect that the workshop has by itself.[7]

[6]Treatment effects are almost as large on the gap between aspirations and perceived current position for assets and income, so transfers do not simply raise aspirations by raising recipients' beliefs about their current position.

[7]Importantly, the heterogeneous treatment effects of the aspirations workshop would not have predicted this pat-

Our four-step argument shows that while people in poverty may form aspirations that are low enough to limit their investments, both psychological interventions and improvements in economic conditions can raise aspirations and contribute to higher investment and living standards. This is consistent with the idea that lower aspirations may lead to lower investment and hence to worse economic outcomes, entrenching poverty.

Our work contributes to the theoretical and empirical literature studying the origins and determinants of aspirations and their role in shaping investment and living standards. While theoretical work, outlined above, hypothesises a long causal chain from poverty to low aspirations to low investment to entrenched poverty, empirical work focuses on the relationship between aspirations and investment. This work has documented positive associations between aspirations and investment (e.g. Eble & Escueta 2022; Guyon & Huillery 2021; Janzen et al. 2017; La Ferrara 2019; Ross 2019) and studied interventions that might shift both aspirations and investment. One strand of work shows that exposure to *role models*, in real life (Beaman et al., 2012; Carlana et al., 2022; Macours & Vakis, 2014) or through videos (Bernard et al., 2023), raises both aspirations and investments in education.[8] A second strand studies multi-week *psychological training courses* to build aspirations, self-efficacy, delayed gratification and ambitious mindsets (Cecchi et al., 2022; Campos et al., 2017; McKenzie et al., 2022; Rojas Valdes et al., 2022). These produce mixed effects on both aspirations and other psychological mechanisms and on the economic investments they target: farming practices, microenterprise investment, and savings. These provide important evidence evaluating these interventions but leave open several questions about the relationship between aspirations, investment, and economic outcomes.

We make three contributions. First, we provide *the first experimental evidence that changes in economic conditions, such as wealth shocks from cash transfers, boost aspirations for people living in poverty.* We do this by randomising a wealth shock and measuring effects on aspirations. No work has studied effects of unconditional cash transfers (UCTs) on aspirations. Existing research studies conditional cash transfers (CCTs), which raise education aspirations (Chiapa et al., 2012; García et al., 2019; Macours & Vakis, 2014).[9] However, evaluations of CCTs cannot identify the effect of changes in wealth. CCTs combine regular cash payments, requirements for taking up health and education programmes, and/or regular meetings with programme staff. Even without the cash component, exposure to nurses and teachers – locally relevant role models – may increase aspirations. In contrast, our UCTs impose no conditions and increased interaction with enumerators

tern. The workshop is equally effective for households that are poorer versus richer at baseline, relative to the placebo.

[8]Other work on role models does not measure aspirations directly but shows related results: exposure to teachers and mentors shifts education outcomes (e.g. Carrell et al. 2010; Fairlie et al. 2014; Kofoed & McGovney 2019; Porter & Serra 2020), while exposure to role models in television shows shifts school performance, fertility and women's autonomy (e.g. La Ferrara 2016; Jensen & Oster 2009; Riley 2019).

[9]Note that Contreras Suarez & Cameron (2020) find no effects of a CCT programme on education aspirations.

is common to both the cash and placebo arms, so effects are likely due to the wealth shock.

More broadly, our work speaks to research on UCTs, which largely finds, as we do, that recipients of lump-sum UCTs substantially increase asset wealth and labour supply (Crosta et al., 2024; Egger et al., 2022; Haushofer & Shapiro, 2016). Higher aspirations may be one channel motivating recipients to make these investments. These findings are of key policy relevance, contradicting theories that cash transfers discourage work and investment.

We also speak to theoretical work on the determinants of aspirations. Our findings are consistent with the idea that aspirations are malleable: shaped by, as well as shaping, economic conditions (e.g. Dalton et al. 2016). In line with our results, interventions that offer new resources can directly raise aspirations and motivate investment, enhancing their impact on living standards.[10]

Second, we provide *the clearest evidence to date that an intervention targeting aspirations can facilitate broad-based rises in investment, and the first robust evidence linking this to improved living standards*. We do this by using a large enough sample to precisely study the full causal chain from aspirations to investment to living standards, measuring living standards as well as investment, and using a cluster-randomised design to avoid spillovers. Almost all the studies of role model exposure and psychological training courses described above focus on specific investments such as student effort, savings, or adoption of specific technologies. The only exception, Bernard et al. (2023), , measures living standards broadly but with less precision due to a smaller sample. They also randomise within villages, which might generate spillovers, leading to treatment effect estimates that are downward-biased – if role models generate positive economic or psychological spillovers – or upward-biased – if they lead to resentment or discouragement. Most studies of role models compare outcomes within schools or villages and face the same risks of spillovers.

Third, we provide *the clearest evidence to date that rising aspirations specifically can explain rising investment and living standards, rather than alternative psychological or non-psychological mechanisms*. We do this by comparing our aspirations workshop to an information-equivalent placebo and showing near-zero effects on alternative psychological mechanisms. In all studies of role models and psychological training courses described above, the interventions also provide specific information about possible avenues for investment.[11] So these studies cannot separate the role of aspirations, or indeed any psychological mechanism, from traditional economic models

[10] In contrast, others view aspirations as difficult to shift and shaped by early life experiences or long-run factors like culture or social hierarchies (Benabou, 1996; Durlauf, 1996; Genicot & Ray, 2017; Lybbert & Wydick, 2018).

[11] Experiments evaluating role model videos provide information by showing role models' investments, without using information-equivalent placebos (Bernard et al., 2023; Riley, 2019). In studies using variation in exposure to real-life models, role models can provide information and other important inputs. For example, in addition to acting as role models, female community leaders can change local policies (Chattopadhyay & Duflo, 2004) and teachers can teach skills and provide career guidance (Kearney & Levine, 2020).

of learning (Delavande, 2022; Genicot & Ray, 2020). In contrast, our placebo video contains all the information about specific investment activities and outcomes present in the aspirations video and we find no workshop-placebo differences in beliefs about returns to economic activities or mimicry of role models' actions. This shows that effects on economic outcomes must be driven by some psychological mechanism(s), distinct from information transmission. Our rich psychological measures allow us to show a large, persistent role for aspirations and near-zero effects on other plausible psychological mechanisms, including proxies for planning ability.[12] In contrast, the psychological training courses described above find mixed effects on aspirations and substantial effects on other psychological mechanisms, making it difficult to isolate a role for aspirations. Few studies of role model exposure directly measure aspirations. Those that do, do not rule out other psychological mechanisms (Beaman et al., 2012; Bernard et al., 2023; Macours & Vakis, 2014).

Our work complements but substantively differs from research studying intensive multi-week training interventions run by teachers or skilled facilitators which successfully change other mindsets, non-cognitive skills, or preferences through repeated exposure and practices. For example, interventions target patience (Alan & Ertac, 2018; Blattman et al., 2017), grit ("perseverance toward a set goal") (Alan et al., 2019), self-image and self-efficacy (Ghosal et al., 2020; McKelway, 2021) or innovative mindsets (Campos et al., 2017). Our intervention differs theoretically, by targeting a different psychological mechanism, aspirations; and practically, because it can be run by laypeople in one session. We show aspirations respond to simple, short exposures to similar role models and practices of setting long-term aims in populations unlikely to have encountered these practices elsewhere. Unlike many of these studies, we both show the intervention works through the targeted mechanism *and* rule out plausible alternative mechanisms, contributing to understanding of which beliefs, preferences, or soft skills respond to which types of intervention. Our work also differs from economic research on goal-setting interventions, which typically study "specific, measurable, attainable, relevant, and timed" goals (Goerg, 2015), rather than the broader, long-term aspirations we study. Our findings suggest an important role for aspirations in policy, both through policies that directly target aspirations and as a mechanism linking more conventional policies to economic investment decisions and outcomes.

2 Context and Sample

We study households living in poverty in all 415 villages in northern Homa Bay and southern Siaya, rural counties either side of Lake Victoria in Western Kenya. The area is one of the poorest in the country and is adjacent to areas where previous studies with the NGO GiveDirectly have

[12] Our measures of self-efficacy, locus of control, grit and growth mindset are self-reported, so we cannot completely rule out that the workshop improved these concepts. But increases in aspirations are at least part of the mechanism.

been conducted (Egger et al., 2022; Haushofer & Shapiro, 2016). The population is mainly Luo, Kenya's second largest ethnic group. The area is fairly densely populated, with 395 people per km^2 compared to 91 for the country as a whole. Villages contain 96 households on average. Villages have reasonable market access: most are within a few hours' drive of Kisumu, Kenya's third largest city, and closer to smaller towns. Roughly half of the villages contain a primary school, one third contain a market, and one sixth contain a healthcare clinic (Table B.2).

2.1 Target Population

Our population of interest is households living in poverty. To identify them, we conduct a census of 41,322 households in the 415 villages and collect data for a simple proxy means test used by GiveDirectly. The eligibility test assesses if households are eligible for cash transfers using a combination of easy-to-collect measures of housing quality and asset ownership, such as the size of the house and floor material. GiveDirectly has found these to be strong predictors of living below poverty lines defined in terms of consumption expenditure. 43% of censused households are classified as living in poverty on this test. Of the households classified as poor on the means test, 89% have per capita consumption below the World Bank's 2018 poverty line for Kenya in our survey data (described below).

We draw a random sample of roughly 20 households per village that are i) poor and cash-transfer-eligible on the means-test ii) contain an adult woman and iii) are non-polygamous.[13] This results in a baseline sample of 8,309 adult women. See Appendix B for details on the census, sampling, and eligibility rules. We focus on adult women because in this area, as in many parts of rural Africa, female-headed households are common and often poor: they are 32% of all households and 41% of means-tested-poor households.[14] Targeting our interventions at female heads of household or married women in couple-headed households enables us to design workshops targeted at one gender (by producing videos featuring female role models and hiring female staff to facilitate workshops) and include a high proportion of households living in poverty in these villages.

2.2 Sources of Data

We ran baseline surveys an average of 5 months before treatment. We completed all baseline surveys for all villages in an area before any interventions began or were announced. We surveyed the "primary adult woman" in each household: the female spouse in a couple-headed household or the widowed or single household head. We ran endline surveys an average of 17 months after treatment, finishing before the COVID-19 pandemic, attempting tracking of all migrants. We surveyed 87.1% of the baseline sample at endline. Attrition is balanced on treatment and

[13] We drop 4.5% of poor, cash-transfer eligible households without an adult woman and 11% which are polygamous.

[14] This pattern occurs because more men than women died during earlier waves of the HIV/AIDS pandemic (Sifuna et al., 2018). Single or widowed Luo women often do not have land rights and are thus poorer (Potash, 1978).

treatment×baseline covariates (Table B.1).

The baseline and endline surveys covered three prespecified household-level investment measures – labour supply, expenditure on productive inputs and hired labour, and children's education – and three prespecified economic outcomes – revenue, consumption expenditure, and asset value.

2.3 Sample Descriptives

We report summary statistics for the endline placebo sample in Table 1. We use endline so that we use the same time period and survey instrument as the treatment effects analysis; baseline statistics for the full sample are similar (Table B.2). See the Online Supplement for details on measurement.

The average respondent in the placebo group is 41 years old with an interdecile range of 23-65. The average household contains 2.8 adult members and 2.8 children, which includes biological and non-biological children of younger respondents and grandchildren of older respondents. 59% of the female respondents are married and the remainder are single (widowed, divorced or never married). Only 43% of respondents have completed primary education.

Households are living in poverty based on common metrics. Households consume an average of 3,796 USD PPP annually, or 2.5 USD PPP per adult equivalent per day. Households own non-land assets worth 1,529 USD PPP on average, less than half of the value of their annual consumption. The average household owns another 5,638 USD PPP of land and housing assets, although the values of land and housing are difficult to measure accurately as land transactions are rare.

Households engage in multiple economic activities: 95% of households grow crops, most commonly maize; 83% raise livestock, most commonly chickens, but also goats and cattle; 40% do casual or salaried work outside the household; and 44% operate a non-farm enterprise, most commonly retail (50%), manufacturing (23%), services, or fishing (11% each). Households have three main avenues for investment: labour supply, spending on inputs for home production, and children's education. The average household supplies 525 days of labour per year or 216 days per adult member per year and spends 857 USD PPP per year on productive inputs (Table 1). Households' labour supply and input expenditure generates mean annual revenue of 2,101 USD PPP.[15]

The average household spends 640 USD PPP per year on education, 183 USD PPP per child, mostly on school fees. Kenyan public primary and secondary schools nominally eliminated fees in 2003 and 2008 respectively but many still impose costs on parents and private schools charge fees (Glennester et al., 2011). School enrollment is 98% for ages 6-13, roughly corresponding to primary school age, and 79% for ages 14-20, a very broad definition of secondary school age. So there is limited scope for our treatments to affect school enrollment except for older teenagers.

[15]Our consumption measure is higher on average than our revenue measure, a common pattern in agricultural household surveys (e.g. Bandiera et al. 2017; Egger et al. 2022). We include the value of goods produced for home consumption in both the consumption and revenue measures.

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)
	Mean	Std dev.	10th percentile	90th percentile
Demographics				
Respondent Age	40.6	16.5	23.0	65.0
Household Size	5.56	2.53	2.00	9.00
Number of Children	2.77	1.86	0.00	5.00
Married	0.591	0.492		
Primary School Education	0.426	0.495		
Household Revenue and Living Standards				
Consumption	3,796	1,959	1,643	6,407
Consumption per Adult Equivalent	928	579	397	1,605
Value of Non-land Assets	1,529	1,506	289	3,219
Revenue	2,101	3,204	165	4,708
Household Economic Investment				
Labour Supplied (days)	525	347	84	960
Expenditure on Productive Inputs	857	1,932	23	1,973
Education Expenditure	640	940	42	1,703
Enrolment Rate for Ages 6-13	0.978	0.124	1.000	1.000
Enrolment Rate for Ages 14-20	0.788	0.343	0.000	1.000

Notes: This table shows endline summary statistics for the 1767 households of the placebo group. All currency values are measured in 2018 USD PPP. All flow measures except education expenditure are in annual terms. Details on measurement are given in the Online Supplement. Consumption expenditure captures the value of purchased and home-produced food, nondurable and durable household goods, and social expenditures following Deaton & Zaidi (2002). Consumption per adult equivalent is adjusted for household demographic composition using adult equivalent scales for Kenya following Anzagi & Bernard (1977). Non-land assets are the estimated value, if sold, of durable assets, livestock, and stocks of dried maize, as well as cash savings. Revenue captures the value of production sold or consumed at home from these activities, valued at farm-gate prices. Labour supply is days of work on farm and non-farm household enterprises or supplied to the market, for all household members older than 15. Input expenditure includes purchase of inputs and stocks and inventory, rental, maintenance, and expenditure on hired labour, for household activities in crop agriculture or livestock rearing, or for non-farm enterprises owned or operated by household members. We value output and expenditure on inputs following the Living Standards Measurement Surveys (Grosh & Glewwe, 2000) and UN System of National Accounts (FAO, 1996). Education expenditure is all fee and non-fee expenditure in the current and preceding school years summed over all household members aged 6-20. It is set to missing for households with no members aged 6-20.

3 The Psychological Correlates of Investment and Wealth

We describe relationships between wealth, investment, and psychological characteristics in our sample to motivate the idea that aspirations might affect investment and wealth and in turn be shaped by wealth. We measure seven psychological characteristics which predict investment or wealth in other contexts: aspirations and expectations for one's future economic position, beliefs about one's ability to carry out actions or achieve desired outcomes, beliefs about returns to specific investment activities, time and risk preferences, and depression. See the Online Supplement for more details on measurement.

3.1 Definitions and Measurement of Psychological Characteristics

Aspirations for One's Future Economic Position: We define aspirations as the set of future outcomes that people prefer and aim for. We measure aspirations as the level of economic outcomes respondents would like their household to attain in three domains: the levels of assets and income they would like their household to reach in ten years, and the level of education they would like one of their children to reach in adulthood.[16] We chose these domains because they are relevant to almost all respondents and because, in qualitative scoping work, most respondents described a better life in terms of improvements on these domains. We prespecified that we focus on an inverse covariance-weighted average over these three domains to capture a general aspirational mindset.[17] Similar measures of aspirations in other studies predict higher savings and education investment (Janzen et al., 2017; Ross et al., 2021; Beaman et al., 2012) and small business investment (Dalton et al., 2018).

Our respondents report aspiring to improve their current position. Aspirations are higher than respondents' beliefs about their current assets and income, but not dramatically, with median ratios of aspirations to current levels of 3 and 4.2 respectively.[18] Respondents' aspirations are lower than their beliefs about the highest levels someone from their village could reach in ten years: the median ratio of own aspirations to this "village maximum" is 0.5 for assets and 0.4 for income.

Aspirations can refer to more specific long-term goals (Locke & Latham, 2002): for example, one family in the video aspires to earn enough to put a tin roof on their house. We collect some data on participants' specific long-term goals from the workshop and combined groups: in the Best Possible Selves and goal-setting exercises, participants imagine their lives in five years "after everything has gone as well as it possibly could" and selected their most important and achievable goals. We find our measures of aspirations are informative proxies for having a specific goal in that domain: those with an education goal aspire to 0.25 more years of education; those with an income-related goal aspire to 739 USD PPP more annual income; those with an asset-related goal aspire to 1,376 USD PPP more assets. All differences are statistically significant. We also find suggestive evidence that people take some steps to implement what they planned: there are positive and mostly significant relationships between having a goal in a domain and spending in

[16] Before asking about aspirations, we ask respondents' beliefs about their current positions to provide anchors for their answers. For example, we ask "What is the total value of all the assets owned by your household?", and then ask "What is the level of assets that you would like your household to reach at the end of the next ten years?". To measure income aspirations we ask "What is the level of annual income that you think your household will reach at the end of the next ten years?" and to measure education aspirations, we ask "What level of education would you like [child name] to achieve?" for one of the respondent's children.

[17] Averaging multiple measures of related concepts to improve precision is common in aspirations research (Beaman et al., 2012; Bernard & Taffesse, 2014; Janzen et al., 2017) and econometrics in general (Schennach, 2022).

[18] Respondents' beliefs about their current assets and income, based on single questions, are similar to our main survey measures of their current assets and income, which we construct from their item-by-item survey responses. The median ratio of respondents' beliefs to the measures we construct is 0.75 for assets and 0.93 for income.

that domain. For example, the investment aggregate and spending on non-farm enterprise inputs are higher for respondents who have a business goal.[19]

Expectations: We directly elicit respondents' expectations about future assets and income, asking respondents what they think they will achieve instead of what they would like to achieve.[20] We also measure the level of education respondents expect a child to reach in adulthood (the same child as in the aspirations measure) and combine the three measures into an inverse covariance-weighted average. Such measures are widely found to affect investment (Delavande, 2022).

Our respondents' aspirations are closely related but not identical to their expectations, with $\rho=0.6$ for the two indices. We also measure respondents' expectations about their possible distributions of assets and income over the same time period as aspirations and estimate the individual-specific means of these distributions, following Dominitz & Manski (1997).[21] Our measures of aspirations capture something close to the upper limits of outcomes respondents believe are possible: the average respondent has asset and income aspirations at respectively the 71st and 66th percentiles of their individual expectations distribution and 85% of respondents have aspirations above the mean of their expectations distribution. Similarly, for the level of children's education, 21% of respondents have higher aspirations than expectations and only 3% have lower aspirations than expectations.

Self-Beliefs, including Planning and Implementation Ability: "Self-beliefs" are people's beliefs about their ability to achieve desired outcomes (Locke & Latham, 2002). We measure three different self-beliefs. Generalised self-efficacy is our best measure of the ability to plan and implement actions to achieve goals. It is a psychological scale which captures a person's self-reported ability to plan for and carry out actions and achieve their goals or desired outcomes across domains in their lives (Bandura, 1977). In other studies of the psychological techniques we test in the intervention, psychologists use the self-efficacy scale to capture whether people believe they have improved in their ability to plan and execute actions, problem-solve and overcome obstacles, behaviours targeted by the techniques (Conroy & Hagger, 2018; Kwasnicka et al., 2013). Sample items closely linked to these abilities include "It is easy for me to stick to my aims and accomplish

[19] We do not attempt to use such goals as an outcome measure because they are difficult to quantify or compare between respondents or over time. Most participants expressed their goals in qualitative terms e.g. "put an iron roof on my house" rather than quantitative values. And respondents set goals in diverse topics, in line with the intervention's aim for participants to articulate goals most important to them, as reported on page 17. We also did not ask the placebo and cash groups for their goals, as this could constitute a goal-setting intervention.

[20] To measure assets and income expectations, we ask the same questions we used for aspirations, replacing "you would like your household to reach" with "you think your household will reach". For education, we ask about the same child as for aspirations and replace "would you like [child name] to achieve?" with "you think [child name] will achieve?".

[21] Using the directly elicited expectations and the means of the expectations distributions yields almost identical inverse covariance weighted-averages, which have correlation 0.970 in the endline placebo group. We focus on the directly elicited expectations measure throughout the paper to align with the aspirations measure.

my goals", "I can always manage to solve difficult problems if I try hard enough" and "When I am confronted with a problem, I can usually find several solutions". We also capture locus of control, a person's beliefs about whether their actions or fate determine outcomes (Rotter, 1966), a measure widely used in economics, and growth mindset, a person's belief that their skills can be altered through effort (Dweck, 2012). We combine the three measures in an inverse covariance-weighted average, as they are correlated in our sample and in other work (Bong & Skaalvik, 2003), but also report effects on them separately. These measures predict labour supply, job search, saving, and educational effort in existing research (Caliendo et al., 2015; Dweck, 2012; Heckman & Kautz, 2012; John & Orkin, 2022; Lindqvist & Vestman, 2011; McKelway, 2021). We measure these beliefs on psychological scales validated in other contexts, which we translated and back-translated into Luo and validate to show they have appropriate psychometric properties (see the Online Supplement).

Beliefs about Returns to Investment: We measure respondents' beliefs about the returns to three potential investment activities available to most households: using more labour on their farm, using more fertiliser on their farm, and a university degree (vs secondary education) for their child. For example, we elicit beliefs about the levels of maize output if a farmer works 12 hours per week more for a given land size and set of inputs, and calculate the percentage change they expect relative to working their current hours. We picked these activities because most households face choices about whether to invest in them. Beliefs about these individual returns are broadly realistic, in that they are aligned with measures of actual returns from other work in this literature, as we discuss in the Online Supplement. Beliefs about returns to investment and/or labour supply predict investment in education, migration and economic activity in other settings (see Delavande (2022) for a review.)

Present Bias and Patience: We measure time preferences using an incentivised seven-choice multiple price list with choices over tomorrow vs 15 days and 15 vs 29 days (Andersen et al., 2008).[22] Individuals who place higher utility weight on future relative to current consumption have higher savings and educational attainment in many contexts (Falk et al., 2018).

Propensity for Risk-Taking: We create an ordinal measure of risk-taking from an incentivised Eckel & Grossman (2002) measure. Propensity to take risks is correlated with higher levels of technology adoption and business ownership, potentially because it lowers the value of certain consumption and leisure today relative to uncertain future outcomes (Falk et al., 2018; Liu, 2013).

Mental Health: We focus on depression, a common mental health condition. It is measured

[22]We capture patience via the discount factor, calculated assuming a linear utility function and using the switching points from receiving money soon to later in the 15 vs 29 day frame. Respondents are 'not present biased' if they do not switch to the (higher) future amount later in the near than in the future time frame. These measures are likely to be related to choices over monetary investment, even if there is debate over how much they capture underlying time preferences (Cubitt & Read, 2007).

with the standardised 10-item Centre for Epidemiological Studies-Depression scale (Andresen et al., 1994), which captures how often in the last week participants experienced symptoms of clinical depression: feelings of hopelessness about the future, fear and loneliness; impairment in motivation; inability to focus; and poor sleep (Beck & Alford, 2009). Treatments which reduce depression improve labour supply, savings and education investment (Angelucci & Bennett, 2021; Baranov et al., 2020; Lund et al., 2022). We reverse-code the scale so higher scores indicate higher mental health.

Relationships between Characteristics: Aspirations and expectations are closely related to each other, with $\rho = 0.6$, and weakly related to all other psychological characteristics we measure, with $\rho \leq 0.14$ (Table A.2). Regressing either of aspirations or expectations on all the other psychological concepts explains only 2% of their variation and a LASSO estimation approach gives similar conclusions (Table A.3). Their strongest correlation is with mental health, but is only 0.1-0.14 and is driven by the few mental health scale items conceptually related to aspirations, such as hope for the future. We conclude aspirations and expectations are closely related but not identical and are conceptually distinct from these other psychological mechanisms.

3.2 Relationships Between Psychological Characteristics and Investment and Wealth

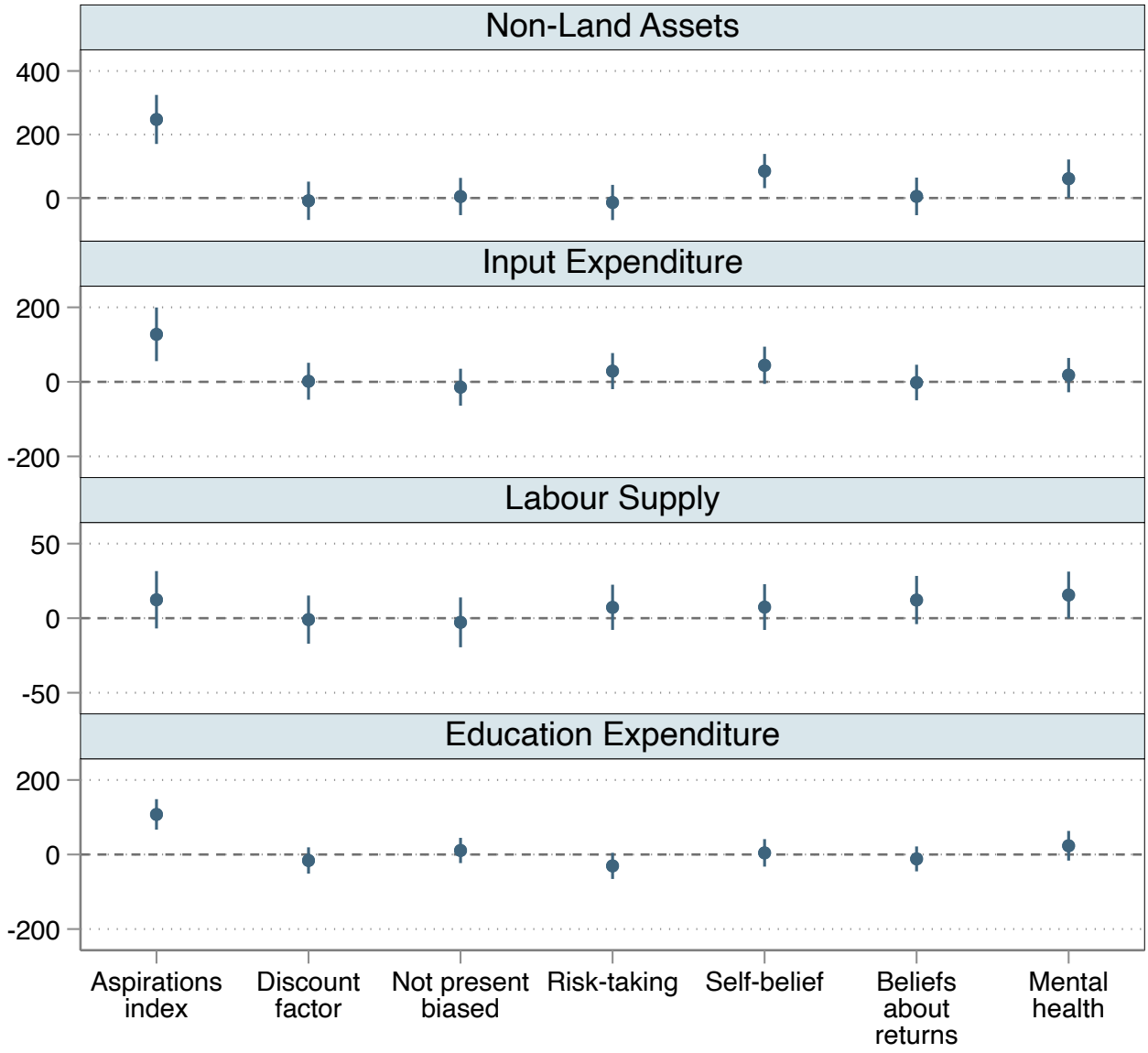
Here we describe four patterns in the relationships between investment, wealth, and psychological characteristics in the placebo group that motivate our intervention and conceptual framework.

First, aspirations are strongly positively associated with wealth. One standard deviation higher aspirations is associated with 247 USD PPP more non-land assets, controlling for all other psychological measures from the previous subsection, demographics, and geography (Figure 1, top row). Second, aspirations are strongly positively associated with investment, conditional on wealth. One standard deviation higher aspirations is associated with 128 USD PPP more input expenditure, 12 more days of household labour supply (not statistically significant), and 108 USD PPP more education expenditure, controlling for the other psychological measures, demographics, geography, and two wealth proxies: consumption and non-land asset value (Figure 1, bottom three rows). Jointly, these first two patterns are consistent with higher aspirations motivating investment and hence raising wealth, although other interpretations are obviously possible.

Third, the same regressions show that other psychological characteristics are at most weakly associated with investment and wealth. Wealthier people have marginally higher self-beliefs and mental health but these relationships are far weaker than the aspirations-wealth relationship, with all psychological measures standardised so the coefficient magnitudes can be compared (Figure 1, top row). None of the three investment measures is strongly associated with any non-aspirations psychological characteristic, conditional on wealth (Figure 1, bottom three rows).[23]

[23]Beliefs about returns to some specific activities are strongly associated with investments in those specific

Figure 1: Relationships Between Wealth, Investment, and Psychological Characteristics



Notes: This figure shows coefficients and 95% confidence intervals from regressing different wealth and investment measures – each shown in a different row of the figure – on psychological characteristics. The first row shows results from regressing a wealth proxy (non-land assets) on respondents' aspirations, self-beliefs, beliefs about returns, time and risk preferences, mental health, age, education, marital status, household size, number of school-aged members, and geography fixed effects. The second, third, and fourth rows show results from regressing investment measures (respectively expenditure on productive inputs and hired labour, labour supply, and education expenditure) on the same controls as in the first row, plus two wealth proxies: consumption and non-land asset value. All wealth and investment measures are defined in the footnote below Table 1. The aspirations index and psychological measures are defined in Section 3.1. All psychological measures are standardised to simplify comparison of the coefficients. All regressions use the endline placebo group data with the top percentiles of aspirations, investment, assets, and consumption trimmed. Sample size is 1376 to 1747 depending on the choice of controls and investment measure. The smaller sample sizes are for education expenditure, as this is set to missing for households with no school-aged children. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

Fourth, we show in the Online Supplement that our expectations index has almost the same relationship with wealth and investment as our aspirations index. We run aspirations and expectations analyses separately rather than including them in the same regression because the two are highly correlated and conceptually similar, as noted in Section 3.1.

These four patterns motivate a focus on aspirations and expectations as potentially important psychological influences on investment and wealth in this context. They are also consistent with a model we present in Section 6.3, where aspirations or expectations proxy for reference points for future consumption and people with higher reference points invest more to accumulate higher wealth and afford higher consumption in the future. However, these four patterns don't prove a causal effect of aspirations on investment and wealth, for which we turn to the experimental analysis.

The patterns shown in these regressions are highly robust: to removing demographic and geographic controls, adding respondent fixed effects using the panel structure, adding controls for cognitive ability, removing all psychological characteristics other than aspirations, measuring aspirations as the gap between aspirations and current economic position, using baseline rather than endline data, and allowing for nonlinear relationships.[24] See Appendix A.1 for full results. Standardised ridge regressions, a method to measure predictive power, classify aspirations and expectations as the strongest predictors of both wealth and investment.

4 Experimental Design and Interventions

Descriptive patterns in this sample suggest that higher aspirations and expectations for future living standards might raise investment and wealth accumulation. We hence run an experiment to test if a targeted workshop and/or cash transfers can raise aspirations and hence raise investment and living standards. We randomly assigned each of the 415 villages to one of four treatments: a placebo workshop ("Placebo"), the aspirations workshop ("Workshop"), an unconditional cash transfer and placebo workshop ("Cash"), or the cash transfer and aspirations workshop ("Combined"). Randomisation details are in Appendix B.2.

We use this four-armed design to make three comparisons. First, we compare the aspirations workshop to the placebo workshop to capture the effect of the psychologically active elements of the workshop, holding constant the effects of common elements in both workshops, such as information or interaction with facilitators. [25] Second, we compare the cash and placebo groups

activities; for example, both labour and fertiliser use in farming. This suggests beliefs are helpful for understanding domain-specific investments, even if they are less helpful than aspirations for understanding overall investment.

[24]Some papers have hypothesised or documented inverse-U-shaped aspirations-investment relationships, which they interpret as very high aspirations leading to frustration and lower investment (Genicot & Ray, 2017; Janzen et al., 2017). We find no evidence of an inverse-U-shaped relationship in our data, using spline regressions.

[25]We could not include an additional pure control group due to logistical constraints. This limits our ability to comment on the impact of the workshop as compared to a "status quo" group.

to capture the effect of cash transfers, holding constant the invitation to the placebo workshop. Third, we compare the cash and combined groups to capture the effect of the aspirations workshop in the presence of the large wealth shock delivered by the cash transfer.

4.1 Aspirations Workshop

We developed a unique workshop to achieve three aims: to raise participants' aspirations, while keeping them attainable; to encourage them to form specific long- and short-term plans to achieve these aspirations; and to encourage them to take actions in the present to begin working towards their aspirations. Workshops contain the following four elements.[26]

Aspirational Role Model Videos: The videos narrate the life stories of two fictional women from a similar area to the participants and from poor backgrounds. They succeeded in improving their socio-economic position by forming ambitious but attainable aspirations and working toward them over a number of years despite obstacles. In one story, Judy and her husband Oyoo aspire to own a house with an iron roof and send their children to higher levels of education. In the medium term, they plan to sell vegetables, for Judy to train to be a tailor, and to save more. Judy struggles to learn to sew, opens a sewing business and faces heavy competition, but eventually she establishes the business and achieves her aspirations. The second story, of Josefine, describes her journey from being a day labourer to continuing her education. The characters also "model" the techniques for setting aspirations and planning actions to achieve them that the participants then learn in the exercises and link their use to achieving one's long-term goals or aspirations.

We target workshops at one gender so characters in the videos seem relevant to participants. We use life stories of relevant role models setting aspirations because social learning theory in psychology suggests people often form aspirations with reference to the outcomes of other similar individuals (Bandura, 1977). Health research also suggests watching a relevant person "modelling" behaviour makes viewers more engaged and changes attitudes more than merely receiving information (La Ferrara, 2016).We make narratives inspirational and emotional, aiming to provide a "vicarious experience" (Bandura, 1977): a resonant and influential experience of the life of another similar person. Importantly, the characters are shown making modest progress over many years and facing difficulties and disappointments. This draws on psychology research which finds role models are most inspiring when people believe their success is attainable and are shown the process for achieving success (Lockwood & Kunda, 1997; Marx & Ko, 2012).

Best Possible Selves: After the videos, participants did a series of exercises. In the first exercise, participants are asked to think about and describe their lives in five years "after

[26]All videos are posted at https://mbrg.bsg.ox.ac.uk/aspirations-and-goal-setting-video-intervention. The Online Supplement contains a summary of the videos and exercise scripts for both interventions.

everything has gone as well as it possibly could" to inspire setting of aspirations (King, 2001). They are encouraged to think of "the realisation of their dreams" where they have "reached their full potential". Fieldworkers recorded the aspirations or long-term aims participants described.

Personal Goal-setting: In the second exercise, participants selected their most important and achievable aspiration or goal and report how long it would take to achieve it (Morisano et al., 2010). Appendix Table A.1 describes the diverse topics of these five-year aspirations in the workshop and combined groups: 78% of respondents report at least one goal related to farming (mostly raising a specific type of livestock) and the shares are 87% for housing, 51% for education, and 36% for non-farm enterprises. We did not promote setting aspirations or goals in any specific domain.

Mental Contrasting, Implementation Intentions, and Anticipation of Obstacles: In a third exercise, participants formulate "implementation intentions": a concrete step they could take in the next week towards achieving their aspiration for their life in five years (Gollwitzer & Sheeran, 2006). They identify potential obstacles and strategies to overcome these obstacles (Duckworth et al., 2013). They engage in "mental contrasting:" identifying how their lives would be improved and how they would feel if they achieve their aspiration (Oettingen & Gollwitzer, 2010).[27] Participants are given a reminder calendar, which shows the characters and sayings from each video, on which they placed a sticker to remind them of their aspirations.

The videos were filmed by a production company based on our scripts, with ordinary people who were paid to be amateur actors, near the study location. The stories are compilations of life stories of real people we collected in qualitative work. The videos were extensively pilot-tested in two rounds of focus groups and adapted. The workshop techniques draw elements from studies in psychology. We reviewed the psychological literature, mainly searching the American Psychological Association's PsycNet database, to identify short exercises that targeted one of the aims of the intervention. Each of the exercises has, on its own, had effects on lab tasks or intentions to undertake healthy behaviours or education effort in richer countries, but the psychology literature has not studied economic investment or living standards or longer time horizons (Cross & Sheffield, 2019; Duckworth et al., 2013; Gollwitzer & Sheeran, 2006; Loveday et al., 2018). Some related techniques are taught in schools in high-income countries (EEF, 2021; US Department of Education, 2007) but our participants are unlikely to have been exposed to them.The Kenyan primary school life skills curriculum did not include such techniques when our participants were at school (Kenya Institute for Curriculum Development, 2017) and only 43% completed primary school.

[27] In our pre-analysis plan, we stated the videos have elements to encourage a growth mindset (Dweck, 2012). However, subsequent work suggests growth mindset interventions must be highly specific and focus on specific ideas about neuroplasticity of intelligence (Yeager & Dweck, 2020), which our interventions do not.

4.2 Placebo Workshop

The placebo intervention includes two videos showing and describing all activities in the aspirations video. They describe farming in rural Kenya and education and types of work, mirroring the information available in the stories of Judy and Josefine. We made these videos by including at least one shot of every scene and character from the aspirations video but cutting out the "psychologically active" components – descriptions of characters or development of their stories, shots of people conveying emotion, or music – and replacing dialogue or narrative voiceovers with factual descriptions of the content of the scene. This is followed by exercises of similar length to the psychological techniques. In these, respondents recall and discuss the videos' content, accuracy and quality but do not learn the Best Possible Selves, goal-setting, mental contrasting, implementation intentions or anticipation of obstacles techniques. The placebo group also receive a calendar and stickers.

The placebo design rules out that effects are caused by participants learning about economic activities, or other factual information, from the videos. It also rules out effects of being selected for an intervention by people from outside the community or interacting with a facilitator, as this occurs in both interventions. Finally, it rules out the effects of meeting with a group, as both interventions have the same group structure.

4.3 Cash Transfers

The second intervention is a large, lump-sum unconditional cash transfer, delivered by our partner GiveDirectly, of 2,237 USD PPP, equal to 59% of mean annual consumption expenditure and 146% of mean non-land asset value in our sample. The transfer was delivered in three tranches of 203, 1,017 and 1,017 USD PPP one month apart via the mobile money payments system M-Pesa. The total transfer amount is comparable to current government pilot programmes targeted at the ultra-poor in other regions: the government's Hunger Safety Net Programme pays out the equivalent of the GiveDirectly transfer in 21 months (Kenya National Social Protection Secretariat, 2022).

4.4 Sample, Timing of Interventions and Experimental Integrity

All interventions are administered to our sample of baselined households, after the census and an average of 5 months after baseline. The workshops are delivered to the "primary adult woman" in each household, who also completes our baseline surveys, by specially trained Innovations for Poverty Action enumerators. The workshops involved two ten-minute videos shown on a tablet, followed by facilitated exercises. Median exercise times are 30 and 33 minutes for the placebo and aspirations workshops respectively. With introductions and pauses, the total median workshop time was roughly 80 minutes. The times, and authors' observations of the workshops, suggest participants were equally engaged. Workshops are mainly administered in groups. We randomly assigned participants in each

village into groups of up to four people and invited each group to a workshop at a specific time and location in their village. Those who missed meeting times received the intervention individually.

The cash transfer administration was part of GiveDirectly's normal programme. After baseline surveys and randomisation, GiveDirectly began operations in cash villages with a meeting to explain their programme, including that it targeted poor households and that transfers were from an independent non-governmental organisation. GiveDirectly then visited all cash-eligible households to offer them the transfer, making an extra visit if households were initially not at home. Households who signed up were asked to register for M-Pesa at a network of agents in most small stores. Each household picked one member whose M-Pesa account would receive the transfer: 91% picked the adult woman, who we also target for the workshop.

We worked with GiveDirectly to roll out all interventions within a sublocation, an administrative unit containing on average 10 contiguous villages, in the same month. Once a whole sublocation had been censused, baselined and registered for cash transfers, GiveDirectly sent transfers by mobile money on the 15th day of each month to that sublocation. Workshops were administered in the month around the date when GiveDirectly sent the first tranche of the cash transfer to villages receiving cash. Workshops were timed so all cash-eligible households knew they were receiving cash by the time they attended a workshop. We ran endline surveys an average of 17 months after treatment, finishing before the COVID-19 pandemic, attempting tracking of all migrants. We surveyed 87.1% of the baseline sample at endline. Endline is timed so that at least two growing seasons, one short rains season and one long rains season, have elapsed since treatment.

We report on balance, attrition and compliance in Appendix B. Attrition is balanced across treatment arms, only weakly related to baseline household characteristics, and balanced across treatment times baseline household characteristics (Table B.1). The treatment assignments are balanced on village-, household-, and respondent-level characteristics measured before treatment (Table B.2). All four experimental arms have equal take-up of the workshop they are offered – aspirations or placebo – and take-up of cash transfers is equal between the cash-only and combined arms.

4.5 Estimation and Inference

We estimate models of the form

$$Y_{iv} = \text{Cash}_v \cdot \beta_C + \text{Workshop}_v \cdot \beta_P + \text{Combined}_v \cdot \beta_{CP} + \mathbf{X_{iv}} \cdot \mathbf{\Gamma} + \epsilon_{iv}, \tag{1}$$

where i and v index individuals and villages. Y_{iv} is the post-treatment outcome of interest at endline. Cash_v, Workshop_v, and Combined_v are indicators for assignment to respectively cash transfers, the aspirations workshop, and the combined intervention. β_{CP} measures the effect of the combined intervention relative to the placebo, not the interaction effect. $\mathbf{X_{iv}}$ contains prespecified

covariates and sublocation fixed effects.[28] The covariates make little difference to the estimated treatment effects but lower the estimated standard errors. We report heteroskedasticity-robust standard errors clustered by village, the unit of treatment assignment. We control the false discovery rate across multiple tests by reporting sharpened q-values for the effect of each treatment and for tests of equality of each pair of treatment effects (Benjamini et al., 2006). We run all analysis at the household level except some individual-level education analyses in the appendices.

Our estimation and inference methods and outcome measures are prespecified at https://www.socialscienceregistry.org/trials/996. We make a few departures from the pre-analysis plan to improve comparability across economic aggregates and remove components with high measurement error. We list these in the Online Supplement and highlight two here. First, we adjust for multiple testing across the six economic aggregates and across all the main psychological mechanisms. This is more conservative than the prespecified adjustment, which was only across the prespecified components of each aggregate or index. Second, we summarise results by constructing a non-prespecified inverse covariance-weighted average of the six economic aggregates, following Anderson (2008).

5 Effects on Economic Outcomes

Table 2 shows treatment effects of the three interventions on six prespecified economic aggregates. To help interpret these results, we also discuss treatment effects on some components of these aggregates and show the component results in Appendix A. Since all economic aggregates are measured at the household level, we test if the interventions have affected household size or composition, including fertility. We don't find any impact on any of these outcomes.

5.1 Economic Effects of the Aspirations Workshop

The workshop has substantial effects on households' economic activities, increasing multiple types of investment, revenue, asset value, and consumption. The workshop increases households' annual labour supply by 27 days per year, 5% of the placebo mean (Table 2, column 2). This is driven by roughly equal increases in labour supply by the respondent and other adult household members, with no effect on children's labour supply (Table A.4). It also increases annual expenditure on intermediate inputs for home production and hired labour by 230 USD PPP, 27% of the placebo mean (Table 2, column 3), driven by increases in input expenditure (Table A.5). Both changes are concentrated in non-farm enterprises, where households work 9 more days and spend 174 USD

[28] The prespecified covariates are month-of-endline fixed effects (to account for seasonality); the baseline values of Y_{iv}; baseline household size, asset value, a self-beliefs index (made up of locus of control and self-efficacy scales, defined in Section 3.1), respondent education, and respondent age.

PPP more on inputs (Table A.5).[29]

The workshop increases annual revenue by 260 USD PPP, 12% of the placebo mean (Table 2, column 5). This increase is also concentrated in non-farm enterprises, where treated households earn 284 USD PPP more revenue, and earn 109 USD PPP more profit (Table A.5), although the latter effect is not statistically significant. The concentration of labour supply, investment and revenue effects in non-farm enterprises is consistent with a causal chain from treatment-induced investment to revenue. Effects are driven by growth of enterprises present at the baseline, rather than the creation of new ones requiring start-up capital. In contrast, crop- and livestock-raising contribute little to the treatment effects on input expenditure and revenue, even though there are two growing seasons between treatment and endline.

The workshop raises non-land asset value by 98 USD PPP: 6% of the placebo group mean (Table 2, column 6). The increase in asset stocks is mostly explained by small, non-lumpy items – durable assets and cash savings (Table A.6). Effects on livestock are driven by small livestock, not by lumpy purchases of large livestock that would be difficult to afford without a direct wealth infusion.

How did the households purchase additional inputs and assets? We provide evidence on three potential channels of behaviour change not requiring more financial resources. First, households supply more labour to revenue-generating household activities. Second, they increase the adoption of new business practices in non-farm enterprises, such as new/improved products and new customers/markets, which likely do not require large monetary investments (Table A.5, column 4). Third, there is a large increase in savings in the workshop arm of 26.4 USD PPP, 22% of the placebo mean (Table A.6, column 4). There are corresponding changes in savings behaviour: respondents join more savings groups, used as commitment savings devices in this area (Gugerty, 2007), and make more contributions to them.[30]

The workshop increases consumption expenditure by 142 USD PPP, 4% of the placebo mean (Table 2, column 7), typically interpreted as a rise in household welfare. This might be financed by the higher revenue generated by the treatment-induced rise in investment. The workshop raises both consumption and input expenditure, but it shifts the composition of spending from consumption toward investment. To see this, note that in the placebo group consumption is 81% of total expenditure (on consumption, productive inputs, and hired labour). But the treatment

[29]Breaking down effects by type of household economic activity is prespecified. All measures in Table A.5 are prespecified except returns to factors and profit. We code activity-specific measures as zeroes for households that do not engage in that activity to avoid sample selection problems. For example, we code non-farm enterprise investment and revenue as zero for households without non-farm enterprises. Hence treatment effects for the roughly half of the sample who own an enterprise are even larger.

[30]It is perhaps surprising that households living in poverty can build up savings. But the possibility that households living in poverty can save and yet do not always do so is consistent with some of the evidence and intertemporal choice models reviewed by Frederick et al. (2002).

Table 2: Treatment Effects on Economic Behaviour and Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Index Components					
	Economic Index	Labour Supplied (Days)	Inputs & Hired Labour	Education Index	Revenue	Non-Land Assets	Consumption Expenditure
Workshop	0.109***	26.8**	230**	0.019	260*	98**	142*
	(0.033)	(11.6)	(100)	(0.033)	(155)	(46)	(74)
	[.]	[0.069]	[0.069]	[0.127]	[0.069]	[0.069]	[0.069]
Cash	0.237***	27.2**	451***	0.049	465***	406***	322***
	(0.037)	(12.4)	(103)	(0.039)	(159)	(50)	(77)
	[.]	[0.012]	[0.001]	[0.036]	[0.003]	[0.001]	[0.001]
Combined	0.236***	9.0	653***	0.074**	546*	352***	232**
	(0.059)	(11.5)	(214)	(0.036)	(303)	(47)	(95)
	[.]	[0.096]	[0.006]	[0.033]	[0.046]	[0.001]	[0.022]
P: cash	0.001	0.972	0.040	0.435	0.230	0.000	0.032
= workshop	[.]	[0.526]	[0.073]	[0.354]	[0.208]	[0.001]	[0.073]
P: cash	0.983	0.127	0.357	0.532	0.801	0.292	0.367
= combined	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
P: workshop	0.007	0.118	0.007	0.121	0.217	0.000	0.327
= combined	[.]	[0.138]	[0.018]	[0.138]	[0.211]	[0.001]	[0.222]
P: cash	0.067	0.007	0.881	0.910	0.536	0.025	0.049
+ workshop	[.]	[0.046]	[0.835]	[0.835]	[0.674]	[0.066]	[0.081]
= combined							
Placebo mean	-0.000	525	857	-0.000	2,101	1,529	3,796
# clusters	413	413	413	412	413	413	412
# obs	7,243	7,240	7,243	6,379	7,243	7,242	7,224

Notes: This table shows household-level treatment effects of the interventions on six prespecified economic outcomes and an index combining them. All currency values are measured in 2018 USD PPP in annual terms. The outcome in column (1) is an inverse covariance-weighted average of the outcomes in columns (2)-(7), following Anderson (2008). The outcome in column (4) is an inverse covariance-weighted average of education expenditure and days of school attended in the last week (set to zero for the non-enrolled), both averaged over the number of household members aged 6-20. Education expenditure outcomes in all other columns use the same definitions as in Table 1. Coefficients are from OLS regressions of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village, in parentheses. Sharpened q-values controlling for the false discovery rate across all outcomes except the summary index, separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. The number of observations varies slightly across columns because some respondents don't answer all questions, which also drops one small village.

effect on consumption is 36% of the treatment effect on total expenditure. This is consistent with the workshop's emphasis on working and investing towards long term aspirations.

One exception to this general pattern is that the workshop does not substantially shift an index of education investments, made up of expenditure, enrolment and attendance for all school-aged members age 6-20 (Table 2, column 4). A prespecified breakdown by age shows an 11% rise in expenditure per primary school-aged child but negligible effects on expenditure per older child (Table A.7), possibly because secondary school costs are larger and lumpier (Lucas & Mbiti, 2012) and treated households do not have sufficient resources to make them. There are few effects on enrolment and attendance, perhaps because these are already high for all but older teenagers (Table A.8).

These results provide strong evidence that the workshop substantially shifts economic investment and living standards. The workshop shifts a summary index of the six aggregate measures by 0.11 standard deviations (Table 2, column 1). Effects on five of the six economic aggregates are statistically significant, even after adjusting for multiple hypothesis testing over the six outcomes, a more stringent adjustment than we prespecified. We can also run non-prespecified analyses that combine the three investment measures in columns 2-4 of Table 2 into a single "productive investment" index and combine the three other measures in columns 5-7 of Table 2 into a single "living standards" index. The workshop has significant effects of 0.1 standard deviations (standard error 0.036) on the productive investment index and 0.1 standard deviations (standard error 0.038) on the living standards index. These are large effects for an 80-minute workshop occurring 17 months earlier. Effects may persist or increase if households sustain behaviours kickstarted by the workshop, such as higher labour input, revenue generation, or continued saving and asset accumulation. There is some evidence from other interventions that effects on investment may persist over a five-year horizon (Bernard et al., 2023). Conversely, effects might fade if households forget the intervention, though 90% still recalled the workshop's main characters at endline, indicating it is memorable.

5.2 Economic Effects of Cash Transfers

The cash transfer increases our summary index of the six economic aggregates by 0.24 standard deviations, roughly twice the workshop's effect (Table 2, column 1). Given the large resource injection of the cash transfer, its effects on all pecuniary outcomes are unsurprisingly larger than the workshop: roughly double the size for input expenditure and revenue; triple for consumption; and quadruple for assets, although not all differences are statistically significant. Effects are similar to those of the same cash transfer programme in a nearby area (Egger et al., 2022). Our results are consistent with Crosta et al. (2024), a meta-analysis of lump-sum unconditional transfer effects, which finds positive effects on consumption, revenue and assets over the time horizon we study. They find these persist for assets but not consumption in the longer-run.

Effects on non-pecuniary investments are similar across the workshop and cash transfer. Both interventions have similar positive effects on annual household labour supply.[31] Both interventions increase adoption of new business practices in non-farm enterprises, such as new/improved products and new customers/markets (Table A.5, column 4). Cash has a larger effect on technology adoption in agriculture than the workshop, but the difference is not statistically significant.

5.3 Economic Effects of the Combined Intervention

The combined intervention increases all economic aggregates we study but does not have systematically larger effects than the cash transfer alone. In particular, the cash and combined interventions raise the economic index by nearly identical amounts: 0.237 and 0.236 standard deviations, respectively (Table 2, column 1). This is potentially surprising: the workshop raises the index by 0.109 standard deviations relative to the placebo, substantially and statistically significantly larger than the 0.01 standard deviation effect of adding the workshop to the cash transfer. Across the six economic outcomes in Table 2, there is no clear evidence that either intervention produces systematically higher effects than the other on the three investment measures or the three living standards measures.[32] We discuss potential explanations for the similarity of the cash and combined intervention effects in Section 6.5.

5.4 Heterogeneous Treatment Effects of the Interventions

All our interventions produce broad-based economic improvements, as no intervention has treatment effects that vary substantially across different types of respondents. To show this, we estimate heterogeneous treatment effects on the summary economic index over eight prespecified baseline measures. We use two approaches: regressions with treatment $\times$ covariate interactions and causal forests (Wager & Athey, 2018). Both approaches give similar results: the workshop's effects do not vary substantially by baseline wealth; by baseline values of the targeted psychological concepts (aspirations and expectations); or by respondent age, education, household size, or marital status, even though our sample includes people with a wide range of demographic characteristics.[33] The

[31] The meta-study by Crosta et al. (2024) also documents generally positive effects on labour supply.

[32] The cash and combined interventions have very similar effects on the (non-prespecified) "productive investment" index of investment measures in columns 2-4, of 0.18 and 0.21 standard deviations respectively. They also have similar effects on the "living standards" index of measures in columns 5-7, of 0.26 and 0.23 standard deviations. The cash-combined difference in effects on the education index occurs because the combined intervention has a larger effect on education expenditure (Table A.7); although neither intervention increases enrolment or attendance (Table A.8).

[33] Although our eligibility criteria mean that everyone in the sample is relatively poor, there is still substantial variation in baseline wealth that we use to estimate heterogeneous treatment effects: standard deviation 1,496 USD PPP and interquartile range 1,248 USD PPP, respectively 119% and 99% of the mean. Consistent with the lack of heterogeneous effects by marital status, Mahmud et al. (2022) find few effects of the individual interventions on intrahousehold relationship quality, decision-making, or intimate partner violence in a subsample of married women in one part of the study area.

effects of cash transfer alone and the combined intervention also do not vary substantially over any of these characteristics. See the Online Supplement for detailed methods and results.

6 Mechanisms

In this section, we provide evidence that aspirations and expectations are the most plausible mechanism for the economic effects of the interventions. First, we show the workshop increases aspirations and expectations but has no effects on the other candidate psychological mechanisms, including proxies for planning. Second, we rule out some alternative non-psychological mechanisms. Third, we present a simple conceptual framework to define aspirations and expectations in terms of economic concepts and show theoretically how they might affect investment and wealth. Fourth, we show that the cash transfers also raise aspirations and expectations and present suggestive evidence that this influences how recipients invest the transfers. Finally, we show that the cash and combined interventions have similar effects on all psychological mechanisms, including substantial positive effects on aspirations and expectations and argue that this explains their similar effects on economic outcomes.

6.1 Effects of the Workshop on Psychological Mechanisms

Table 3 shows effects of the workshop on a range of candidate mechanisms that might plausibly explain households making increased investments. We code all measures so that higher values are theoretically associated with higher investment: e.g. we report effects on mental health (the Z score on a depression scale multiplied by negative one). We report sharpened q-values adjusting for multiple testing across all mechanisms. This is conservative because we prespecified time and risk preferences and mental health as unlikely mechanisms that we measured only for the purpose of ruling them out.

Aspirations and Expectations: The aspirations workshop increases the index of aspirations for the future by 0.092 standard deviations relative to the placebo group (Table 3, column 1). This effect is broad-based: it is non-negative at all quantiles, with no clear pattern of larger effects at higher or lower quantiles, and the average effect does not vary substantially by baseline aspirations or other baseline characteristics (see the Online Supplement). We focus on the prespecified index rather than the three domain-specific aspirations measures because the index best captures a general aspirational mindset that might alter investment in many domains of life. Combining the three domain-specific aspirations measures into an index also allows more precise estimation (Schennach, 2022). The increase in the aspirations index is driven more by a statistically significant increase in aspirations for children's education than by effects on assets and income aspirations (Table A.9). However, changes in one domain of aspirations need not only result in changes in investment in that specific domain: for example, higher aspirations for children's education might motivate higher investment in non-farm enterprises to generate revenue

and higher savings in the period we observe, to finance higher education spending in future.

The workshop also increases an index of expectations for participants' future outcomes by 0.091 standard deviations relative to the placebo group (Table 3, column 2).[34] The increases across domains, patterns across quantiles and lack of heterogeneous effects mirror those for aspirations. The similarity of aspirations and expectations effects suggests that participants view their higher aspirations as attainable. The aspirations and expectations effects are statistically significant at the 1 and 5% levels respectively. They remain significant when we adjust for multiple hypothesis testing across all eight mechanisms. No effect on any other mechanism is large or statistically significant.

The positive effects on aspirations and expectations do not occur via increases in participants' beliefs about their current economic positions. Recall from Section 3 that we measure participants' beliefs about their current assets and income before measuring their aspirations. We construct an index of aspirations minus current perceived position using these asset and income measures, along with participants' aspired education level for their child minus their own education level. The treatment effect on this index is only slightly smaller than on aspirations itself, and a similar pattern holds for expectations minus current perceived position. This shows that treatment raised aspirations and expectations for their economic futures, rather than simply changing participants' beliefs about their current economic position.

Self-Beliefs, including Planning Ability and Grit: Treatment has no effect on the pre-specified index of three psychological scales capturing self-beliefs (Table 3, column 3) or on any of the individual scales (Table A.10, columns 2-4). One of the individual scales, the self-efficacy scale, is our main measure of planning and implementation ability. It is the main measure psychologists use to capture people's self-reported capacities to plan and execute actions, problem-solve and overcome obstacles in studies of the psychological techniques we test in the intervention (Conroy & Hagger, 2018; Kwasnicka et al., 2013). We report effects on the scale and each of the seven items which make up the scale (Table A.11). We find no effects on self-efficacy as a whole or on items particularly linked to planning ability, such as whether participants report finding it easy to stick to aims and accomplish goals or to solve difficult problems with effort.

The second individual scale in the self-beliefs index, the "locus of control" scale, captures a person's beliefs about whether their actions or fate determine outcomes (Rotter, 1966). We also see no effects on this measure (Table A.10, column 3).

The last of the prespecified individual scales in the self-beliefs index is the "growth mindset" scale (Blackwell et al., 2007), capturing a person's beliefs about the malleability of ability (if they believe that their skills can be altered through effort). We see no effects on this measure (Table

[34]We estimate the impact on the standard deviation and the interquartile range of the respondents' expectations index distribution and find that the dispersion of the distribution is unaffected.

Table 3: Treatment Effects on Psychological Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Aspir-ations Index	Expect-ations Index	Self-belief Index	Returns Index	Discount Factor	No Present Bias	Risk-taking	Mental Health Z-score
Workshop	0.092***	0.091**	0.006	-0.011	-0.006	-0.023	-0.018	-0.015
	(0.035)	(0.039)	(0.046)	(0.037)	(0.011)	(0.016)	(0.038)	(0.035)
	[0.078]	[0.079]	[1.000]	[1.000]	[1.000]	[0.428]	[1.000]	[1.000]
Cash	0.130***	0.178***	-0.053	-0.002	-0.002	-0.015	-0.014	0.086**
	(0.036)	(0.038)	(0.042)	(0.038)	(0.011)	(0.016)	(0.036)	(0.034)
	[0.002]	[0.001]	[0.344]	[0.907]	[0.907]	[0.526]	[0.854]	[0.027]
Combined	0.178***	0.134***	0.025	-0.002	-0.003	-0.028*	-0.030	0.044
	(0.040)	(0.043)	(0.044)	(0.036)	(0.011)	(0.017)	(0.033)	(0.035)
	[0.001]	[0.006]	[0.668]	[0.908]	[0.788]	[0.235]	[0.594]	[0.339]
P: cash	0.324	0.042	0.198	0.830	0.728	0.617	0.927	0.006
= workshop	[0.948]	[0.171]	[0.657]	[1.000]	[1.000]	[1.000]	[1.000]	[0.052]
P: cash	0.259	0.331	0.043	0.997	0.935	0.402	0.696	0.207
= combined	[1.000]	[1.000]	[0.528]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
P: Workshop	0.027	0.319	0.687	0.813	0.781	0.768	0.777	0.097
= combined	[0.277]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.515]
P: cash	0.396	0.019	0.224	0.825	0.728	0.652	0.958	0.581
+ workshop	[1.000]	[0.179]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
= combined								
Placebo mean	0.000	-0.000	0.000	0.000	0.698	0.753	0.000	-0.000
# clusters	413	413	413	413	413	413	413	413
# obs	7,232	7,233	7,221	7,110	7,243	7,243	7,170	7,213

Notes: This table shows treatment effects of the interventions on psychological outcomes that might explain the treatment effects on economic outcomes. Measures are defined briefly in Section 3.1 and in more detail in the Online Supplement. Most outcomes are inverse covariance-weighted averages of multiple measures so the treatment effects are in standard deviation units, following Anderson (2008). Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For psychological outcomes where the order in which questions appear in the survey are randomised, a set of order indicator variables are also included as additional controls. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across all outcomes separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. The number of observations varies slightly across columns because some respondents don't answer all questions, which also drops one small village.

A.10, column 4). Growth mindset is also argued to be one of two elements of grit (Alan et al., 2019, 2), an individual's "perseverance toward a set goal ... closely related to conscientiousness", so this finding shows the workshop does not work by activating this component of grit. The second element of grit in Alan et al. (2019) is "effort beliefs": if individuals believe goals can be achieved with effort and tend to persevere despite obstacles. We create a non-pre-specified scale

to proxy for this concept, using a subset of items from the self-efficacy and locus of control scales, such as "When I get what I want, it's usually because I worked hard for it". We also find no effects on our measure of this second component of grit (Table A.10, column 5).

It is perhaps unsurprising that a short workshop does not change these underlying non-cognitive skills or capacities: while such traits can be altered, this is largely documented with much more intensive interventions (Alan et al., 2019; Ghosal et al., 2020; McKelway, 2021).[35] Results also align with studies in psychology of the techniques in our workshop, which find that the individual psychological exercises do not change self-efficacy or locus of control, even when they change intentions to undertake future-oriented behaviours, like use of preventive health interventions or exerting effort in education (Conroy & Hagger, 2018; Kwasnicka et al., 2013).

Information Acquisition, Beliefs about Returns, Mimicry or Recall: We find little evidence that the workshop changes beliefs about returns to specific activities. There is no effect on beliefs about returns to labour on the farm in terms of yields or to investment in university education in terms of wages (Table A.12). We measured these beliefs because the videos contain characters making these investments. This is unsurprising: we minimise the potential for learning about the returns to activities from the workshop by comparing the aspirations workshop group to the placebo group, who receive the same images and descriptions of economic activities. We also find no effect on beliefs about returns to fertiliser, an input not featured in the videos, nor on an index of all three beliefs (Table 3, column 4).

We also test if respondents simply copy specific activities depicted in the aspirations and placebo videos. The workshop does not affect an index of five dummy variables capturing if respondents mimic activities shown in the videos: weaving baskets, keeping savings in jar, taking a sewing class, training as a teacher, or growing vegetables for market sale (Table A.13, column 6). This is perhaps because the activities were common – 28% of placebo group respondents engage in at least one. Few respondents in the workshop or combined groups set goals for the same activities shown in the videos. Only 3% of respondents set goals in tailoring, Judy's core activity in the video. Only 1% set a savings goal and 7% a goal related to farming infrastructure, although savings and building a greenhouse were described in the videos.

Finally, we examine if people are more likely to recall or pay attention to information about activities in the workshop because it is more entertaining (Hanna et al., 2014; La Ferrara, 2016). In both the aspirations and placebo videos, we include information that having secondary education in neighbouring counties of Western Kenya increases the likelihood of salaried work, rather than working in self-employment, taken from Ozier (2018). The workshop does not affect recall of this

[35] Growth mindset can respond to short interventions, but the literature finds these need to be highly specific, focusing on messages that skills are malleable and can grow with effort (Yeager & Dweck, 2020).

information relative to the placebo (Table A.13, column 5). Thus, while we cannot measure beliefs or recall over all information in the video, we find no direct evidence for a framework based on information acquisition, mimicry of particular activities or improved recall of information.

Time and Risk Preferences: The workshop has negligible effects on risk preferences and on two proxies for patience over monetary payments: the discount factor and an indicator for not being present biased (Table 3, columns 5-7). We prespecified that we did not expect changes in these preferences. Other light-touch interventions also find no effects on these conventional measures of time preferences (John & Orkin, 2022).

Mental Health: The workshop has a very small, non-significant effect of 0.015 standard deviations on mental health (Table 3, column 8) and near-zero effects on an indicator capturing whether an individual meets criteria for clinical depression (Table A.10, column 5). We prespecified changes in mental health as unlikely: the mental health scale captures symptoms of clinical depression such difficulty sleeping, lack of motivation, focus and feelings of fear and loneliness which require more sustained intervention to reduce. The workshop is shorter than most therapies, which take 6 to 20 sessions, and omits many key elements of common therapies (Cuijpers et al., 2018). The workshop's effects on aspirations and economic outcomes do not vary by baseline mental health and effects on mental health do not vary by other baseline characteristics. See the Online Supplement for details. We unfortunately did not include a direct question on subjective well-being. We analyze the impact on two items of the CES-D scale, "I was happy" and "I felt hopeful about the future' which can proxy for subjective well-being. We find no impact of the workshop on these outcomes including on any part of the distribution of these items.

6.2 Ruling Out Alternative Non-Psychological Mechanisms

Experimenter Demand: Our measures are based on self-reported data, as is common in studies in poor rural settings with very little administrative data. Participants in the workshop may give answers that they believe the surveyors want to hear, perhaps reporting choices consistent with what they reported in the exercise. Five pieces of evidence suggest this does not drive effects.

First, for two prespecified outcomes, we collect both self-reported answers from respondents and objective measures from the enumerators. Respondents answer these questions without knowing that enumerators will check these answers. If the treatment effects of the workshop were driven by socially desirable reporting, we would see larger treatment effects on the self-reported measures than the verified measures as households would be inflating self-reported measures. Similarly, we would see larger differences between the combined and cash arms on self-reported measures than verified measures.

We do not find evidence consistent with experimenter demand using our first measure, of

housing expenditure and quality. Column 1 of Table A.13 shows self-reported measures of housing expenditure (in USD PPP) and Column 2 shows enumerator ratings of the expense of housing materials on a standardised scale from one to four that rates more expensive materials higher (Arias & De Vos, 1996). The workshop has no effect on either self-reported housing expenditure nor verified housing quality, even though one household in the video is shown buying a higher-quality roof so respondents might think reporting housing expenditure was socially desirable. The results in the cash and combined arms also do not suggest experimenter demand: both interventions have large positive effects on both self-reported housing expenditure and verified housing quality. The difference between the cash and combined arms is negligible for both the self-reported and the verified measure.

Similarly, we do not find evidence consistent with experimenter demand using our second measure, of small asset ownership. Column 4 captures enumerators' count of the number of seven easy-to-observe asset types that the household owns, which was done at the end of the survey and not preannounced. Column 3 reports on the self-reported number of these assets from earlier in the survey. The treatment effect of the workshop are consistently zero on both self-reported and enumerator-observed measures of these assets.[36] Again, there is no sign the combined arm inflate their reports relative to the cash group: enumerators observe increases in the number of assets households owned by households in the cash and combined arms; both arms report similarly on these increases. Table A.14 shows treatment effects for each of the seven assets that households report owning (Panel A) and that households are verified as owning (Panel B). Again, treatment effects are highly consistent across self-reported and verified measures for all three treatment groups.

Second, the enumerators did not know the treatment status of the villages, limiting scope for them to prompt or probe respondents differently. And the placebo-controlled design means that even if respondents discussed the workshop during the endline survey, enumerators could not easily tell if they were referring to the aspirations or the placebo workshop.

Our third piece of evidence against experimenter demand effects comes from Section 6.1, where we showed workshop participants are not more likely to report doing specific activities depicted in the video. Fourth, the workshop does not increase self-reported education spending for secondary school children (Table A.7) and no intervention increases school enrolment or attendance (Table A.8), even though these are socially desirable behaviours depicted in the videos. Fifth, the workshop and placebo arms had equally low shares of respondents who viewed our enumerators as associated with specific types of organisations, including GiveDirectly, suggesting respondents are not trying to answer in a way that pleases them or hoping for future help.

Spillovers: Many existing experiments randomise psychological interventions within villages,

[36]The workshop does not increase self-reports of the number of the these specific seven specific asset types but it does increase the total self-reported value of *all* assets including livestock, savings and durables (Table 2, column 6).

schools, or even within community/social groups. These risk spillover effects on untreated people that may lead to treatment effect estimates that are downward-biased – if the interventions have positive economic or psychological spillovers – or upward-biased – if untreated people are resentful, discouraged, or face higher economic competition. Our village-level randomisation reduces spillover effects on untreated people because we compare treatments to a placebo group located in different villages. We test for within-village spillovers by comparing outcomes between villages assigned to the aspirations workshop and the placebo, for a random sample of roughly 2,800 households who are too wealthy to receive the cash transfer on GiveDirectly's criteria. Spillover effects of the workshop on the main psychological and economic outcomes are mostly small and none are statistically significant after correcting for multiple hypothesis testing (Tables A.15 and A.16). The lack of within-village spillovers suggests that spillovers onto the placebo group, who live farther away in separate villages, are unlikely. However, we view these results with some caution as untreated respondents within the same village may be affected by knowledge that others received interventions. We also test in a companion paper whether participants talk to each other or to non-participants about the workshop content (Garlick et al., 2022). We find little evidence of this: people talk infrequently with others about their goals and challenges – fewer than two on average in the last 12 months – and this discussion is concentrated within villages. It is possible that there are cross-participant spillovers within the treated villages, in which case our estimated treatment effects might differ if the workshops were administered to a higher or lower share of people in treated villages.

Group/facilitator effects: As discussed in Section 4.1, the placebo-controlled design means that interaction with outsiders or meeting with a group should not drive the workshop's effects. We also see no evidence that group delivery or composition drives results. Group size is balanced across treatment arms. The share of participants who report at endline that they still talk regularly with members of their workshop group is similar across all four treatment arms. We also find no heterogeneity in effects on the economic or aspirations index when we compare respondents who receive the workshop in groups to the sample of recipients who receive the intervention individually because they miss their group meeting (see Online Supplement). Our main findings are robust to using facilitator fixed effects.

Hawthorne or John Henry Effects: In experimental settings where the control group are aware others receive a different treatment, they could change reporting or behaviour because they are disappointed or believe the treatment group have received information that advantages them. Alternatively, the treatment group may believe they have particular attributes which have led to their selection. Our village-level design largely rules out potential effects from knowledge that others had received a different intervention.

6.3 A Conceptual Framework Linking Psychological and Economic Outcomes

We propose a simple framework to show how the aspirations workshop can activate a causal chain from aspirations to investment and other economic outcomes. We summarize the framework here and present the full version in Appendix C. Our framework closely follows the way other authors have modelled the economics of aspirations because our paper's primary contribution is empirical rather than theoretical (e.g. Dalton et al. 2016; Genicot & Ray 2017; Lybbert & Wydick 2018).

Our framework is one of reference-dependent utility (Kőszegi & Rabin, 2006, 2007). In each period, people in our framework allocate their time between leisure and labour, and allocate their financial resources between consumption and investment. Leisure and consumption generate current utility, while labour and investment generate future financial resources. To maximise lifetime utility, people choose allocations that equate the marginal utilities of current consumption and leisure with the discounted marginal utility of future consumption arising from current investment and labour. Utility from future consumption depends partly on a reference point: an idea about the appropriate or desired level of consumption. People receive an extra utility gain, an "aspirational payoff", from reaching or exceeding their reference point for consumption, while missing it decreases utility. We interpret aspirations as empirical proxies for reference points. We follow the existing literature in assuming that reference points are malleable – so the workshop can raise reference points – but that people do not endogenously choose their reference points.[37] We assume utility over consumption relative to the reference point is concave, like most other work. This means that if the workshop raises the reference point for future consumption, then the marginal utility of each level of future consumption rises relative to current consumption. People therefore raise their current labour and investment and lower their current leisure and consumption until current and future marginal utilities are again equalised.

Expectations can enter this framework in two ways: as another proxy for the reference point, akin to aspirations; or as beliefs about the outcomes of investment decisions that depend on aspirations. Our data are consistent with both interpretations. See Appendix C for more discussion.

In Appendix C, we also develop alternative frameworks that capture each of the other psychological mechanisms we measure. We show that workshop-induced increases in aspirations, self-beliefs (including proxies for planning skill), beliefs about returns to investment, patience, risk tolerance, and mental health could theoretically produce the same economic effects. Our rich data on psychological mechanisms allows us to show that the workshop is more likely to affect outcomes through aspirations or expectations than these alternative mechanisms, a conclusion

[37] Otherwise, people would set their reference points to zero so that they could always exceed their the aspirations by as much as possible, irrespective of their economic environment or receipt of interventions.

we could not have reached using only data on the economic effects.

6.4 Effects of the Cash Transfer on Psychological Mechanisms

The cash transfer substantially increases both aspirations and expectations, by respectively 0.13 and 0.18 standard deviations (Table 3, columns 1-2). These effects do not just occur because the cash transfers raise recipients' current wealth, as treatment also increases the gap between aspirations and respondents' beliefs about their current economic position.[38] The positive effect of wealth shocks on aspirations is a novel finding, which advances work on reference point formation (e.g. Kőszegi & Rabin 2006) and shows that aspirations can respond relatively quickly to changing economic circumstances as in Dalton et al. (2016), rather than depending mainly on long-term circumstances or culture (Genicot & Ray, 2017; Lybbert & Wydick, 2018).

Interpreted using our reference-dependent utility model, these results suggest that cash transfers can influence economic outcomes through both a conventional wealth effect and a psychological effect. The psychological effect may occur because cash transfers raise the reference point for future consumption, proxied by aspirations. This may motivate people to invest more of the cash transfers than in a model without reference-dependent utility. Separately identifying this possible psychological effect from a wealth effect is difficult: if the psychological effect does exist, then it is not possible to experimentally manipulate wealth without activating both the wealth and psychological effects.

Finally, we consider if other psychological mechanisms other than aspirations and expectations could account for the cash transfers' effects on economic outcomes. The cash transfers have near-zero effects on other psychological mechanisms that might account for more future-oriented behaviour, in particular time preferences and self-beliefs (Table 3, columns 3-7). They have a modest positive effect on mental health of 0.086 SD (column 8). This is similar in magnitude to effects of other cash transfer programmes on depression (Ridley et al., 2020). But it is modest relative to the effects of treating this clinical condition: meta-analyses of interventions to treat depression in poor countries find an average effect of 1.1 SD on depression (Cuijpers et al., 2018). The effect is driven by improvements in two items in the depression scale which capture more general wellbeing, whether participants felt afraid or unhappy, which are potentially less likely to drive investment behaviour. There is little effect on specific symptoms of mental ill health, such as whether participants had difficulty sleeping or lacked motivation or energy. We conclude these results are consistent with aspirations and expectations being the main psychological mechanism activated by the cash transfers.

[38] See Section 3 for a description of this 'gap' measure. The cash transfer increases this gap measure by 0.09 standard deviations (Table A.17).

6.5 Effects of the Combined Intervention on Psychological Mechanisms

The combined intervention has very similar effects to the cash transfer alone on the psychological mechanisms. Both interventions raise aspirations and expectations by similar margins of 0.13 – 0.18 standard deviations (Table 3, columns 1-2). We cannot reject equality of the cash and combined effects for aspirations ($p = 0.259$) or expectations ($p = 0.331$). In contrast, columns 3-8 show that both the cash and combined interventions have similar, near-zero effects on other psychological mechanisms.

The two experimental arms' similar effects on aspirations and expectations help to explain their similar effects on economic outcomes. We find that the workshop by itself substantially raises aspirations and expectations relative to the placebo. In our reference-dependent utility framework, this activates a causal chain from higher reference points, proxied by aspirations and expectations, to higher investment. When added to the cash transfer, the workshop does not activate this aspirations-investment chain, potentially because the cash transfer itself activates the chain. The treatment effects suggest that the workshop and cash transfer have strongly concave or substituteable effects on aspirations, so the aspiration increase may even plateau, seemingly crowding out the effect of the workshop in our results for the combined intervention.

The similar effects of the combined intervention and cash transfer alone do not arise simply because the workshop is less effective for households with more economic resources. To show this, we re-estimate our treatment effects model in equation (1) including a proxy for baseline resources and interactions between this proxy and the treatment indicators. The coefficients on the interaction terms are essentially zero for all economic outcomes and all three resource proxies we use: the value of all assets, assets excluding the (difficult to measure) value of land and housing, and annual consumption (Table A.18).[39] This shows that simply having more economic resources does not crowd out the effect of the workshop, although we cannot speak to the possible effects of the workshop for substantially wealthier people.[40] Instead, it appears that the windfall or unanticipated nature of the cash transfer might itself increase reference points more than an equivalent amount of existing economic resources. This is consistent with the fact that the cash transfers' effect on aspirations (0.06 standard deviations per 1000 USD PPP) is substantially larger than the the placebo group's association between aspirations and total household resources, with or without other control variables ($\leq$ 0.03 standard deviations per 1000 USD PPP).

[39] We obtain a similar result using spline models that allow more flexible heterogeneous treatment effects.

[40] One might worry that the cash transfer is so large that it is not informative to compare the workshop effects for households with more versus fewer baseline economic resources. However, there is substantial variation in baseline economic resources relative to the value of the cash transfer, even within this relatively poor sample. The cash transfer value corresponds to 0.18 standard deviations (SDs) of baseline assets, 1.5 SDs of baseline non-land-non-housing assets, and 0.84 SDs of baseline consumption.

We conclude that the treatment effects of all three interventions on economic outcomes most likely occur because the workshop and cash transfer both shift the same mechanism: reference points, proxied by aspirations or expectations. This pattern does not occur because the workshop has smaller effects on households with more economic resources. Our findings raise the possibility that cash transfers shift economic outcomes through both conventional wealth effects and psychological effects, and interact in important ways with other interventions that target aspirations directly.

7 Intervention Costs and Benefit-Cost Ratios

The workshop has substantial benefits when offered on its own. When combined with a large cash transfer, it has modest effects relative to the cash transfer alone. This section presents data on the costs of the intervention and the results of one method of comparing benefits to costs. We show that the workshop yields positive returns by the time of the endline on its own, but not when added to the cash transfer. We also discuss what conclusions can be drawn from comparing between the benefit-cost ratios of the psychological intervention and cash transfer.

Intervention Costs: We report actual intervention costs collected from our delivery of the workshop and GiveDirectly's delivery of the cash transfer in Table C.1 in the Online Supplement, calculated following J-PAL (2016) guidelines. We divide total costs by the number of people offered treatment, to be consistent with the intention-to-treat estimates we use to evaluate benefits, all in 2018 USD PPP.

The average variable cost of just workshop *delivery* was a relatively low 54 USD PPP, including staff time, travel and the tablets. This cost could be lowered even further by delivering the workshop as part of an existing field operation, such as activities of community health or agricultural extension workers.[41] We incurred additional costs running the workshop for our trial. The average variable *programme* cost was 218 USD PPP. This covered censusing participants to target the programme at poor households, as well as indirect costs for running a field operation. These costs could be reduced or avoided if the intervention were delivered as part of an existing programme or at larger scale. The average fixed cost of workshop *development* was 80 USD PPP per person offered the treatment. Delivering workshops to more households if the intervention were scaled would lower fixed costs per beneficiary. Adding together these fixed and variable costs, the workshop has an average *total* cost per participant of 353 USD PPP in our study.

Benefit-Cost Ratios: We compare intervention costs and benefits to evaluate if the aspirations workshop has a positive return. We define benefits as improvements in living standards,

[41] The results from the combined arm suggest that it is only useful to add the workshop to activities that are unlikely to shift aspirations or planning skills on their own.

proxied by higher household consumption, following common practice (e.g. Deaton 1997).[42] We construct a broad measure of consumption spending by combining four components, following similar principles to Bandiera et al. (2017) and Banerjee et al. (2015), who also study programmes targeted at poor households aimed at improving living standards. The four components are treatment effects on consumption expenditure, education expenditure (included in the education index in Table 2), land and housing expenditure, and the value of non-land non-housing assets.[43] The first three treatment effects capture increases in spending due to the interventions, which we assume cease at endline. We assume that the fourth treatment effect, on assets by the time of endline, is a proxy for the likely treatment effects of the intervention on future consumption, as the additional assets can be sold to fund future consumption. Results are reported in Panel B of Table C.1 in the Online Supplement.

Strikingly, there is a relatively high benefit-cost ratio from this modest one-session workshop. At the study's scale, the ratio of benefits to average *total* costs of the aspirations workshop is 96% i.e. it nearly pays for itself after 17 months, even when using the highest possible measure of intervention costs. The benefit-cost ratio using just the average variable cost of workshop *delivery* is a massive 627%. Our calculations assume limited post-endline persistence of effects on living standards, with future gains in consumption based only on current asset accumulation. This may understate benefits if behaviours like saving or labour supply shift permanently, but could overstate them if assets depreciate markedly or are appropriated. We cannot make precise quantitative claims about the time path of benefits without multiple rounds of follow-up surveys and a formal model of asset accumulation, use, and depreciation.

Using a similar approach, we can calculate the benefit-cost ratio of the cash and combined interventions. The combined intervention has marginally lower ratio of benefits to total costs (47%) than the cash transfer alone (56%), as the benefits are smaller while its costs are higher, although the difference in benefit-cost ratios is not statistically significant.[44] From a policy evaluation perspective, these results suggest there is little justification for adding the workshop to the cash transfer, at least over this 17-month horizon.[45] A recent meta-analysis similarly finds that the costs of adding "cash plus" programming such as training on financial literacy or business management are similar to, or higher than, the estimated benefits in terms of consumption, although this

[42] We discuss the relationship between gains in utility and gains in living standards in our reference-dependent utility framework in Appendix C.

[43] We scale the consumption and education effects from annual figures to cover the individual-specific period from the interventions to endline. We collect data directly on expenditure on land and housing for the period from the intervention to endline.

[44] We calculate the cost of the cash transfer excluding the cost of the placebo intervention. If the placebo intervention delivers any benefits, our approach attributes extra benefits to the cash transfer and hence understates the cost-effectiveness of the combined intervention relative to the cash transfer.

[45] The combined intervention has a slightly larger effect on education expenditure than the cash transfer alone, which may generate differences in their intergenerational benefits.

programming is typically more expensive than our workshop (Leight et al., 2024).

Finally, we can compare the benefit-cost ratios of the workshop and cash transfer, excluding the cost of the placebo workshop. The benefit-cost ratio of the cash transfer arm reflects the types of returns households could earn in this setting if given cash instead of the workshop and serves as a benchmark for evaluating the workshop as an anti-poverty intervention. We find the workshop delivers remarkably large gains in living standards relative to the cash transfer given its low cost, highlighting that the improvements in living standards delivered by the workshop are of an economically important magnitude. At the scale of this experiment, the benefits accumulated by endline are 56% of the total costs for the cash transfer, but 96% – nearly twice as high – for the workshop. The benefit-cost ratio using just the average variable cost of delivery is 62% for the cash transfer and 627% – ten times as high – for the workshop.[46]

However, there are caveats to the comparison. We cannot conduct a precise benchmarking exercise, where the benefits of a non-cash intervention are compared to a cash transfer of the same cost as the non-cash intervention (McIntosh & Zeitlin, 2022). The cash transfer costs about six times the workshop (2,149 USD PPP vs 353 USD PPP).[47] The comparison between cash and workshop is therefore plausible only if returns to cash transfers in terms of consumption are linear, i.e. the returns per dollar to our large transfer and a transfer corresponding to the cost of the workshop are the same. Existing research using randomised variation in transfer sizes shows this assumption is somewhat plausible. A meta-analysis of 44 lump sum unconditional cash transfer programmes by Crosta et al. (2024) tests for and finds no evidence of non-linearity of returns for both assets and consumption.[48]

8 Conclusion

We study how material and psychological constraints facing people living in poverty influence their investment decisions and living standards. We provide empirical evidence consistent with the idea that people living in poverty may lack opportunities to build their "capacity to aspire" (Appadurai, 2013): to set higher aspirations to improve their socioeconomic position and plan to achieve these goals, which in turn limits their investments and entrenches their poverty. The idea is important because psychological constraints could be one channel through which adverse historical conditions of discrimination and segregation persist and perpetuate contemporary poverty (Durlauf, 1996).

We provide experimental evidence that aspirational capacity can be easily built at low cost:

[46]The difference in total costs is not statistically significant (p=0.26) but the difference in variable costs is (p=0.02).

[47]This occurred for two reasons. First, the costs of the workshop were not known in advance as it had not been run at scale. Second, the specific cash transfer design was chosen to be combined with the workshop because it had meaningfully improved living standards in past trials (Egger et al., 2022; Haushofer & Shapiro, 2016) and we sought to test the effects of the workshop on its own and in a population whose living standards had meaningfully improved. This meant using a transfer of a similar size to previous interventions.

[48]This may differ for outcomes other than consumption e.g. school enrolment (Baird et al., 2011).

a short, scalable workshop targeted at aspirations leads participants to work and invest more, leading to higher revenue, asset wealth, and consumption. We show the workshop likely works by increasing participants' aspirations and expectations: the long-term goals they choose and aim for, and their beliefs about what future is possible. The workshop is highly cost effective: the average gain in consumption and assets in the 17 months from treatment to endline roughly equals the average total cost of the workshop. This estimate is based on the potentially conservative assumption that effects on all economic outcomes, including labour supply and asset accumulation, do not persist beyond endline. The workshop could have an even higher benefit-cost ratio if they do.

Our findings suggest that aspirations are a critical consideration in the design of poverty reduction interventions. There are potential market failures in the building of aspirational capacities. For example, there is no market where good role models can be compensated for the aspirational benefits they produce and institutions like schools or workplaces may not have incentives to develop these capacities. Further work could study how the effects of workshops like this might change at larger scales. This is a relevant question because the improvements in economic outcomes generated by the larger-scale implementation of the workshop might generate direct economic spillovers or might influence the aspirations of other people, as in some models of aspirations formation (Genicot & Ray, 2017). Similarly, future work might examine how workshops like this work in different contexts and whether changes persist over longer time horizons.

We also provide experimental evidence that improving economic conditions can themselves raise aspirations and expectations for the future. This is consistent with the idea that cash transfers can shift economic outcomes through both conventional wealth effects and psychological effects.

Our findings can also inform future work on psychological poverty traps. Recent work models 'aspirational poverty traps,' in which poverty lowers aspirations, which in turn lowers investment, and entrenches poverty (Dalton et al., 2016; Genicot & Ray, 2020; Lybbert & Wydick, 2018). Our results do not directly test these models but we do find evidence consistent with positive wealth-aspirations and aspirations-investment relationships. To the extent that some people face aspirational poverty traps, our results suggest that the low aspirations part of the trap might be addressed either through targeted interventions like our workshop or simply improving economic conditions.

References

Alan, S., T. Boneva & S. Ertac (2019) "Ever Failed, Try Again, Succeed Better: Results from a Randomized Educational Intervention on Grit," *The Quarterly Journal of Economics*, 134 (3), 1121–1162.

Alan, S. & S. Ertac (2018) "Fostering Patience in the Classroom: Results from a Randomized Educational Intervention," *Journal of Political Economy*, 126 (5), 1865–1911.

Andersen, S., G. Harrison, M. Lau & E. Rutström (2008) "Eliciting Risk and Time Preferences,"

Econometrica, 76 (3), 583–618.

Anderson, M. (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.

Andresen, E., J. Malmgren, W. Carter & D. Patrick (1994) "Screening for Depression in Well Older Adults: Evaluation of a Short Form of the CES-D," *American Journal of Preventive Medicine*, 10 (2), 77–84.

Angelucci, M. & D. Bennett (2021) "The Economic Impact of Depression Treatment in India," *Institute for the Study of Labour (IZA) Discussion Paper*, 14393.

Anzagi, S. & F. Bernard (1977) "Population Pressure in Rural Kenya," *Geoforum*, 8 (2), 63–68.

Appadurai, A. (2013) *The Future as a Cultural Fact: Essays on the Global Condition*, London, UK: Verso Books.

Arias, E. & S. De Vos (1996) "Using Housing Items to Indicate Socioeconomic Status: Latin America," *Social Indicators Research*, 38 (1), 53–80.

Ashraf, N., G. Bryan, A. Delfino, E. Holmes, L. Iacovone & A. Pople (2022) "Learning to See the World's Opportunities: The Impact of Imagery on Entrepreneurial Success," *London School of Economics & Political Science*, *Working Paper*.

Baird, S., C. McIntosh & B. Ozler (2011) "Cash or Condition? Evidence from a Cash Transfer Experiment," *The Quarterly Journal of Economics*, 126 (4), 1709–1753.

Bandiera, O., R. Burgess, N. Das, S. Gulesci, I. Rasul & M. Sulaiman (2017) "Labor Markets and Poverty in Village Economies," *The Quarterly Journal of Economics*, 132 (2), 811–870.

Bandura, A. (1977) *Social Learning Theory*, Englewood Cliffs, New Jersey: Prentice-Hall.

Banerjee, A., E. Duflo, N. Goldberg et al. (2015) "A Multifaceted Program Causes Lasting Progress for the Very Poor: Evidence from Six Countries," *Science*, 348 (6236), 12607991–126079916.

Baranov, V., S. Bhalotra, P. Biroli & J. Maselko (2020) "Maternal Depression, Women's Empowerment, and Parental Investment: Evidence from a Large Randomised Control Trial," *American Economic Review*, 10 (3), 824–859.

Barker, N., G. Bryan, D. Karlan, A. Ofori-Atta & C. Udry (2022) "Cognitive Behavioral Therapy Among Ghana's Rural Poor is Effective Regardless of Baseline Mental Distress," *American Economic Review: Insights*, 4 (4), 527–545.

Beaman, L., E. Duflo, R. Pande & P. Topalova (2012) "Female Leadership Raises Aspirations and Educational Attainment for Girls: A Policy Experiment in India," *Science*, 335 (6068), 582–586.

Beck, A. & B. Alford (2009) *Depression: Causes and Treatment*, Philadelphia: University of Pennsylvania Press.

Benabou, R. (1996) "Equity and Efficiency in Human Capital Investment: The Local Connection," *The Review of Economic Studies*, 63 (2), 237–264.

Benjamini, Y., A. Krieger & D. Yekutieli (2006) "Adaptive Linear Step-up Procedures that Control the False Discovery Rate," *Biometrika*, 93 (3), 491–507.

Bernard, T., S. Dercon, K. Orkin, G. Schinaia & A. S. Taffesse (2023) "The Future in Mind: Aspirations and Long-term Outcomes in Rural Ethiopia," *CEPR*, *Working Paper* (18492).

Bernard, T. & A. S. Taffesse (2014) "Aspirations: An Approach to Measurement with Validation Using Ethiopian Data," *Journal of African Economies*, 23 (2), 189–224.

Bhat, B., J. de Quidt, J. Haushofer, V. Patel, G. Rao, F. Schilbach & P. Vautrey (2022) "The Long-Run Effects of Psychotherapy on Depression, Beliefs, and Economic Outcomes," *National Bureau of Economic Research Working Paper*, 30011.

Blackwell, L., K. Trzesniewski & C. Dweck (2007) "Theories of Intelligence and Achievement Across the Junior High School Transition: A Longitudinal Study and an Intervention," *Child Development*, 78 (1), 246–63.

Blattman, C., J. Jamison & M. Sheridan (2017) "Reducing Crime and Violence: Experimental Evidence from Cognitive Behavioural Therapy in Liberia," *American Economic Review*, 107 (4), 1165–1206.

Bong, M. & E. Skaalvik (2003) "Academic Self-Concept and Self-Efficacy: How Different Are They Really?," *Educational Psychology Review*, 15 (1), 1–40.

Caliendo, M., D. Cobb-Clark & A. Uhlendorff (2015) "Locus of Control and Job Search Strategies," *The Review of Economics and Statistics*, 97 (1), 88–103.

Campos, F., M. Frese, M. Goldstein, L. Iacovone, H. Johnson, D. McKenzie & M. Mensmann (2017) "Teaching Personal Initiative Beats Traditional Training in Boosting Small Business in West Africa," *Science*, 357 (6357), 1287–1290.

Carlana, M., E. La Ferrara & P. Pinotti (2022) "Goals and Gaps: Educational Careers of Immigrant Children," *Econometrica*, 90 (1), 1–29.

Carrell, S., M. Page & J. West (2010) "Sex and Science: How Professor Gender Perpetuates the Gender Gap," *The Quarterly Journal of Economics*, 125, 1101–1144.

Cecchi, F., A. Garcia, R. Lensink & B. Wydick (2022) "Aspirational Hope, Dairy Farming Practices, and Milk Production: Evidence from a Randomized Controlled Trial in Bolivia," *World Development*, 160, 106087.

Chattopadhyay, R. & E. Duflo (2004) "Women as Policy Makers: Evidence from a Randomized Policy Experiment in India," *Econometrica*, 72, 1409 – 1443.

Chiapa, C., J. Garrido & S. Prina (2012) "The Effect of Social Programmes and Exposure to Professionals on the Educational Aspirations of the Poor," *Economics of Education Review*, 31 (5), 778 – 798.

Conroy, D. & M. Hagger (2018) "Imagery Interventions in Health Behaviour: A Meta-Analysis," *Health Psychology*, 37 (7), 668–679.

Contreras Suarez, D. & L. Cameron (2020) "Conditional Cash Transfers: Do They Result in More Patient Choices and Increased Educational Aspirations?" *Economic Development and Cultural Change*, 68 (3), 729–761.

Cross, A. & D. Sheffield (2019) "Mental Contrasting for Health Hehaviour Change: A Systematic Review and Meta-analysis of Effects and Moderator Variables," *Health Psychology Review*, 13 (2), 209–225.

Crosta, T., D. Karlan, F. Ong, J. Rüschenpöhler & C. R. Udry (2024) "Unconditional Cash Transfers: A Bayesian Meta-Analysis of Randomized Evaluations in Low and Middle Income Countries," *National Bureau of Economic Research Working Paper*, 32779.

Cubitt, R. & D. Read (2007) "Can Intertemporal Choice Experiments Elicit Time Preferences for Consumption?," *Experimental Economics*, 10 (4), 369–389.

Cuijpers, P., E. Karyotaki, M. Reijnders, M. Purgato & C. Barbui (2018) "Psychotherapies for Depression in Low- and Middle-Income Countries: A Meta-Analysis," *World Psychiatry*, 17 (1), 90–101.

Dalton, P., S. Ghosal & A. Mani (2016) "Poverty and Aspirations Failure," *Economic Journal*, 126 (590), 165–188.

Dalton, P., J. Ruschenpohler & B. Zia (2018) "Determinants and Dynamics of Business Aspirations: Evidence from Small-Scale Entrepreneurs in an Emerging Market," *World Bank Policy Research Working Paper*, 8400.

Deaton, A. (1997) *The Analysis of Household Surveys: A Microeconometric Approach to Development Policy*, Baltimore: Johns Hopkins University Press.

Deaton, A. & S. Zaidi (2002) *Guidelines for Constructing Consumption Aggregates for Welfare Analysis*, 135: World Bank Publications.

Delavande, A. (2022) "Expectations in Development Economics," in *Handbook of Economic Expectations*: Elsevier.

Dominitz, J. & C. Manski (1997) "Using Expectations Data to Study Subjective Income Expectations," *Journal of the American Statistical Association*, 92 (439), 855–867.

Duckworth, A., T. Kirby, A. Gollwitzer & G. Oettingen (2013) "From Fantasy to Action Mental Contrasting With Implementation Intentions (MCII) Improves Academic Performance in Children," *Social Psychological and Personality Science*, 4 (6), 745–753.

Duckworth, A. L. & P. D. Quinn (2009) "Development and Validation of the Short Grit Scale (GRIT–S)," *Journal of Personality Assessment*, 91 (2), 166–174.

Durlauf, S. (1996) "A Theory of Persistent Income Inequality," *Journal of Economic Growth*, 1 (1), 75–93.

Dweck, C. (2012) "Mindsets and Human Nature: Promoting Change in the Middle East, the Schoolyard, the Racial Divide, and Willpower," *American Psychologist*, 67 (8), 614–622.

Eble, A. & M. Escueta (2022) "When Bootstraps Aren't Enough: Demand, Supply, and Learning in a Very Low-Income Context," *Annenberg Institute at Brown University*, *Working Paper*.

Eckel, C. & P. Grossman (2002) "Sex Differences and Statistical Stereotyping in Attitudes Toward Financial Risk," *Evolution and Human Behaviour*, 23 (4), 281–295.

EEF (2021) "Aspirations Interventions. Teaching and Learning Toolkit," https://educationendowmentfoundation.org.uk/education-evidence/teaching-learning-toolkit/aspiration-interventions, Online; accessed 7 September 2022.

Egger, D., J. Haushofer, E. Miguel, P. Niehaus & M. Walker (2022) "General Equilibrium Effects of Cash Transfers: Experimental Evidence from Kenya," *Econometrica*, 90 (6), 2603–2643.

Fairlie, R., F. Hoffmann & P. Oreopoulos (2014) "A Community College Instructor Like Me: Race and Ethnicity Interactions in the Classroom," *American Economic Review*, 104 (8), 2567–2591.

Falk, A., A. Becker, T. Dohmen, B. Enke, D. Huffman & U. Sunde (2018) "Global Evidence on Economic Preferences," *The Quarterly Journal of Economics*, 133 (4), 1645–1692.

FAO (1996) *A System of Economic Accounts for Food and Agriculture*, Rome: Food and Agriculture Organization.

Frederick, S., G. Loewenstein & T. O'Donoghue (2002) "Time Discounting and Preference : A Critical Review," *Journal of Economic Literature*, 40, 351–401.

García, S., A. Harker & J. Cuartas (2019) "Building Dreams: The Short-term Impacts of a Conditional Cash Transfer Program on Aspirations for Higher Education," *International Journal of Educational Development*, 64, 48–57.

Garlick, R., G. Nyeki & K. Orkin (2022) "Evaluating Multidimensional Programs in The Presence of Endogenous, Multidimensional Networks," *Duke University*, *Working Paper*.

Genicot, G. & D. Ray (2017) "Aspirations and Inequality," *Econometrica*, 85 (2), 489–519.

——— (2020) "Aspirations and Economic Behavior," *Annual Review of Economics*, 12, 715–746.

Ghosal, S., S. Jana, A. Mani, S. Mitra & S. Roy (2020) "Sex Workers, Self-Image and Stigma: Evidence from Kolkata Brothels," *The Review of Economics and Statistics*, 104 (3), 431–448.

Glennester, R., M. Kremer, I. Mbiti & K. Takavarasha (2011) "Access and Quality in the Kenyan Education System: A Review of the Progress, Challenges and Potential Solutions," Report Prepared for Office of the Prime Minister of Kenya.

Goerg, S. (2015) "Goal Setting and Worker Motivation," *IZA World of Labor*, 178.

Gollwitzer, P. M. & P. Sheeran (2006) "Implementation Intentions and Goal Achievement: A Meta-analysis of Effects and Processes," *Advances in Experimental Social Psychology*, 38, 69–119.

Grosh, M. & P. Glewwe (2000) *Designing Household Survey Questionnaires for Developing Countries: Lessons from 15 Years of the Living Standards Measurement Study*, Washington, D.C.: World Bank.

Gugerty, M. (2007) "You Can't Save Alone: Commitment in Rotating Savings and Credit Associations in Kenya," *Economic Development and Cultural Change*, 55 (2), 251–282.

Guyon, N. & E. Huillery (2021) "Biased Aspirations and Social Inequality at School: Evidence from French Teenagers," *The Economic Journal*, 131 (634), 745–796.

Hanna, R., S. Mullainathan & J. Schwartzstein (2014) "Learning Through Noticing: Theory and Evidence from a Field Experiment," *The Quarterly Journal of Economics*, 129 (3), 1311–1353.

Haushofer, J. & J. Shapiro (2016) "The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya," *The Quarterly Journal of Economics*, 131 (4), 1973–2042.

Heckman, J. & T. Kautz (2012) "Hard Evidence on Soft Skills," *Labour Economics*, 19 (4), 451–464.

J-PAL (2016) "J-PAL Costing Guidelines," https://www.povertyactionlab.org/sites/default/files/research-resources/costing-guidelines.pdf, Online; accessed 7 September 2022.

Janzen, S., N. Magnan, S. Sharma & W. Thompson (2017) "Aspirations Failure and Formation in Rural Nepal," *Journal of Economic Behavior and Organization*, 139, 1–25.

Jensen, R. & E. Oster (2009) "The Power of TV: Cable Television and Women's Status in India," *The Quarterly Journal of Economics*, 124 (3), 1057–1094.

John, A. & K. Orkin (2022) "Can Simple Psychological Interventions Increase Preventive Health Investment?" *Journal of the European Economic Association*, 20 (3), 1001–1047.

Kearney, M. & P. Levine (2020) "Role Models, Mentors, and Media Influences," *The Future of Children*, 30 (1), 83–106.

Kenya Institute for Curriculum Development (2017) "Basic Education Curriculum Framework," https://kicd.ac.ke/wp-content/uploads/2017/10/CURRICULUMFRAMEWORK.pdf, Online; accessed 7 September 2022.

Kenya National Social Protection Secretariat (2022) "National Safety Net Program," https://www.socialprotection.or.ke/social-protection-components/social-assistance/national-safety-net-program, Online; accessed 7 September 2022.

King, L. (2001) "The Health Benefits of Writing About Life Goals," *Personality and Social Psychology Bulletin*, 27 (7), 798–807.

Kofoed, M. & E. McGovney (2019) "The Effect of Same-Gender and Same-Race Role Models on Occupation Choice: Evidence from Randomly Assigned Mentors at West Point," *Journal of Human Resources*, 54, 430–467.

Kőszegi, B. & M. Rabin (2006) "A Model of Reference-Dependent Preferences," *The Quarterly Journal of Economics*, 121 (4), 1133–1165.

——— (2007) "Reference-Dependent Risk Attitudes," *American Economic Review*, 97 (4), 1047–1073.

Kwasnicka, D., J. Presseau, M. White & F. Sniehotta (2013) "Does Planning How to Cope with Anticipated Barriers Facilitate Health-Related Behaviour Change? A Systematic Review," *Health Psychology Review*, 7 (2), 129–145.

La Ferrara, E. (2016) "Mass Media and Social Change: Can We Use Television to Fight Poverty?" *Journal of the European Economic Association*, 14 (4), 791–827.

——— (2019) "Presidential Address: Aspirations, Social Norms, and Development," *Journal of the European Economic Association*, 17 (6), 1687–1722.

Laajaj, R. & K. Macours (2021) "Measuring Skills in Developing Countries," *Journal of Human Resources*, 56 (4), 1254–1295.

Leight, J., K. Hirvonen & S. Zafar (2024) "The Effectiveness of Cash and Cash Plus Interventions on Livelihoods Outcomes: Evidence from a Systematic Review and Meta-Analysis," *International Food Policy Research Institute, Working Paper, 02262*.

Levenson, H. (1981) "Differentiating Among Internality, Powerful Others, and Chance," in Lefcourt, H. M. ed. *Research with the Locus of Control Construct Vol.1*, 15–63, New York: Academic Press.

Lindqvist, E. & R. Vestman (2011) "The Labour Market Returns to Cognitive and Non-Cognitive Ability: Evidence from the Swedish Enlistment," *American Economic Journal: Applied Economics*, 3 (January), 101–128.

Liu, E. (2013) "Time to Change What to Sow: Risk Preferences and Technology Adoption Decisions of Cotton Farmers in China," *The Review of Economics and Statistics*, 95 (4), 1386–1403.

Locke, E. A. & G. P. Latham (2002) "Building a Practically Useful Theory of Goal Setting and Task Motivation: A 35 Year Odyssey," *American Psychologist*, 57 (9), 705–717.

Lockwood, P. & Z. Kunda (1997) "Superstars and Me: Predicting the Impact of Role Models on the Self," *Journal of Personality and Social Psychology*, 73 (1), 91–103.

Loveday, P., G. Lovell & C. Jones (2018) "The Best Possible Selves Intervention: A Review of the Literature to Evaluate Efficacy and Guide Future Research," *Journal of Happiness Studies*, 19 (2), 607–628.

Lucas, A. & I. Mbiti (2012) "Access, Sorting and Achievement: The Short-Run Effects of Free Primary Education in Kenya," *American Economic Journal: Applied Economics*, 4 (4), 226–253.

Lund, C., K. Orkin, M. Witte et al. (2022) "Treating Mental Health Conditions Improves Labor Market and Other Economic Outcomes in Low and Middle-Income Countries," *University of Oxford, Working Paper*.

Lybbert, T. & B. Wydick (2018) "Poverty, Aspirations and the Economics of Hope," *Economic Development and Cultural Change*, 66 (4), 709–753.

Macours, K. & R. Vakis (2014) "Changing Households' Investment Behaviour Through Social Interactions with Local Leaders: Evidence from a Randomised Transfer Programme," *Economic Journal*, 124 (576), 607–633.

Mahmud, M., K. Orkin & E. Riley (2022) "Economic and Psychological Constraints to Women's Empowerment," *University of Oxford, Working Paper*.

Marx, D. M. & S. J. Ko (2012) "Superstars "Like" Me: The Effect of Role Model Similarity on Performance Under Threat," *European Journal of Social Psychology*, 42, 807–812.

McIntosh, C. & A. Zeitlin (2022) "Using Household Grants to Benchmark the Cost Effectiveness of a USAID Workforce Readiness Program," *Journal of Development Economics*, 157, 102875.

McKelway, M. (2021) "Women's Employment in India: Intra-Household and Intra-Personal Constraints," *Dartmouth, Working Paper*.

McKenzie, D., A. Mohpal & D. Yang (2022) "Aspirations and Financial Decisions: Experimental Evidence from the Philippines," *Journal of Development Economics*, 156, 102846.

Michie, S., M. Richardson, M. Johnston et al. (2013) "The Behavior Change Technique Taxonomy of 93 Hierarchically Clustered Techniques: Building an International Consensus for the Reporting of Behavior Change Interventions," *Annals of Behavioral Medicine*, 46 (1), 81–95.

Morisano, D., J. Hirsh, J. Peterson, R. Pihl & B. Shore (2010) "Setting, Elaborating, and Reflecting on Personal Goals Improves Academic Performance," *Journal of Applied Psychology*, 95 (2), 255–264.

Oettingen, G. & P. Gollwitzer (2010) "Strategies of Setting and Implementing Goals," in Maddux, J. & J. Tangney eds. *Social Psychological Foundations of Clinical Psychology*, 114–135, New York: The Guilford Press.

Ozier, O. (2018) "The Impact of Secondary Schooling in Kenya: A Regression Discontinuity Analysis," *Journal of Human Resources*, 53 (1), 157–188.

Porter, C. & D. Serra (2020) "Gender Differences in the Choice of Major: The Importance of Female Role Models," *American Economic Journal: Applied Economics*, 12 (3), 226–54.

Potash, B. (1978) "Some Aspects of Marital Stability in a Rural Luo Community," *Africa*, 48 (4), 380–397.

Rapport, N. (2016) "Aspiration as Universal Human Capacity," in Oscar, O. H. & T. Tufte eds. *Voice and Matter: Communication, Development and the Cultural Return*, 217–225, Gothenburg: Nordicom.

Ridley, M., G. Rao, F. Schilbach & V. Patel (2020) "Poverty, Depression, and Anxiety: Causal Evidence and Mechanisms," *Science*, 370 (6522), eaay0214.

Riley, E. (2019) "Role Models in Movies: the Impact of Queen of Katwe on Students' Educational Attainment," *The Review of Economics and Statistics*, 1–48.

Rojas Valdes, R., B. Wydick & T. Lybbert (2022) "Can Hope Elevate Microfinance? Evidence from Oaxaca, Mexico," *Oxford Economic Papers*, 74 (1), 236–264.

Ross, P. (2019) "Occupation Aspirations, Education Investment, and Cognitive Outcomes: Evidence from Indian Adolescents," *World Development*, 123, 104613.

Ross, P., P. Glewwe, D. Prudencio & B. Wydick (2021) "Developing Educational and Vocational Aspirations Through International Child Sponsorship: Evidence from Kenya, Indonesia, and Mexico," *World Development*, 140, 105336.

Rotter, J. (1966) "Generalised Expectancies for Internal Versus External Control of Reinforcement," *Psychological Monographs: General and Applied*, 80 (1), 1–28.

Schennach, S. (2022) "Measurement Systems," *Journal of Economic Literature*, 60 (4), 1223–63.

Schwarzer, R. & M. Jerusalem (1995) *Generalised Self-Efficacy Scale*: Windsor, UK: NFER-NELSON.

Sifuna, P., L. Otieno, B. Andagalu et al. (2018) "A Spatiotemporal Analysis of HIV-associated Mortality in Rural Western Kenya 2011–2015," *Journal of Acquired Immune Deficiency Syndromes*, 78 (5), 483.

US Department of Education (2007) "Encouraging Girls in Math and Science," https://ies.ed.gov/ncee/wwc/PracticeGuide/5, Online; accessed 7 September 2022.

Wager, S. & S. Athey (2018) "Estimation and Inference of Heterogeneous Treatment Effects using Random Forests," *Journal of the American Statistical Association*, 113 (523), 1228–1242.

Willis, P. (1977) *Learning to Labour. How Working Class Kids Get Working Class Jobs*, New York: Columbia University Press.

Wilson, W. J. (1987) *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy*, Chicago: University of Chicago Press.

Yeager, D. & C. Dweck (2020) "What Can be Learned from Growth Mindset Controversies?" *American Psychologist*, 75 (9), 1269–1284.

Appendices for Online Publication Only

Contents

A Additional Results **45**

A.1 Associations Between Aspirations, Expectations and Other Measures 45

A.2 Additional Treatment Effects 51

B Data Collection, Sampling, Treatment Assignment & Receipt **65**

B.1 Sources of Data and Sample Eligibility 65

B.2 Treatment Assignment and Treatment Receipt 66

C Conceptual Framework Appendix **71**

A Additional Results

A.1 Associations Between Aspirations, Expectations and Other Measures

This appendix shows additional results cited in Section 3 of the paper. Table A.1 shows the types of goals that respondents report holding. Tables A.2 and A.3 show that aspirations and expectations are closely related to each other and are weakly related to all other psychological measures we collect.

The next set of figures show show that the relationships between aspirations, wealth, and investment from Section 3.2 are robust to:

- Removing all controls, removing all psychological characteristics except aspirations, or adding respondent fixed effects using the panel structure (Figure A.1).
- Using baseline data instead of endline placebo group data, although the baseline data does not capture time and risk preferences (Figure A.2).
- Replacing aspirations with the difference between aspirations and respondents' self-reports about their current assets, income, and education (Figure A.3). This shows that the positive aspirations-investment association does not simply occur because people with higher wealth, or higher perceived wealth, have higher aspirations. We do not run this analysis for the aspirations-wealth relationships, because then wealth would mechanically appear on both the left-hand side of the regression and the right-hand side, via the differencing. This would produce artificially negative relationships.

Furthermore, the relationships are similar when we control for cognitive ability using a subsample of data. We only collect cognitive ability for a subsample to use as a robustness check because

it is unlikely to respond to a short workshop with adults (Heckman & Kautz, 2012). We find that cognitive ability is positively associated with wealth and investment, echoing the finding from Laajaj & Macours (2021) in the same context.

We show in the Online Supplement that the relationships between expectations, investment, and wealth are very similar to the relationships between aspirations, investment, and wealth. This is unsurprising, given the close empirical relationship between aspirations and expectations.

Table A.1: Proportion of Respondents Reporting Goals by Domain

	(1)	(2)
Keywords	Workshop	Combined
Productive assets	0.96	0.93
Chicken, chick, hen, poultry	0.52	0.48
Goat	0.03	0.03
Sheep	0.01	0.02
Cow, cattle	0.34	0.32
Farming infrastructure[(a)]	0.08	0.07
Income		
Small business goals[(b)]	0.36	0.37
Income-generating farming goals	0.78	0.80
Tailoring goal	0.03	0.04
Savings and credit	0.00	0.01
Savings, save, ROSCA	0.00	0.01
Credit, loan	0.00	0.00
Housing[(c)]	0.87	0.85
Education	0.51	0.50
Primary and secondary schooling[(d)]	0.50	0.49
Tertiary education[(e)]	0.06	0.06

Notes: Bolded row titles represent indicators for any subset of groupings of the reference categories beneath them. Proportions do not necessarily sum within reference categories, as often respondents will nominate more than one of each of the members of a category. Proportions do not sum to 100 as participants set more than one goal. We code the open-ended goals recorded by fieldworkers to identify common domains or activities in which participants listed goals using STATA's regular expression function, regexm, to filter for a given string. We define a dummy = 1 if a participant has a goal which contains the string. [(a)] is made up of the keywords hoe, irrigation, fence, mill, tractor, greenhouse, tank, plot. [(b)] is made up of retail, trade, business, tailor, tailoring, shop, sell, fish, rent, boda, motorbike cereal, omena, mala, commercial. [(c)] is made up of house, home, roof, walls, floor, tin, simba, kitchen, room. [(d)] is made up of school, educat, educate, education, schooling and any keywords in [(e)]. [(e)] is made up of university, college, diploma, tertiary.

Table A.2: Pairwise Correlations of Psychological Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AI	EI	DF	NPB	RTI	S-bI	RI	MHZ
Aspirations Index	1.00	0.60	-0.05	0.02	0.02	0.07	0.04	0.10
Expectations Index	0.60	1.00	-0.05	0.01	0.03	0.04	0.01	0.14
Discount Factor	-0.05	-0.05	1.00	0.26	-0.00	-0.02	-0.02	-0.04
No Present bias	0.02	0.01	0.26	1.00	0.01	-0.01	0.01	-0.00
Risk Taking Index	0.02	0.03	-0.00	0.01	1.00	0.02	0.03	-0.00
Self-belief Index	0.07	0.04	-0.02	-0.01	0.02	1.00	-0.07	0.09
Returns Index	0.04	0.01	-0.02	0.01	0.03	-0.07	1.00	0.02
Mental Health Z-score	0.10	0.14	-0.04	-0.00	-0.00	0.09	0.02	1.00

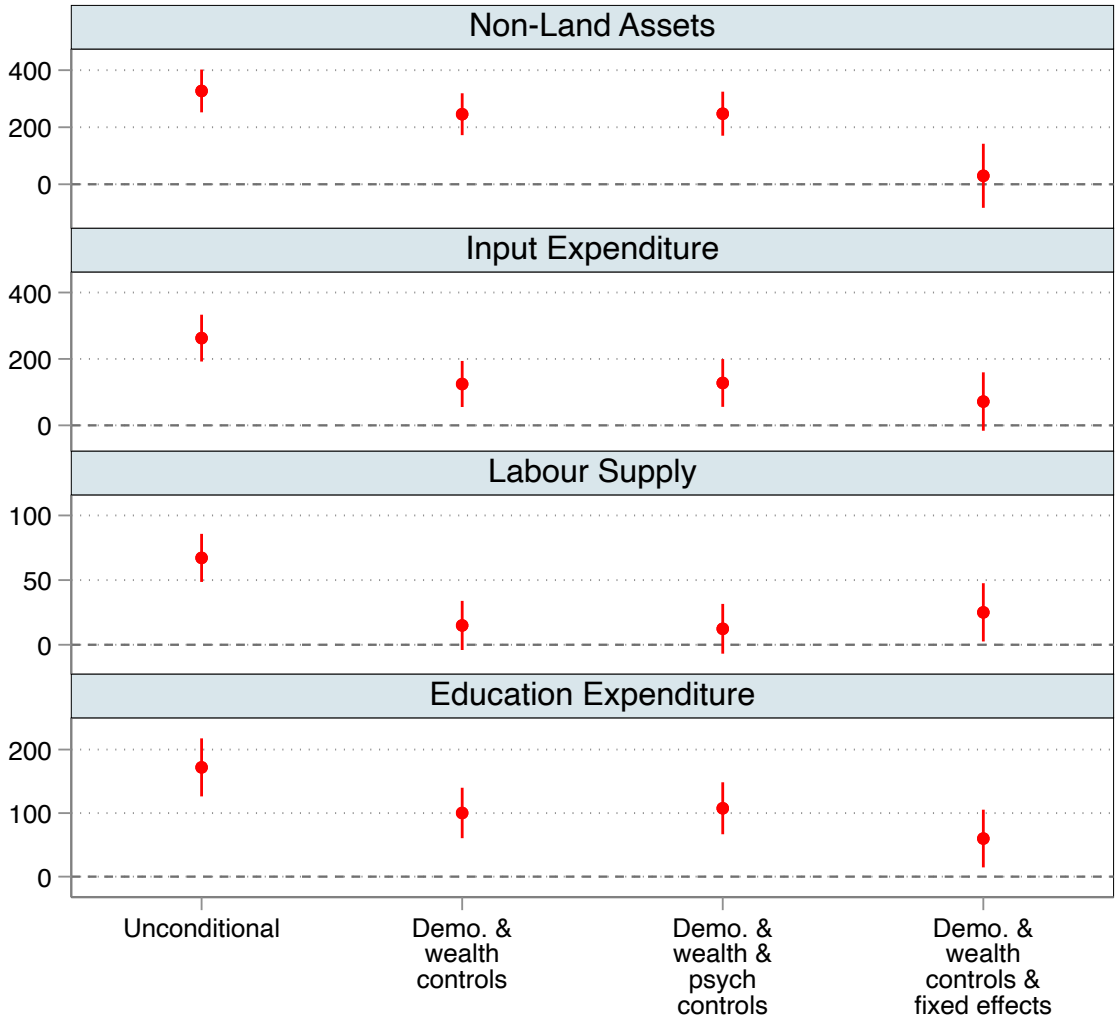
Notes: This table shows pairwise correlations between psychological outcomes. All measures are defined in Section 3.1.

Table A.3: Relationship Between Aspirations, Expectations, and Other Psychological Outcomes

	(1)	(2)	(3)	(4)
	Asp index OLS	Asp index LASSO	Exp index OLS	Exp index LASSO
Discount factor	-0.125		-0.121	
	(0.12)		(0.12)	
No present bias	0.052		0.030	
	(0.34)		(0.57)	
Risk-taking	0.012		0.022	
	(0.63)		(0.36)	
Self-belief	0.062***		0.017	
	(0.01)		(0.45)	
Beliefs about returns	0.036		0.007	
	(0.20)		(0.79)	
Mental health Z-score	0.086***	0.013	0.141***	0.063
	(0.00)		(0.00)	
Observations	1716	1716	1717	1717
R^2	0.02	0.00	0.02	0.01

This table shows the relationship between the aspirations and expectations indices and other psychological measures defined in Figure 1 in the placebo group at endline. Odd columns are the estimates from OLS regressions of the aspirations index (1) and expectations index (3) on the other psychological mechanisms. Even columns are the estimates from LASSO regressions of the Aspirations index (2) and Expectations index (4) on the other psychological mechanisms, with the lamba parameter chosen using cross validation. Heterosekdasticity-robust standard errors shown in parentheses for the OLS estimates. No standard errors shown for the LASSO estimates because post-model-selection inference is not valid. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Figure A.1: Relationships Between Wealth, Investment, and Aspirations Using Different Control Variables

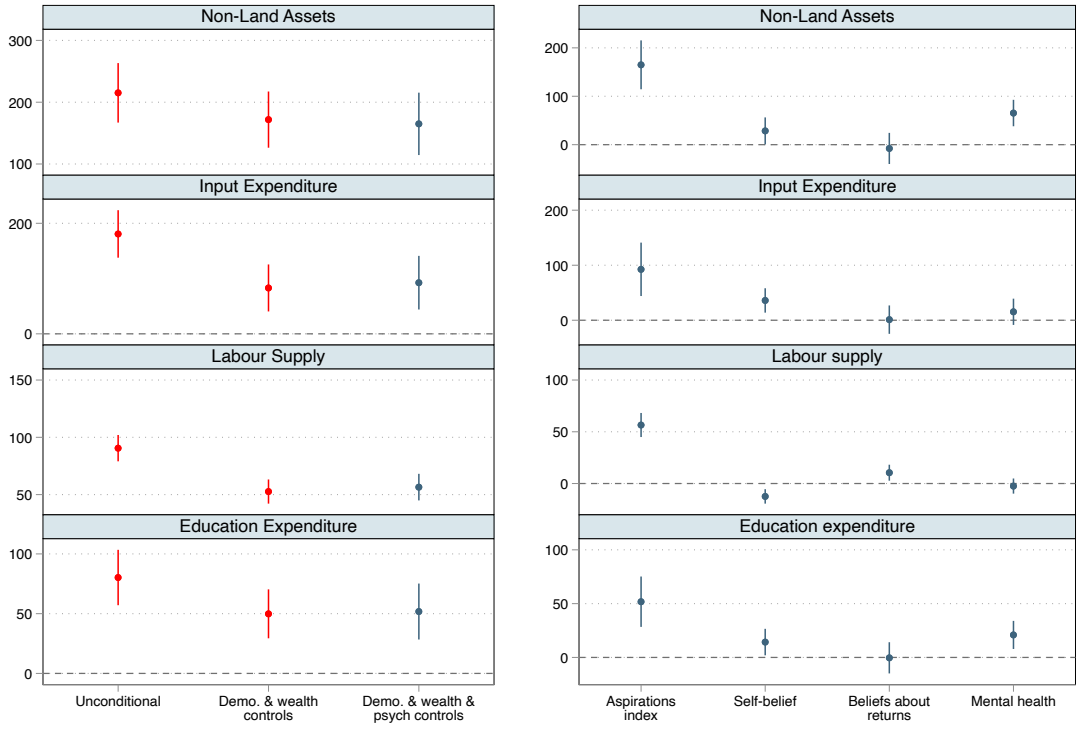


Notes: This figure shows that the wealth-aspirations and investment-aspirations relationships from Section 3.2 and Figure 1 are robust to using different control variables.

The four vertically stacked rows show coefficients and 95% confidence intervals from regressing a wealth proxy (non-land assets) and three investment measures (input expenditure, labour supply and education expenditure) on the standardised aspirations index and other variables. Within each row, the first column shows the coefficients on the aspirations index from bivariate regressions; the second column shows the coefficients on the aspirations index controlling for respondent age, education, marital status, household size, number of school-aged members, geography fixed effects and (except for the top row) asset value and consumption; the third column shows the coefficients on the aspirations index controlling for the same variables and the psychological characteristics shown in Figure 1; and the fourth column shows the coefficients on the aspirations index controlling for the same variables as in the second column, plus respondent fixed effects.

All asset and investment measures are defined in the footnote below Table 1. The aspirations index and psychological measures are defined in Section 3.1. All regressions use the endline placebo group data with the top percentiles of expectations, investment, assets, and consumption trimmed. Sample size is 1376 to 1747 depending on the choice of controls and investment measure. The smaller sample sizes are for education expenditure, as this is set to missing for households with no school-aged children. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

Figure A.2: Relationships Between Wealth, Investment, Aspirations, and Psychological Characteristics At Baseline Using Different Control Variables

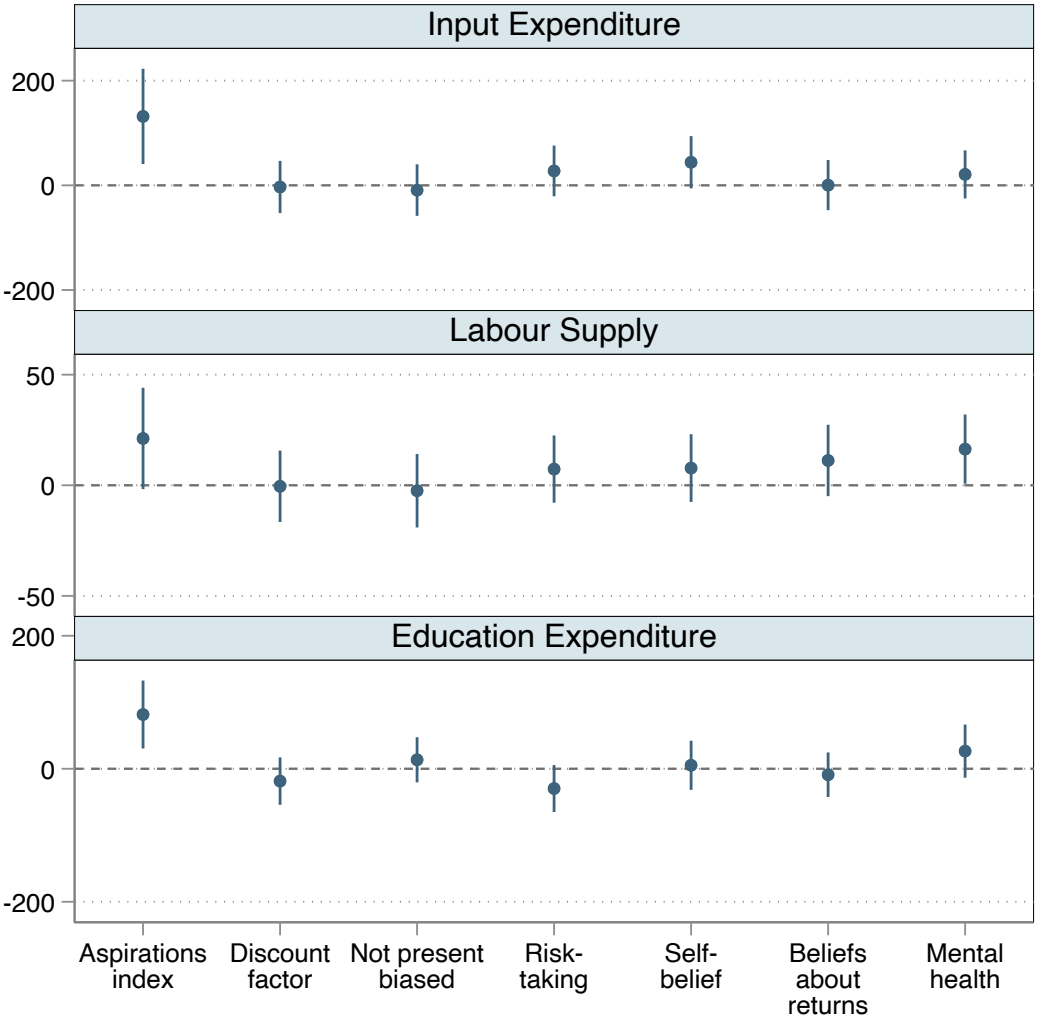


Notes: This figure shows that the wealth-aspirations and investment-aspirations relationships from Section 3.2 are very similar when we use baseline data instead of endline placebo group data. The right panel replicates Figure 1 and the left panel replicates Figure A.1. This analysis omits time and risk preferences because they were not measured at baseline. Beliefs about returns is the standardised measure of beliefs about returns to fertiliser, the only belief about returns measured at baseline. Compared to Figures 1 and A.1, no fixed effects specification is reported as the relationship reports on cross-sectional data at baseline.

The four vertically stacked rows in the left panel show coefficients and 95% confidence intervals from regressing different wealth and investment measures on psychological characteristics at baseline. The top row on the left shows results from regressing a wealth proxy (non-land assets) and the second to fourth rows show results from regressing three investment measures (input expenditure, labour supply and education expenditure). Within each left-hand row, the first column shows the coefficients on the aspirations index from bivariate regressions; the second column shows the coefficients on the aspirations index controlling for respondent age, education, marital status, household size, number of school-aged members, geography fixed effects and (except for the top row) asset value and consumption; and the third column shows the coefficients on the aspirations index controlling for the same variables and the psychological characteristics shown in the right-hand panel. The right panel shows coefficients on each of the psychological characteristics.

All regressions use the full baseline sample with the top percentiles of aspirations, investment, assets, and consumption trimmed. All asset and investment measures are defined in the footnote below Table 1. The psychological measures are defined in Section 3.1. Here, each is standardised to allow for coefficient comparison. Sample size is 5731 to 8175 depending on the choice of controls and investment measure. The smaller sample sizes are for education expenditure, as this is set to missing for households with no school-aged children. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

Figure A.3: Relationships Between Investment, (Aspirations - Perceived Current Economic Position), and Other Psychological Characteristics



Notes: This figure shows that the investment-aspirations relationships from Section 3.2 and Figure 1 are robust to replacing the aspirations index with the aspirations index minus respondents' beliefs about their current economic position. This figure does not show relationships between this "gap" measure and wealth proxies, because the goal of this analysis is to show specifically that the investment-aspirations relationships are not driven by beliefs about current assets, income, or education.

The three vertically stacked rows show coefficients and 95% confidence intervals from regressing three investment measures (expenditure on productive inputs and hired labour, labour supply, and education expenditure) on psychological characteristics. All regressions control for respondent age, education, marital status, household size, number of school-aged members, geography fixed effects and (except for the top row) asset value and consumption.

All asset and investment measures are defined in the footnote below Table 1. All psychological measures are defined in Section 3.1. All regressions use the endline placebo group data with the top percentiles of aspirations, investment, assets, and consumption trimmed. Sample size is 1364 to 1745 depending on the choice of controls and investment measure. The smaller sample sizes are for education expenditure, as this is set to missing for households with no school-aged children. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

A.2 Additional Treatment Effects

As we prespecified, we adjust for multiple hypothesis testing over tests that use components of the same prespecified aggregate or concept. This applies to input expenditure and hired labour in Table A.5, the four components of non-land assets in Table A.6, the two components of age-group-specific education expenditure in Table A.7, the three aspirations measures in Table A.9, the three self-beliefs measures in Table A.10, and the beliefs about returns to specific inputs in Table A.12. We also adjust for multiple hypothesis testing across the six economic aggregates in each of Tables A.15, and A.16, and A.18 to be consistent with Table 2. We do not adjust across all measures in a table if they are not components of the same prespecified aggregate.

Table A.4: Treatment Effects of Total Labour Supply by Household Member Type

	(1)	(2)	(3)	(4)
	Adults	Respondent	Non-respondent Adults	Non-adults
Workshop	26.8**	14.0*	13.0	-0.29
	(11.6)	(7.1)	(8.7)	(6.11)
Cash	27.2**	14.4*	12.4	1.20
	(12.4)	(7.8)	(8.1)	(5.65)
Combined	9.0	10.0	-2.3	-8.15
	(11.5)	(7.5)	(8.4)	(5.78)
P: cash = workshop	0.972	0.956	0.943	0.807
P: cash = combined	0.127	0.564	0.071	0.100
P: workshop = combined	0.118	0.574	0.100	0.206
P: cash + workshop = combined	0.007	0.078	0.023	0.277
Placebo mean	525	315	216	76.9
# clusters	413	413	413	413
# obs	7,240	7,121	7,240	7,240

Notes: All variables are at the household level, measured in days and scaled to annual figures. Aggregate labour supply is defined as the total days of labour supplied to: a) household farm activities which consist of agricultural and livestock activities, b) household non-farm enterprises and c) casual and salaried employment outside the household. The aggregate labour supply of the household can be subdivided into different categories. Column (1) reports the aggregate labour supply of all adult household members. Columns (2), (3) and (4) report the aggregate labour supply of the respondent, other non-respondent adults in the household and non-adult members of the household respectively. Adults are defined as household members aged older than 15. For the endlined sample, the mean numbers of adult members (including the respondent) is 2.8 and the mean number of non-adult members is 2.8. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.5: Treatment Effects on Inputs to and Outputs from Farm and Non-farm Activities

	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Input Expenditure	Hired Labour	Technology Adoption	Labour Supply (Days)	Returns to Factors
Panel A: Farm (Agriculture and Livestock)						
Workshop	16.2	9.8	-0.6	0.088	15.7	7.0
	(47.6)	(9.3)	(15.1)	(0.103)	(9.8)	(18.2)
	[.]	[1.000]	[1.000]	[.]	[.]	
Cash	63.1	83.3***	27.8	0.209**	19.9*	-17.8
	(48.3)	(10.3)	(19.4)	(0.095)	(10.7)	(19.3)
	[.]	[0.001]	[0.083]	[.]	[.]	
Combined	20.0	93.4***	63.3***	0.152	2.9	-16.8
	(44.9)	(10.5)	(21.3)	(0.107)	(10.7)	(20.0)
	[.]	[0.001]	[0.002]	[.]	[.]	
P: cash	0.355	0.000	0.115	0.206	0.663	0.144
= workshop	[.]	[0.001]	[0.062]	[.]	[.]	
P: cash	0.363	0.429	0.127	0.526	0.113	0.959
= combined	[.]	[0.341]	[0.341]	[.]	[.]	
P: workshop	0.935	0.000	0.002	0.549	0.193	0.199
= combined	[.]	[0.001]	[0.002]	[.]	[.]	
P: cash	0.378	0.985	0.202	0.288	0.025	0.822
+ workshop	[.]	[0.971]	[0.676]	[.]	[.]	
= combined						
Placebo mean	733	163	99.7	2.90	341	146
# clusters	413	413	413	413	413	413
# obs	7,242	7,242	7,243	7,235	7,240	7,232
	Revenue	Input Expenditure	Hired Labour	Technology Adoption	Labour Supply (Days)	Profits
Panel B: Non-farm						
Workshop	284**	174**	16.8	0.084**	9.0*	109
	(136)	(87)	(14.3)	(0.035)	(5.3)	(70)
	[.]	[0.100]	[0.138]	[.]	[.]	
Cash	452***	305***	15.2	0.082**	18.2***	213***
	(139)	(92)	(16.9)	(0.036)	(6.0)	(72)
	[.]	[0.002]	[0.226]	[.]	[.]	
Combined	557**	442**	17.1	0.116***	13.4**	167
	(275)	(187)	(17.2)	(0.042)	(6.6)	(114)
	[.]	[0.039]	[0.190]	[.]	[.]	
P: cash	0.269	0.166	0.915	0.954	0.095	0.167
= workshop	[.]	[0.498]	[0.843]	[.]	[.]	
P: cash	0.723	0.481	0.908	0.388	0.479	0.706
= combined	[.]	[1.000]	[1.000]	[.]	[.]	
P: workshop	0.191	0.063	0.979	0.421	0.453	0.544
= combined	[.]	[0.144]	[0.958]	[.]	[.]	
P: cash	0.500	0.832	0.495	0.339	0.100	0.209
+ workshop	[.]	[1.000]	[1.000]	[.]	[.]	
= combined						
Placebo mean	815	478	43.2	0.350	106	313
# clusters	413	413	413	413	413	413
# obs	7,241	7,082	7,243	7,243	7,240	7,031

Notes: All variables are at the household level and scaled to annual figures. All currency values are measured in constant 2018 USD PPP. Farm activities capture agricultural and livestock related production. Non-farm activities capture production in non-farm enterprises owned or operated by household members. Revenue, input expenditure, hired labour expenditure and labour supply are measured as in Table 1. Technology adoption for farm activities is a dummy equal to one if the household used one of 14 modern agricultural practices since intervention. For non-farm enterprises, it is one if during the last 12 months, an enterprise introduced new or improved products or services or went into a new market or accessed new customers. For farm enterprises, returns to factors of production is revenue minus expenditure on intermediate inputs and costs of renting in assets minus costs of hired labour. For non-farm enterprises, profits are revenue minus input expenditure and hired labour. Model specification is as in Table 2. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within the family 'Inputs and Hired Labour', separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.6: Treatment Effects on Assets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Non-land Asset Components					
	Non-land Assets	Durables	Livestock	Savings	Maize	Land Purchases	Housing Expend-itures
Workshop	98**	43*	25.4	26.4**	2.97	12.8	18
	(46)	(25)	(26.9)	(11.7)	(4.70)	(8.0)	(26)
	[.]	[0.144]	[0.299]	[0.113]	[0.359]	[.]	[.]
Cash	406***	236***	132.4***	29.8***	9.35*	34.6**	487***
	(50)	(31)	(27.7)	(10.1)	(5.26)	(14.0)	(34)
	[.]	[0.001]	[0.001]	[0.003]	[0.020]	[.]	[.]
Combined	352***	224***	77.0***	46.2***	7.92	43.8***	465***
	(47)	(27)	(26.5)	(16.2)	(5.51)	(10.6)	(33)
	[.]	[0.001]	[0.005]	[0.005]	[0.040]	[.]	[.]
P: cash = workshop	0.000	0.000	0.000	0.787	0.267	0.062	0.000
	[.]	[0.001]	[0.001]	[0.553]	[0.217]	[.]	[.]
P: cash = combined	0.292	0.689	0.053	0.341	0.810	0.566	0.591
	[.]	[1.000]	[0.269]	[1.000]	[1.000]	[.]	[.]
P: workshop = combined	0.000	0.000	0.067	0.204	0.390	0.006	0.000
	[.]	[0.001]	[0.112]	[0.158]	[0.243]	[.]	[.]
P: cash + workshop	0.025	0.167	0.040	0.597	0.552	0.854	0.430
= combined	[.]	[0.335]	[0.192]	[0.502]	[0.502]	[.]	[.]
Placebo mean	1,529	765	576	122	65.6	9.22	196
# clusters	413	413	413	413	412	413	413
# obs	7,242	7,242	7,243	7,241	7,170	7,239	7,168

Notes: All variables are at the household level and scaled to annual figures. All currency values are measured in constant 2018 USD PPP. Non-land assets are made up of durable assets, livestock, savings and stocks of dried maize. Respondents estimate the value of household holdings of each asset if they were to sell them today in their current condition. For cash savings, we include savings in multiple places, as well as the payout households receive from ROSCAs to which they belong. Land purchases are the value of any compound and non-compound land that was purchased since the intervention. Housing expenditures includes the costs of any housing repairs, maintenance and construction since intervention. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within the family of non-land non-housing assets, separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.7: Treatment Effects on Individual-level Education Expenditure

	(1)	(2)	(3)	(4)	(5)	(6)
	Education Expenditure		Fee Expenditure		Non-fee Expenditure	
Household members aged:	6-13	14-20	6-13	14-20	6-13	14-20
Workshop	9.03*	2.3	7.67*	2.5	1.01	-1.55
	(5.01)	(14.8)	(3.95)	(13.2)	(1.56)	(3.75)
	[.]	[.]	[0.118]	[1.000]	[0.350]	[1.000]
Cash	9.56**	37.6**	7.50*	29.4*	2.15*	7.15*
	(4.66)	(18.8)	(3.86)	(16.6)	(1.29)	(4.17)
	[.]	[.]	[0.107]	[0.096]	[0.107]	[0.096]
Combined	13.29**	69.6***	11.18**	62.7***	1.74	7.76**
	(5.56)	(17.7)	(4.51)	(15.8)	(1.44)	(3.81)
	[.]	[.]	[0.029]	[0.001]	[0.130]	[0.022]
P: cash = workshop	0.923	0.063	0.968	0.105	0.483	0.054
	[.]	[.]	[1.000]	[0.118]	[1.000]	[0.118]
P: cash = combined	0.502	0.130	0.411	0.075	0.786	0.888
	[.]	[.]	[1.000]	[0.177]	[1.000]	[0.799]
P: workshop = combined	0.467	0.000	0.446	0.000	0.660	0.021
	[.]	[.]	[1.000]	[0.001]	[1.000]	[0.011]
P: cash + workshop	0.458	0.236	0.485	0.168	0.498	0.700
= combined	[.]	[.]	[0.993]	[0.507]	[0.993]	[0.539]
Placebo mean	85.7	342	61.2	278	24.6	63.0
# clusters	411	408	410	408	411	408
# obs	12,003	8,528	11,936	8,447	11,936	8,437

Notes: All variables are at the individual level, with one observation for each household member aged 6-20. All currency values are measured in constant 2018 USD PPP. Education expenditure is the total expenditure on school-related fees and non-fee expenses during the current and preceding school years for each child belonging to the relevant age group. Fees include school and activity fees while non-fee expenditure includes other school related supplies (e.g. books), and uniforms. The number of clusters varies across columns because some small villages have no sampled households with members aged 6-13 or 14-20. The sample size is higher for total expenditure than either of fee or non-fee expenditure. If only one type of expenditure is missing for a household, we calculate that household's total education expenditure as the non-missing component times the sample mean ratio of total education expenditure over the non-missing expenditure component. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, age, gender and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. Sharpened q-values controlling for the false discovery rate across outcomes within each family, namely each of the education measures for each age group, separately for each intervention arm, are shown in brackets.

Table A.8: Treatment Effects on Individual-level Education Participation and Enrolment

	(1)	(2)	(3)	(4)
	Education Participation		Education Enrolment	
	6-13	14-20	6-13	14-20
Workshop	0.004	0.005	0.002	0.013
	(0.013)	(0.017)	(0.006)	(0.013)
	[1.000]	[1.000]	[1.000]	[1.000]
Cash	-0.004	0.006	-0.013**	0.013
	(0.012)	(0.016)	(0.006)	(0.013)
	[0.577]	[1.000]	[0.088]	[1.000]
Combined	-0.009	-0.006	-0.009	0.011
	(0.012)	(0.015)	(0.007)	(0.013)
	[0.553]	[1.000]	[0.553]	[1.000]
P: cash = workshop	0.573	0.975	0.021	0.961
	[0.402]	[1.000]	[0.044]	[1.000]
P: cash = combined	0.727	0.457	0.549	0.830
	[1.000]	[1.000]	[1.000]	[1.000]
P: workshop = combined	0.371	0.538	0.088	0.874
	[0.228]	[1.000]	[0.215]	[1.000]
P: cash + workshop	0.652	0.477	0.823	0.399
= combined	[1.000]	[0.913]	[1.000]	[0.913]
Placebo mean	0.892	0.679	0.977	0.753
# clusters	412	409	412	409
# obs	11,913	8,404	12,316	8,813

Notes: All variables are at the individual level, with one observation for each household member aged 6-20. Education participation is defined as the share of days of school attended out of the last 5, coded as zero for non-enrolled students. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, age, gender and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. Sharpened q-values controlling for the false discovery rate across outcomes within each family, namely each of the education measures for each age group, separately for each intervention arm, are shown in brackets.

Table A.9: Treatment Effects on Aspirations

	(1)	(2)	(3)	(4)
		Index Components		
	Aspirations Index	Assets	Income	Education
Workshop	0.092***	265	127	0.226***
	(0.035)	(565)	(216)	(0.067)
	[.]	[0.742]	[0.742]	[0.003]
Cash	0.130***	2,008***	611***	0.058
	(0.036)	(624)	(205)	(0.076)
	[.]	[0.005]	[0.005]	[0.177]
Combined	0.178***	2,511***	570**	0.130*
	(0.040)	(606)	(242)	(0.072)
	[.]	[0.001]	[0.020]	[0.030]
P: cash = workshop	0.324	0.007	0.030	0.019
	[.]	[0.022]	[0.022]	[0.022]
P: cash = combined	0.259	0.457	0.874	0.293
	[.]	[1.000]	[1.000]	[1.000]
P: workshop = combined	0.027	0.000	0.062	0.137
	[.]	[0.002]	[0.066]	[0.101]
P: cash + workshop	0.396	0.788	0.602	0.104
= combined	[.]	[1.000]	[1.000]	[0.456]
Placebo mean	0.000	8,499	5,357	15.5
# clusters	413	413	413	410
# obs	7,232	7,204	7,185	6,102

Notes: Column (1) is an Anderson (2008) index consisting of variables in columns (2), (3) and (4). Column (1) is an Anderson (2008) index consisting of variables in columns (2), (3) and (4). The index definition includes all observations that have non-missing values for at least one component, using only the non-missing components in the averaging. Income aspirations are the level of annual income that a household would like to reach at the end of the next ten years. Income is defined as all sources of cash income for the household, including earnings from production and transfers from any NGO or government programmes. Asset aspirations are the level of assets that the household would like to reach at the end of the next ten years, including their house, furniture, consumer goods and a transport vehicles. Income and assets are measured in constant 2018 USD PPP. Education aspirations are the aspirations for years of education attained by a randomly selected child, set to missing for households without children. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family, separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.10: Additional Treatment Effects on Psychological Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)
		Index Components				
	Self-beliefs Index	Self-Efficacy Score Standardised	Locus of Control Score Standardised	Growth Mindset Score Standardised	Effort Beliefs Standardised	Depression Score >10
Workshop	0.006	-0.042	-0.019	0.055	-0.036	0.010
	(0.046)	(0.044)	(0.038)	(0.041)	(0.026)	(0.018)
	[.]	[0.536]	[0.536]	[0.536]	[0.536]	[.]
Cash	-0.053	-0.063	-0.043	-0.006	-0.057**	-0.031**
	(0.042)	(0.043)	(0.037)	(0.039)	(0.025)	(0.016)
	[.]	[0.257]	[0.318]	[0.475]	[0.105]	[.]
Combined	0.025	0.010	0.019	0.022	-0.020	-0.030*
	(0.044)	(0.045)	(0.038)	(0.037)	(0.026)	(0.016)
	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[.]
P: cash = workshop	0.198	0.628	0.552	0.131	0.423	0.021
	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[.]
P: cash = combined	0.043	0.057	0.097	0.420	0.127	0.933
	[.]	[0.205]	[0.205]	[0.205]	[0.205]	[.]
P: workshop = combined	0.687	0.221	0.355	0.391	0.562	0.020
	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[.]
P: cash + workshop	0.224	0.041	0.125	0.601	0.039	0.701
= combined	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[.]
Placebo mean	0.000	0.000	0.000	0.000	0.000	0.658
# clusters	413	413	413	413	413	413
# obs	7,221	7,211	7,213	7,209	7,218	7,213

Notes: Column (1) is an Anderson (2008) index consisting of variables in columns (2), (3) and (4). The index definition includes all observations that have non-missing values for at least one component, using only the non-missing components in the averaging. The self-beliefs index is made up of self-efficacy, locus of control, and growth mindset scales. Self-efficacy is measured with the Schwarzer & Jerusalem (1995) scale. Locus of control is measured using the Internal subscale from the Internal, Powerful Others and Chance (IPC) scale (Levenson, 1981). Growth mindset is measured with an adapted version of the 6-item Implicit Theories of Intelligence scale (Blackwell et al., 2007). We show standardised scores for all scales. In Table 3, mental health is the 10-item CES-D depression scale from Andresen et al. (1994), multiplied by minus one. Column (6) in this table is a binary variable where individuals with scores at or above a threshold of 10 are identified as at high risk of depression or as experiencing psychological distress. This is how the score is used if it is used for clinical screening. The effort beliefs measure is not prespecified. It consists of a subset of items from the self-efficacy (SE) and locus of control (LOC) scales which capture effort beliefs, perseverance towards a set goal. Scales measuring effort beliefs capture beliefs about the role of effort in achieving goals and tendency to persist despite obstacles (Duckworth & Quinn, 2009). We include the following items: i) When I get what I want, it's usually because I worked hard for it (LOC); ii) I can always manage to solve difficult problems if I try hard enough (SE); iii) I can solve most problems if I invest the necessary effort (SE); iv) When I am confronted with a problem, I can usually find several solutions (SE); v) If I am in trouble, I can usually think of a solution (SE). Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. Sharpened q-values controlling for the false discovery rate across outcomes within each family, namely self-beliefs and depression, separately for each intervention arm, are shown in brackets.

Table A.11: Treatment Effects on Self-Efficacy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Scale Components						
	Self-Efficacy Score Standardised	Stick to Plans to Achieve Goals	Find Solutions to Problems	Problem-Solving With Effort	Problem-Solving Through Hard Work	Achieve Goals Despite Opposition	Persist Through Difficulty	Think of Solution When in Trouble
Workshop	-0.042	-0.057	-0.081**	-0.041	-0.000	0.044	-0.039	-0.023
	(0.044)	(0.045)	(0.038)	(0.042)	(0.038)	(0.037)	(0.039)	(0.037)
	[.]	[0.657]	[0.304]	[0.657]	[0.861]	[0.657]	[0.657]	[0.861]
Cash	-0.063	-0.056	-0.106***	-0.071*	0.001	-0.040	0.018	-0.029
	(0.043)	(0.042)	(0.040)	(0.042)	(0.032)	(0.037)	(0.037)	(0.035)
	[.]	[0.466]	[0.063]	[0.375]	[0.920]	[0.521]	[0.783]	[0.690]
Combined	0.010	0.019	-0.004	-0.015	-0.014	0.046	0.022	-0.033
	(0.045)	(0.047)	(0.039)	(0.044)	(0.038)	(0.037)	(0.035)	(0.035)
	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
P: cash	0.628	0.986	0.516	0.482	0.968	0.027	0.135	0.881
= workshop	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[0.234]	[0.685]	[1.000]
P: cash	0.057	0.076	0.006	0.152	0.693	0.016	0.922	0.910
= combined	[.]	[0.147]	[0.047]	[0.218]	[0.653]	[0.052]	[0.653]	[0.653]
P: workshop	0.221	0.108	0.044	0.538	0.754	0.958	0.081	0.794
= combined	[.]	[0.337]	[0.337]	[1.000]	[1.000]	[1.000]	[0.337]	[1.000]
P: cash	0.041	0.029	0.001	0.084	0.784	0.399	0.390	0.699
+ workshop	[.]	[0.097]	[0.005]	[0.163]	[0.812]	[0.470]	[0.470]	[0.812]
= combined								
Placebo mean	0.000	-0.000	0.000	0.000	0.000	-0.000	0.000	-0.000
# clusters	413	413	413	413	413	413	413	413
# obs	7,211	7,208	7,212	7,208	7,209	7,211	7,207	7,207

Notes: We use the Schwarzer & Jerusalem (1995) Generalised Self-efficacy scale. Column 1 is the standardised sum of scores on all the items on a Likert scale, with higher scores indicating stronger agreement with the item and higher self-efficacy. Columns 2-8 show treatment effects on standardised scores on individual items. The full wording for items in these columns is: 'It is easy for me to stick to my aims and accomplish my goals'; 'When I am confronted with a problem, I can usually find several solutions'; 'I can solve most problems if I invest the necessary effort'; 'I can always manage to solve difficult problems if I try hard enough'; 'If someone opposes me, I can find the means and ways to get what I want'; 'I can remain calm when facing difficulties because I can rely on my coping ability'; 'If I am in trouble, I can usually think of a solution'. Coefficients are from OLS regressions of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across all outcomes except the summary index, separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.12: Treatment Effects on Beliefs About Returns to Specific Investments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Maize Farming					Education		
	Yields			Returns		Income		Returns
	Usual	w/ Fertiliser	w/ Labour	Fertiliser	Labour	Secondary	Degree	Degree
Workshop	2.95	-6.65	-4.34	0.009	-0.036	-193.8	-392	0.041
	(6.91)	(12.57)	(9.81)	(0.077)	(0.031)	(140.9)	(341)	(0.139)
	[.]	[.]	[.]	[1.000]	[1.000]	[.]	[.]	[1.000]
Cash	3.50	15.40	-8.75	0.152*	-0.064**	-78.5	-200	0.126
	(7.15)	(13.85)	(9.41)	(0.078)	(0.030)	(138.8)	(315)	(0.146)
	[.]	[.]	[.]	[0.086]	[0.086]	[.]	[.]	[0.149]
Combined	1.59	-4.16	-10.96	0.025	-0.034	-67.9	-98	0.053
	(6.99)	(12.75)	(10.08)	(0.074)	(0.034)	(125.2)	(330)	(0.145)
	[.]	[.]	[.]	[1.000]	[1.000]	[.]	[.]	[1.000]
P: cash = workshop	0.947	0.130	0.648	0.100	0.364	0.461	0.570	0.550
	[.]	[.]	[.]	[0.430]	[0.574]	[.]	[.]	[0.579]
P: cash = combined	0.798	0.159	0.808	0.093	0.338	0.933	0.745	0.581
	[.]	[.]	[.]	[0.387]	[0.510]	[.]	[.]	[0.633]
P: workshop = combined	0.859	0.849	0.522	0.835	0.950	0.342	0.381	0.931
	[.]	[.]	[.]	[1.000]	[1.000]	[.]	[.]	[1.000]
P: cash + workshop	0.617	0.483	0.874	0.190	0.118	0.255	0.273	0.542
= combined	[.]	[.]	[.]	[0.398]	[0.398]	[.]	[.]	[0.398]
Placebo mean	191	459	258	2.02	0.442	3,684	11,695	3.32
# clusters	413	411	410	411	410	410	410	410
# obs	7,115	6,420	6,147	6,420	6,147	5,137	5,137	5,137

Notes: Columns (1), (2), and (3) show the expected yield of maize with respectively no fertiliser and their current labour input, 50kg of fertiliser per acre and their current labour input, and no fertiliser and 12 hours of extra labour per week. The yields are for a one acre plot in the next long rains season, measured in gorogoro (a local unit approximately equal to 2 kilograms) per acre. The questions ask about DAP fertiliser, the most commonly used fertiliser in the region. Columns (4) and (5) show the implied returns from respectively extra fertiliser and extra labour, with a scale where 1 = 100% return. Columns (6) and (7) show the expected annual income for the respondent's child in two scenarios: complete secondary schooling with a KCSE certificate and with a university degree. Column (8) shows the implied return to a university degree, with a scale where 1 = 100% return. The questions ask about the same child as the education aspirations and expectations questions, at age 30, with earnings expressed in 2018 USD PPP. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across the three returns measures, separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.13: Treatment Effects on Measures that Plausibly Capture Reporting Biases

	(1)	(2)	(3)	(4)	(5)	(6)
	Housing quality		Asset ownership		Reporting biases	
	Self-reported	Verified	Self-reported	Verified	Information Recall	Video Mimicry
Workshop	18	0.009	-0.027	0.06	-0.040	0.005
	(26)	(0.037)	(0.352)	(0.41)	(0.025)	(0.042)
Cash	487***	0.515***	1.758***	1.58***	-0.016	-0.022
	(34)	(0.042)	(0.361)	(0.40)	(0.026)	(0.039)
Combined	465***	0.531***	0.933***	1.17***	-0.024	0.052
	(33)	(0.041)	(0.318)	(0.40)	(0.024)	(0.045)
Placebo mean	196	1.56	20.6	17.3	0.274	-0.000
# clusters	413	413	413	413	409	413
# obs	7,168	7,242	7,243	7,243	6,536	7,235

Notes: Asset ownership and housing quality are measured at the household level. Odd-numbered columns present self-reported measures, while even numbered columns present their field officer-verified analogues. Self-reported housing expenditure is household expenditure on repair, maintenance or construction of housing since intervention. For the verified housing quality score, fieldworkers rated materials used to construct the roof, wall and floor based on expense. Roof materials ratings are: 0 = leaves, grass or tins; 1=iron, cement, tiles or asbestos. Walls material ratings are: 0 = mud, or unburnt bricks; 1 = iron/tin sheets, wood, or mud and cement; 2 = burnt/stabilised brick, cement blocks, or concrete and stones. Floor materials scores are: 0 = mud/earth, other organic, or part organic, part finished; 1 = wood, cement or tiles. Field officer-verified assets are fieldworker observations of the number of seven durable assets cooking pots and pans, jerry cans, chairs/sofa, tables, radios, TVs and poultry houses, counted after the survey finished. The self-reported assets are the number of those specific assets reported in the assets module earlier in the survey. Information recall is an indicator equal to one if respondents correctly recall information about the returns to education for Kenyan men from Ozier (2018), which appears in both the aspirations and placebo video. This question is asked the same day that respondents watch the videos. Mimicry of videos is the standardised sum of indicator variables coded to one if the respondent engaged in any of the following activities at endline, all of which are featured in the videos: (a) weaved baskets; (b) kept savings in a jar; (c) attended a sewing class; (d) trained as a teacher; (e) grew vegetables to sell on the market. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.14: Treatment Effects on Asset Ownership

	(1) Asset Ownership	(2) Cooking Pots/pans	(3) Jerry Cans	(4) Chairs / Sofas	(5) Tables / Desks	(6) Radios	(7) TVs / CD players	(8) Livestock Houses
Panel A: Self-reported								
Workshop	-0.027	0.041	-0.142	0.008	0.047	-0.035	0.024	0.030*
	(0.352)	(0.112)	(0.210)	(0.124)	(0.032)	(0.021)	(0.018)	(0.016)
Cash	1.758***	0.386***	0.200	0.819***	0.201***	0.058***	0.095***	0.005
	(0.361)	(0.114)	(0.199)	(0.144)	(0.030)	(0.021)	(0.018)	(0.015)
Combined	0.933***	0.289**	-0.159	0.529***	0.122***	0.066***	0.066***	0.023
	(0.318)	(0.116)	(0.195)	(0.150)	(0.030)	(0.020)	(0.018)	(0.020)
P: cash = workshop	0.000	0.002	0.069	0.000	0.000	0.000	0.000	0.132
P: cash = combined	0.012	0.369	0.048	0.059	0.008	0.639	0.108	0.345
P: workshop = combined	0.004	0.031	0.923	0.000	0.023	0.000	0.028	0.753
P: cash + workshop = combined	0.106	0.381	0.436	0.124	0.005	0.134	0.040	0.630
Placebo mean	20.6	5.81	6.75	5.57	1.56	0.676	0.170	0.129
# clusters	413	413	413	413	413	413	413	413
# obs	7,243	7,239	7,238	7,239	7,238	7,239	7,238	7,237
Panel B: Field Officer-verified								
Workshop	0.06	0.086	-0.138	0.029	0.047	-0.013	0.027*	0.022*
	(0.41)	(0.148)	(0.209)	(0.141)	(0.041)	(0.026)	(0.015)	(0.013)
Cash	1.58***	0.275*	0.164	0.789***	0.205***	0.071**	0.071***	0.017
	(0.40)	(0.153)	(0.197)	(0.151)	(0.040)	(0.028)	(0.014)	(0.011)
Combined	1.17***	0.235	-0.055	0.596***	0.189***	0.132**	0.064***	0.017
	(0.40)	(0.153)	(0.182)	(0.181)	(0.064)	(0.054)	(0.017)	(0.013)
P: cash = workshop	0.000	0.193	0.109	0.000	0.000	0.002	0.008	0.729
P: cash = combined	0.322	0.780	0.193	0.315	0.807	0.269	0.693	0.970
P: workshop = combined	0.008	0.334	0.650	0.001	0.016	0.002	0.033	0.722
P: cash + workshop = combined	0.427	0.553	0.770	0.342	0.380	0.184	0.126	0.195
Placebo mean	17.3	4.52	5.52	5.11	1.39	0.542	0.129	0.087
# clusters	413	413	413	413	413	413	413	413
# obs	7,243	7,239	7,239	7,239	7,239	7,239	7,238	7,237

Notes: Field officer verified assets (Panel B) are objective measures of the number of seven durable assets – cooking pots and pans, jerry cans, chairs/sofa, tables, radios, TVs and poultry houses. The field officer was asked to count these assets after completing the endline survey. The self-reported assets (Panel A) are the number of those specific assets reported in the assets module earlier in the survey. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.15: Spillover Effects of the Workshop on Economic Outcomes of Ineligible Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Index Components					
	Economic Index	Labour Supplied (Days)	Inputs & Hired Labour	Education Index	Revenue	Non-Land Assets	Consumption Expenditure
Workshop	0.028	-8.20	62	0.088	210	-31	-25.1
	(0.053)	(27.44)	(140)	(0.063)	(258)	(108)	(123.0)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
Placebo mean	-0.000	549	1,053	0.000	2,573	2,264	3,839
# clusters	306	306	306	303	306	306	306
# obs	2,792	2,789	2,792	2,193	2,792	2,792	2,783

Notes: This table shows treatment effects of the workshop on economic outcomes for ineligible households living in the study villages. This sample was only surveyed in a subset of study villages. Each of the economic variables is defined in Table 1 or 2. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family, separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.16: Spillover Effects of Workshop on Psychological Mechanisms of Ineligible Households

	(1)	(2)	(3)	(4)
	Self-belief Index	Aspirations Index	Expectations Index	Mental health Z-score
Workshop	0.047	-0.010	-0.010	0.099*
	(0.068)	(0.051)	(0.065)	(0.055)
	[1.000]	[1.000]	[1.000]	[0.390]
Placebo mean	0.000	-0.000	0.000	-0.000
# clusters	306	306	306	306
# obs	2,785	2,784	2,782	2,776

Notes: This table shows the treatment effects of the workshop on psychological outcomes for ineligible households living in the study villages. Each of the psychological variables is defined in Section 3.1. We did not measure time or risk preferences or beliefs about returns to investments in this sample. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at the village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family, separately for each intervention arm, are shown in brackets. *; **; *** denote significance at the 10; 5; and 1 percent levels respectively. Odd columns are the estimates from OLS regressions of the aspirations index (1) and expectations index (3) on the other psychological mechanisms.

Table A.17: Treatment Effects on Aspirations and Perceived Current Level Gaps (Standardised)

	(1)	(2)	(3)	(4)	(5)	(6)
	Asset Aspirations	Perceived Current Assets	Asset Aspirations - Perceived Current Assets	Income Aspirations	Perceived Current Income	Income Aspirations - Perceived Current Incomes
Cash	0.118***	0.170***	0.091**	0.104***	0.089**	0.089***
	(0.037)	(0.032)	(0.038)	(0.035)	(0.037)	(0.034)
	[0.004]	[0.001]	[0.009]	[0.005]	[0.009]	[0.008]
Combined	0.147***	0.173***	0.121***	0.097**	0.091*	0.071*
	(0.036)	(0.040)	(0.035)	(0.041)	(0.051)	(0.038)
	[0.001]	[0.001]	[0.001]	[0.015]	[0.030]	[0.030]
Placebo mean	-0.000	0.000	-0.000	-0.000	-0.000	-0.000
# clusters	413	413	413	413	413	413
# obs	7,204	7,216	7,199	7,185	7,159	7,126

Notes: This table shows treatment effects of the cash and combined interventions on normalised values of future aspirations for assets and income (columns 1 and 4), perceived levels of assets and income at the endline (columns 2 and 5), and the gap between aspirations and perceived current levels (columns 3 and 6). We define assets as "the worth of your house, your furniture, consumer goods like a TV and fridge and any transport vehicles." We define income as "all sources of cash income for your household, including what you earn from all agricultural and non-agricultural activities, and money that you have received from any NGO or government programmes". For both assets and income, we measure, in order, the household's current position (for example, "What is the total value of all the assets owned by your household?") and the respondent's aspirations, the level the respondent would like their household to reach. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across all outcomes separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.18: Treatment Effects of the Workshop at Different Levels of Baseline Economic Resources

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Index Components					
	Economic Index	Labour Supplied (Days)	Inputs & Hired Labour	Education Index	Revenue	Non-Land Assets	Consumption Expenditure
Panel A: Consumption as wealth proxy							
Workshop	0.109***	27.1**	230**	0.021	268*	99.8**	143*
	(0.033)	(11.7)	(99)	(0.033)	(154)	(45.8)	(74)
	[.]	[0.064]	[0.064]	[0.109]	[0.064]	[0.064]	[0.064]
Interaction * wealth	0.000	-0.002	0.088*	-0.000	0.062	0.035	-0.027
	(0.000)	(0.005)	(0.047)	(0.000)	(0.069)	(0.021)	(0.031)
	[.]	[0.921]	[0.439]	[0.731]	[0.600]	[0.439]	[0.600]
Placebo mean	-0.000	525	857	-0.000	2,101	1,529	3,796
# clusters	413	413	413	412	413	413	412
# obs	7,243	7,240	7,243	6,379	7,243	7,242	7,224
Panel B: Non-land assets as wealth proxy							
Workshop	0.107***	26.2**	232**	0.020	257*	96.1**	139*
	(0.033)	(11.6)	(102)	(0.033)	(156)	(45.6)	(75)
	[.]	[0.077]	[0.077]	[0.136]	[0.077]	[0.077]	[0.077]
Interaction * wealth	-0.000	-0.006	0.109	-0.000	-0.059	0.031	0.033
	(0.000)	(0.008)	(0.080)	(0.000)	(0.112)	(0.056)	(0.058)
	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Placebo mean	-0.000	525	857	-0.000	2,101	1,529	3,796
# clusters	413	413	413	412	413	413	412
# obs	7,243	7,240	7,243	6,379	7,243	7,242	7,224
Panel C: Assets including land as wealth proxy							
Workshop	0.109***	26.8**	229**	0.021	261*	96.0**	139*
	(0.033)	(11.6)	(101)	(0.033)	(156)	(45.8)	(75)
	[.]	[0.076]	[0.076]	[0.130]	[0.079]	[0.076]	[0.076]
Interaction * wealth	-0.000	-0.001	0.006	-0.000*	0.001	-0.001	0.006
	(0.000)	(0.001)	(0.006)	(0.000)	(0.010)	(0.005)	(0.005)
	[.]	[0.658]	[0.658]	[0.658]	[0.859]	[0.859]	[0.658]
Placebo mean	-0.000	525	857	-0.000	2,101	1,529	3,796
# clusters	413	413	413	412	413	413	412
# obs	7,243	7,240	7,243	6,379	7,243	7,242	7,224

Notes: This table shows heterogeneous treatment effects of the workshop on economic outcomes. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, a wealth proxy, the interaction of treatment assignments and the wealth proxy, the baseline outcome, sublocation fixed effects, endline month fixed effects, and prespecified baseline covariates. Each panel shows results from using a separate wealth proxy. The wealth proxy in panel C is the value of all non-loan assets including the respondents' assessment of the value of their land and housing. All other proxies and all outcomes are defined in Table 1 or 2. We report heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family, separately for each intervention arm, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

B Data Collection, Sampling, Treatment Assignment & Receipt

B.1 Sources of Data and Sample Eligibility

We collected five types of data: (1) a **village census** of elders in 415 villages; (2) a **household census** of 41,322 households in these villages; (3) a **baseline survey** of 8,309 sampled households; (4) an **endline survey**, aimed at the same households and (5) **price surveys** throughout the study period. Baseline surveys ran from April 2016 to March 2017. Interventions ran from November 2016 to July 2017. Endline surveys ran from May 2018 to February 2019. Interventions were not run in a sublocation until we had completed baseline surveys for all villages in that sublocation.

Census: We use the village census to collect village-level variables for stratifying the village-level randomisation. We use the household census to determine study eligibility and draw the baseline sample. Of the 41,322 households censused, 32,964 participated in the study, 4,677 refused to participate in research, and 3,681 were not at home.

Identifying poor households and drawing baseline sample: Of those who chose to participate, the group relevant for our study is those who meet GiveDirectly's means test for living in poverty. Households are classified as poor if they met any one of four simple criteria: (1) they had per capita housing space less than 62,000cm^2; (2) they had a mud floor and no mobile telephone;[49] (3) they had a mud floor and the household head was a widow; or (4) the household included an orphan. GiveDirectly has found these criteria to be strong predictors of living below poverty lines defined in terms of consumption expenditure and this is true in our data. Approximately 43% of households in the study villages met the eligibility criteria for living in poverty.

Our study sample excludes some means-tested-poor households: those which do not contain an adult female (4.5%) and those which are polygamous (11%), due to difficulties associated with household definition. These households were still eligible for GiveDirectly transfers. From the remaining households, we sample roughly 20 households per village. If one of the 20 target households could not be found or refused to participate, the field officers included one household on the reserve list as a replacement. Sampling probabilities vary by village because we don't sample exactly the same share of treatment-eligible households in each village. None of the descriptive or causal results in the paper change substantively when we reweight the data to account for variation in sampling probabilities.

The majority of sample households are included because they meet GiveDirectly's first two criteria. 66% of households classified as eligible in our sample meet the first means test criterion, small houses; 25% meet the second criterion, reflecting low levels of housing quality and asset

[49] Households who did not have a mobile phone in the census and were offered a cash transfer were also offered a mobile phone, the cost of which was deducted from the transfer.

ownership. 35% meet criterion 3 (widows with mud floors) and 29% meet criterion 4 (the household contains an orphan). Our survey data shows widows with mud floors and households with orphans are also very likely to be poor: 85% of households who meet these criteria are below the World Bank poverty line in 2018 USD PPP for Kenya, compared to 90% of households who meet criteria 1 and 2.

Attrition from baseline to endline: We define the study sample as all households that completed the baseline survey. In the endline, households that refused to participate or could not be located are treated as attriters. We attempt extensive tracking of all migrants within and outside the study area, including to nearby cities and towns. We have a total response rate of 87.1% of baseline households. Of the endline sample, 1.2% are surveys with migrants and 0.4% are households that split between baseline and endline. For split households, we surveyed the original respondent and a representative from the other part of the split household and average both responses. Table B.1 shows that **attrition does not differ by treatment assignment** (column 1) and does not differ by treatment $\times$ baseline household characteristics (column 5). This shows that the composition of each group remains similar at endline. Attrition is slightly lower for larger households (column 3) but this does not differ between treatment groups. Reasons for attrition are also balanced between treatment groups, with 8.8% attriting due to refusal and the remainder due to death or inability to contact them.

Price surveys: We conducted baseline and endline price surveys of 55 markets in the study area to collect prices for commonly purchased goods and services to construct some economic measures. Baseline price surveys ran from August to November 2016. Endline price surveys ran from May to September 2018.

B.2 Treatment Assignment and Treatment Receipt

Randomisation protocol: We stratify randomisation on location (an administrative division in Kenya containing roughly 10-50 villages); village size (a dummy for if village size exceeded the sample median); and a measure of household asset ownership at village level, using data from the village and household censuses before any baseline surveys began (see Table B.2 for descriptives). Within each block, we randomly assigned villages to the four arms. Some blocks were not multiples of four. We took the residual villages from these blocks, combined them with other villages in the same location and asset category, and randomised within these units.

Balance tests: Table B.2 reports the results of balance tests in the baselined sample. All tests compare the mean values of baseline village- and household-level covariates between the placebo group and each treatment arm (columns 3, 5, and 7) or jointly across all four arms (column 8). The test results are consistent with random assignment: we reject equality across all four arms at the 10% level for only 1/25 tests and at the 5% level for 0/25 tests.

Table B.1: Relationship between Attrition, Treatment Assignment, and Baseline Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Workshop	0.020	(0.014)			0.055	(0.056)
Cash	0.009	(0.013)			0.027	(0.050)
Combined	-0.001	(0.012)			0.064	(0.049)
Household Size			-0.011***	(0.002)	-0.007*	(0.004)
Age			-0.001*	(0.000)	-0.008	(0.005)
Non-land Assets			0.004	(0.003)	-0.002	(0.005)
Consumption			0.001	(0.001)	-0.000	(0.001)
At Least Primary Education			0.005	(0.008)	-0.000	(0.001)
At Least Secondary Education			0.019	(0.017)	-0.000	(0.001)
Married			0.018*	(0.010)	-0.001	(0.001)
Household Size * Workshop					-0.009	(0.008)
Household Size * Cash					0.000	(0.010)
Household Size * Combined					-0.016	(0.011)
Age * Workshop					0.002	(0.010)
Age * Cash					0.000	(0.006)
Age * Combined					0.009	(0.008)
Non-land Assets * Workshop					-0.001	(0.004)
Non-land Assets * Cash					0.002	(0.004)
Non-land Assets * Combined					-0.001	(0.004)
Consumption * Workshop					0.003	(0.015)
Consumption * Cash					-0.013	(0.021)
Consumption * Combined					0.012	(0.024)
At Least Primary Education * Workshop					0.004	(0.023)
At Least Primary Education * Cash					0.044	(0.033)
At Least Primary Education * Combined					-0.025	(0.046)
At Least Secondary Education * Workshop					-0.022	(0.048)
At Least Secondary Education * Cash					-0.042	(0.046)
At Least Secondary Education * Combined					0.008	(0.017)
Married * Workshop					0.039	(0.028)
Married * Cash					0.001	(0.026)
Married * Combined					-0.002	(0.024)
P: All arms = 0	0.362				0.869	
P: cash = workshop	0.411				0.374	
P: cash = combined	0.433				0.612	
P: workshop = combined	0.114				0.662	
Placebo mean	0.122		0.122		0.122	
# obs	8,309		8,309		8,309	

Notes: This table shows the relationship between attrition, treatment assignment, and prespecified baseline covariates. All columns show regressions of a household-level indicator for not being surveyed at endline on treatment arm indicators (cols 1-2); baseline covariates (cols 3-4); and treatment arm indicators, baseline covariates, and their interactions (cols 5-6). The p-values in the bottom panel are for tests of selective attrition. For example: the top right p-value is for a test of the null hypothesis that Workshop = Cash = Combined = 0 and Workshop*X = Cash*X = Combined*X = 0 for all covariates X; the bottom right p-value is for a test of the null hypothesis that workshop = Combined and Workshop*X = Combined*X for all covariates X. All regressions include sublocation fixed effects. The consumption and asset aggregates are measured in constant 2018 USD PPP (000s). If a baseline covariate is missing, we replace the missing values with the sample mean and include a missing data indicator. Heteroskedasticity-robust standard errors, clustered at village level, are reported in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table B.2: Baseline Summary Statistics and Tests of Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Placebo	Workshop		Cash		Combined		F-test	
	Mean	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	p-value	# obs
Panel A: Village-level Characteristics from Census									
Has Primary School	0.490	-0.010	0.890	0.057	0.427	0.028	0.702	0.802	415
Has Market	0.288	-0.075	0.232	-0.040	0.528	-0.028	0.669	0.685	415
Has Clinic	0.163	-0.075	0.129	-0.048	0.323	0.002	0.964	0.294	415
Number of Households	96.3	-2.73	0.679	-5.35	0.349	0.062	0.993	0.756	415
Mean Household Asset Score	0.030	-0.032	0.515	-0.017	0.728	-0.022	0.649	0.929	415
Floor Material is Mud or Organic†	0.666	0.023	0.155	-0.002	0.884	-0.009	0.581	0.188	415
Roof Material is Grass, Leaves or Other†	0.054	-0.006	0.297	0.000	0.976	0.003	0.631	0.420	415
Walls Material is Unburnt Bricks or Mud†	0.846	0.005	0.670	0.003	0.821	0.001	0.954	0.973	415
Drinking Water is Piped/Well†	0.385	0.047	0.089	-0.012	0.705	0.020	0.526	0.206	415
Lighting is Electricity†	0.284	0.004	0.749	0.008	0.492	-0.003	0.787	0.796	415
Panel B: Eligible Respondent Characteristics from Census									
Married	0.584	0.039	0.027	0.018	0.310	0.001	0.961	0.093	8,309
Age	40.8	-0.603	0.283	-0.866	0.153	-0.808	0.197	0.485	8,302
At Least Primary Education	0.423	0.018	0.253	0.026	0.109	0.024	0.123	0.345	8,274
Household Owns a Mobile Phone	0.741	-0.008	0.628	0.017	0.265	0.011	0.490	0.441	7,743
Panel C: Eligible Household Characteristics from the Baseline									
Household Size	5.31	0.075	0.388	0.031	0.720	0.021	0.818	0.845	8,309
Dependency Ratio	1.35	-0.004	0.923	0.023	0.537	0.026	0.500	0.802	8,308
Number of Household Members Under 16	2.85	0.020	0.751	0.052	0.438	0.032	0.649	0.890	8,309
Revenue Aggregate	1,834	-71.0	0.381	32.0	0.693	38.6	0.674	0.572	8,309
Consumption Expenditure	4,331	-78.1	0.470	-83.2	0.420	-98.7	0.359	0.778	8,295
Expenditure on Productive Inputs	699	-125	0.067	-48.6	0.507	-40.2	0.661	0.228	8,309
Value of Non-land Assets	1,230	-7.57	0.887	-0.049	0.999	-3.80	0.946	0.999	8,309
Total Household Labour Supply (Days)	431	-3.50	0.808	-6.72	0.646	-0.218	0.988	0.965	8,283
Education Expenditure	439	7.00	0.779	-10.2	0.717	-13.0	0.589	0.853	6,958
Self-beliefs Index	-0.000	0.013	0.789	0.051	0.246	-0.031	0.520	0.395	8,270
Aspirations Index	0.000	0.024	0.562	0.011	0.774	0.061	0.113	0.369	8,283

Notes: The table reports balance tests for characteristics measured in the village census, household census, and baseline surveys. Panel A reports regressions at the village level. Panel B reports characteristics of eligible respondents who are the primary women in eligible households. Panel C reports household-level characteristics. All balance tests are implemented by regressing the characteristic on a vector of treatment assignments and sublocation fixed effects. The regressions use one observation per village for the village-level characteristics and one observation per household for the household- and respondent-level characteristics. Inference is performed using heteroskedasticity-robust standard errors, clustered by village for regressions with household- or respondent-level characteristics. Column (1) reports the placebo mean for each characteristic. Columns (2)-(7) report treatment arm-specific coefficients and p-values. Column (8) reports the p-value from test of joint equality of means for all four treatment arms. Column (9) reports the number of observations. The household asset score is constructed using principal component analysis on indicators for household ownership of a telephone, bicycle, solar panel, TV, fridge, radio, watch/clock, motorbike, truck and iron box (charcoal or electric). Economic variables in Panel C are measured in constant 2018 USD PPP annually. The dependency ratio is the number of household members under 16 divided by the number of members 16 or above. Outcomes with a † sign denote village-level proportions constructed from household-level data.

Treatment receipt: Table B.3 reports the relationship between treatment assignment and treatment receipt. Compliance is the same between each pair of arms used for our main comparisons: between the aspirations workshop and the placebo workshop arms, between the cash transfer and placebo workshop arm, and between the cash and combined arms. Any differences between these pairs of interventions do not result from differences in intervention take-up between the two groups. Columns 5-8 show statistics for only endlined households, the sample used to estimate treatment effects. The first row shows the number of households assigned to each treatment group. Panel A shows the share of households in each treatment group (in columns) that received each possible combination of treatments, using data capturing which households are present at a workshop (in our records) or receive cash (using GiveDirectly's records). We do not show separate rows for receiving the aspirations and placebo workshops because households could only receive whichever one of these was assigned to their village. Panel B shows few differences in treatment receipt rates between relevant pairs of experimental arms: roughly 90% of endlined households in each of the four groups receive the workshop they are assigned. For example, 89% of endlined households in the placebo group receive the placebo; 90% of endlined households in the aspirations group receive the aspirations workshop ('Workshop only' row of Panel A, column 5). Receipt differs by only 0.6 percentage points with $p=0.66$ ('Workshop Only' row of Panel B, columns 5/6). Any differences in outcomes between these groups do not arise because of differences in attendance at the workshop.

Similarly, outcome differences between the cash transfer and placebo group are unlikely to be due to differences in workshop attendance: 89% of endlined households in both group receive the placebo workshop.

Finally, any differences in outcomes between the cash and combined groups are unlikely to arise because of differences in workshop attendance or cash transfer take-up between these groups. The cash and combined groups have four different treatment receipt measures. In Panel A, they can receive neither the cash transfer nor a workshop ('None' row); cash but not a workshop ('Cash Only' row); a workshop but not cash ('Workshop Only' row) or both interventions ('Cash and Workshop' row). Receipt of the workshops is similar between the cash and combined arms: respectively 90 and 92% of endlined households receive the workshop (sum of 'Workshop only' and 'Cash and Workshop' rows, columns 7 and 8). Receipt of the cash transfers is also similar between the cash and combined arms: respectively 79 and 81% of endlined households have a payment record for having receive the cash transfer (sum of rows 'Cash Only' and 'Cash and Workshop', columns 7 and 8). The differences in the portion of the cash and combined arms in each of the 'None', 'Cash Only', 'Workshop Only' and 'Cash and Workshop' categories range from 0.2 to 1.7 percentage points and no difference is significant with $0.54<p<0.84$ (Panel B, columns 7/8). A

further 4.5–5% of households in both arms may have received cash but GiveDirectly do not have a payment record for them (last row of Panel C).

Unlike many programme evaluations, we can also document reasons for households not receiving the cash transfer programme, among households for whom we have GiveDirectly records in both arms (Panel C). The portion of the sample who do not receive transfers for different reasons is similar in the cash and combined groups, suggesting similar composition of these groups. Treatment receipt of any treatment is also not clearly related to households' baseline characteristics (analysis not shown).

Perhaps surprisingly, there is higher take-up of the once-off workshop (roughly 90%) than the cash transfer (roughly 80%). This does not affect internal validity: we do not seek to compare the cash transfer and workshop except in the benefit-cost comparisons, where we compare benefits and costs accounting for all considerations that affect programme operation in a real-world setting.[50]

Roughly 10% of endlined households are not found by GiveDirectly – the adult female is not at home, has left the village, is found upon revisit to be ineligible or cannot be found for other reasons (rows 2-4 of Panel C). This plausibly reflects difficulties of tracking households in rural settings in all four arms and is similar to the portion of respondents who do not take up the workshops. A further 10% of our endline sample are documented by GiveDirectly as refusing the cash transfers. Reasons included that households did not trust GiveDirectly (75%), were told by someone else not to take the transfer (24%) or did not need money (1%). Both the rates and reasons for refusal are fairly common for programmes of this nature. For example, the multi-country study by Banerjee et al. (2015) reports that a bundled antipoverty programme had a refusal rate of 48% in India and close to zero in other countries. In the US, non-participation among eligible households is 66% for the Temporary Assistance for Needy Families and 32% for Supplemental Security Income, two cash transfer programmes (Ribar, 2014).[51] The *reasons* for refusal we document are also similar to those documented by many other programmes and hence are unlikely to mean that conclusions cannot be generalised: a recent Cochrane review finds lack of trust in programme providers and beneficiaries not wishing to be identified as poor are common reasons for refusal (Atkins et al., 2020).

[50]This has a limited effect on our comparison of the two interventions' benefit-cost ratios because lower cash take-up lowers both the numerator – because the average benefits are intention-to-treat effects – and the denominator – because the average costs are per person offered each intervention – relative to full take-up.

[51]There is little published data on *rates* of refusals in unconditional cash transfer programmes in developing countries: two published studies of GiveDirectly cash transfers do not report receipt rates (Egger et al., 2022; Haushofer & Shapiro, 2016), nor does the prominent study by Baird et al. (2011).

Table B.3: Treatment Receipt Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline Sample by Treatment Group				Endline Sample by Treatment Group			
	Placebo	Workshop	Cash	Combined	Placebo	Workshop	Cash	Combined
# households	2,012	2,057	2,085	2,157	1,767	1,766	1,814	1,896
Panel A: Intervention Receipt Rates (%)								
None	16.20	16.29	14.92	11.54	10.64	9.97	7.22	5.80
Cash only	0.05	0.00	2.59	2.83	0.06	0.00	2.65	2.80
Workshop only	83.75	83.62	14.15	14.79	89.30	89.92	13.45	13.03
Cash and workshop	0.00	0.10	68.35	70.84	0.00	0.11	76.68	78.38
Panel B: Test for Equal Treatment Receipt Rates between Groups (p-values)								
Cash only	-		0.730		-		0.844	
Workshop only	0.946		0.764		0.664		0.832	
Cash and workshop	-		0.485		-		0.538	
All	-		0.231		-		0.722	
Panel C: Cash Transfer Receipt Rates and Reasons for Non-receipt (%)								
Received cash	-	-	70.94	73.67	-	-	79.33	81.17
Ineligible	-	-	3.07	2.83	-	-	2.87	2.85
Not found/home	-	-	2.30	2.32	-	-	1.71	1.58
Left village	-	-	0.96	1.21	-	-	0.66	0.84
Refused	-	-	18.13	15.25	-	-	10.64	9.12
Unknown/Other	-	-	4.60	4.73	-	-	4.80	4.43

Notes: This table shows receipt of treatment for households in each treatment arm. Columns (1), (2), (3) and (4) are statistics for the baseline sample while columns (5), (6), (7) and (8) are for the endline sample. Panel A shows the percentage of each treatment group that received each of the four possible treatment combinations: no treatment (row 1), only cash (row 2), only the workshop (row 3), or both cash and the workshop (row 4). 'Workshop' in rows 3 and 4 refers to completing the treatment workshop or placebo. Panel B shows p-values from tests across pairs of treatment groups for equal rates of treatment receipt: only cash (row 1), only the workshop (row 2), and both cash and the workshop (row 3). The fourth row shows the p-value from a joint test of equality of all four treatment receipt rates. Each p-value is centred between the columns corresponding to the two groups on which the test is run. Panel C shows the percentage of the cash-assigned treatment groups that received cash (row 1 of the panel) or did not receive cash for each reason (rows 2-6). Inference is performed using heteroskedasticity-robust standard errors, clustered at village level.

C Conceptual Framework Appendix

Overview: This appendix develops the conceptual framework, sketched in Section 6.3, that shows how the workshop might influence economic outcomes through seven different psychological mechanisms. The framework shows that the mechanisms have similar predicted impacts on economic outcomes, so we cannot evaluate these mechanisms using only data on economic outcomes. Instead, we directly measure the psychological mechanisms, estimate treatment effects on the mechanisms, and show in Section 6.1 that the workshop shifts only two closely related mechanisms

after 17 months: aspirations and expectations. Although this framework considers seven possible mechanisms, we emphasize that we do not view these mechanisms as equally plausible. We prespecified three of them – time and risk preferences and mental health – as unlikely mechanisms that we measured only for the purpose of ruling them out.

Basic model without psychological components: In each period t, people derive utility $u(c_t,l_t)$ from consumption c_t and leisure l_t. Utility is additively separable through time with discount factor δ. People enter period t with asset holdings a_t and receive revenue y_t, which they allocate between consumption and capital investment k_t, generating the budget constraint $a_t+y_t \geq c_t+k_t$. They allocate their labour endowment T between leisure and labour e_t. Their investment decisions generate revenue $y_{t+1}=f(k_t,e_t;\theta)$ in the next period, where θ is a productivity parameter. Assets depreciate at rate γ between periods. We assume that u and f satisfy standard conditions: $u_x>0$ and $u_{xx}<0$ for $x=c,l$; and $f_y>0$, $f_{yy}<0$, and $f_{y\theta}>0$ for $y=k,e$.

This framework has one state variable – assets a_t – and two control variables – consumption c_t and leisure l_t. The first-order conditions are

$$u_{c_t}=\delta\cdot(1+f_{k_t}-\gamma)\cdot u_{c_{t+1}} \tag{2}$$

$$u_{l_t}=\delta\cdot f_{e_t}\cdot u_{c_{t+1}} \tag{3}$$

Condition (2) is an Euler equation: the marginal utility of current consumption is set equal to the discounted marginal utility of future consumption arising from current capital investment. Condition (3) is the labour-leisure trade-off: the marginal utility of current leisure is set equal to the discounted marginal utility of future consumption arising from current labour.

Our six main economic outcomes proxy for concepts in this framework. Our measures of consumption, labour supply, and assets proxy for those concepts in this framework. Our measures of expenditure on productive inputs and hired labour and revenue proxy loosely for capital investment and revenue respectively. We also measure spending on education, which can be viewed as another form of capital investment, as children are a key source of old-age revenue for parents in settings like rural Kenya with limited formal savings or pensions. We do not attempt to separately model multiple types of investment in education, livestock, non-farm enterprises, etc., as this is outside the scope of a primarily empirical paper.

Introducing psychological components into the model: We sequentially introduce seven psychological components into the model. These correspond to the seven psychological mechanisms that we measure empirically and describe in Section 3. We explain briefly how the workshop might shift each mechanism, show how the mechanism can enter the model introduced above, and derive predicted impacts of a change in the mechanism on economic outcomes.

Aspirations as reference points for consumption: We extend the basic model to allow reference-dependent utility, where people receive an extra utility gain from reaching or exceeding their reference point for consumption h_t. The utility function becomes

$$u(c_t,l_t)=v(c_t,l_t)+z(c_t-h_t), \tag{4}$$

We interpret h_t as akin to aspirations.[52] We follow the literature in assuming that aspirations are malleable but that agents cannot choose their reference point (e.g. Lybbert & Wydick 2018), returning to this assumption later in the appendix. We assume v is increasing and concave in both arguments, as is standard. We also assume people gain utility at a decreasing rate from exceeding their reference point and lose utility from missing it ($z'>0$, $z''<0$, $\text{sign}\{z(c-h)\}=\text{sign}\{c-h\}$).[53]

The first-order conditions become

$$v_{c_t}+z_{c_t}=\delta\cdot(1+f_{k_t}-\gamma)\cdot\left(v_{c_{t+1}}+z_{c_{t+1}}\right) \tag{5}$$

$$v_{l_t}=\delta\cdot f_{e_t}\cdot(v_{c_{t+1}}+z_{c_{t+1}}). \tag{6}$$

In the Euler equation (5), the distance of consumption from the reference point now influences the marginal utilities of both current and future consumption, respectively z_{c_t} and $z_{c_{t+1}}$. The marginal utility of current leisure in equation (6) does not directly depend on the reference point.

This framework shows how the aspirations workshop, by raising aspirations for the future h_{t+1}, can generate the estimated treatment effects on economic outcomes. Higher h_{t+1} increases the marginal utility of future consumption $v_{c_{t+1}}+z_{c_{t+1}}$. Formally, this occurs because z is a concave function of c_t-h_t. Intuitively, this occurs because raising h_{t+1} shifts agents farther below their consumption reference point, making gains in future consumption more valuable. The higher marginal utility of future consumption motivates people to invest now to afford higher future consumption: they raise current capital investment to maintain condition (5) and raise current labour supply to maintain condition (6). This matches the positive treatment effects on input expenditure and on labour supply that we observe in the experiment. This causes higher future revenue and asset values through $f(k_t,e_t;\theta)$, matching the positive treatment effects on revenue and assets we observe.

The effect of an increase in aspirations on consumption at the time of the endline survey is

[52] Like almost all models in the existing literature, we consider aspirations over a single domain, consumption. See Genicot & Ray (2020) for a rare exception that models aspirations over multiple domains under specific assumptions. We do not attempt to separately model aspirations over the three domains in which we measure aspirations: assets, income, and children's education. Instead, we suggest that these might all be viewed at least in part as means for respondents to finance future consumption. We measured aspirations for these domains rather than aspirations for consumption because, in qualitative scoping work, these were more natural domains for respondents to describe a better life.

[53] Other models use slightly different assumptions about the shape of z to study different behaviour, including loss aversion over gambles (DellaVigna et al., 2017; Kahneman & Tversky, 1979). These behaviours are less relevant for our study so we assume z is concave and continuous for tractability.

theoretically ambiguous. There is an intertemporal substitution effect: there is more incentive to invest now and consume in the future, so the time path of consumption will steepen and consumption in periods close to t will fall. But there is also a wealth effect, as the rise in current investment and labour supply increases future assets. Which of these effects dominates in any specific period depends on the parameterisation of the model, as in Deaton (1992). The positive treatment effect we estimate on consumption after 17 months suggests a large role for the wealth effect.

Our framework assumes that aspirations are malleable but are not a choice variable for the household. Otherwise, a "rational" person would choose $h_t = 0$ to maximise utility, irrespective of their economic environment or receipt of interventions. The facts that most people in our sample set aspirations higher than their current position and that our intervention changes aspirations and other behaviours suggest that this assumption is plausible. However, this assumption makes the utility consequences of our intervention ambiguous. It is possible that the intervention raises both h_t and consumption but lowers $c_t - h_t$ enough to lower utility. We cannot directly evaluate this possibility without strong modeling assumptions and/or extensive measures of subjective well-being.

Reference-dependence, aspirations, and expectations: Expectations can enter this framework in two different ways. First, expectations may be another proxy for the reference points, as in Kőszegi & Rabin (2006, 2007). Expectations then have a dual role: they determine economic choices via reference points, and also reflect beliefs about the outcomes of economic choices, leading to complex equilibrium concepts when expectations are formed endogenously. Second, aspirations might proxy for reference points, be affected by the workshop, and drive changes in economic choices and outcomes, while expectations capture beliefs about the outcomes of these changes in economic choices. In the first interpretation, expectations respond directly to the workshop and then affect economic outcomes. In the second interpretation, expectations respond indirectly to the workshop via effects on economic outcomes. The close empirical relationship between aspirations and expectations in our data is consistent with both interpretations, so we do not argue that either one is more plausible than the other.

Self-beliefs, including about planning ability: The workshop may raise participants' beliefs about their ability to achieve desired outcomes, proxied by our empirical measure of self-efficacy and to a lesser extent locus of control and growth mindset. Higher self-efficacy can enter the conceptual framework as a higher perceived value of the productivity parameter θ, as in Ghosal et al. (2020) and McKelway (2021). If θ enhances the marginal productivity of labour supply and capital investment ($f_{k\theta}, f_{e\theta} > 0$), then the aspirations workshop will raise the right-hand sides of conditions (2) and (3). To maintain the first-order conditions, people will reallocate from consumption to capital investment and from leisure to labour supply. This will increase

future revenue and asset values through the revenue function. This will steepen the time path of consumption but have an ambiguous effect on the level of consumption at endline, for the same reasons as in the reference-dependence framework.[54]

Perceived return to investment: The workshop may raise participants' beliefs about the returns to specific investment activities, which we measure empirically. Higher beliefs about the return to investment enter the conceptual framework as a higher perceived value of the productivity parameter θ. This has the same impact on economic behavior as higher self-beliefs, discussed above.

Time preferences: The workshop may raise participants' patience, which we measure empirically. Higher patience enters the framework as a higher discount factor δ, increasing the utility weight on future relative to current consumption. In the more general quasi-hyperbolic discounting framework from Laibson (1997), agents discount utility τ periods in the future by $\beta \cdot \delta^{\tau}$ rather than δ^{τ}. The aspirations workshop could change preferences in this framework by raising either β or δ. These would raise the right-hand side of conditions (2) and (3), leading to the same impacts on economic behavior as the previous mechanisms.

Risk preferences: The workshop may lower participants' risk aversion, which we measure empirically. Risk could enter the framework making future revenue stochastic conditional on current investment, generalising the first-order condition (2) to $u_{c_t} = \delta E_t\left[(1+f_{k_t}-\gamma)u_{c_{t+1}}\right]$. Lower risk aversion lowers the value of certain consumption and leisure today relative to uncertain future outcomes and *can* lead to higher current labour supply and capital investment and hence higher future revenue and asset value. But the exact prediction will depend on the extent to which increased absolute or relative risk aversion affects the covariance across states of the world between f_{k_t} and $u_{c_{t+1}}$.

Mental health: The workshop may improve participants' mental health, which we measure empirically through a depression scale. This may enter the framework in multiple ways due to the diversity of depression's symptoms, making predictions difficult. Depression may lead to pessimistic beliefs (MacLeod & Salaminiou, 2001), reduced cognitive functioning (Prado et al., 2018; Keller et al., 2019) or altered time and risk preferences (Ya'akov et al., 2019; Cobb-Clark et al., 2021). These could enter the framework respectively as low expected returns to investment, lower productivity or altered preferences, with predictions as outlined above.

Appendix References

Anderson, M. (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*

[54] These predictions hold if the workshop increases *only* the perceived value of θ or if the workshop increases *both* the perceived and actual values of θ. The only distinction is that an increase in the actual value of θ will have a larger effect on future revenue and asset values, because it directly raises the return on investment.

American Statistical Association, 103 (484), 1481–1495.

Andresen, E., J. Malmgren, W. Carter & D. Patrick (1994) "Screening for Depression in Well Older Adults: Evaluation of a Short Form of the CES-D," *American Journal of Preventive Medicine*, 10 (2), 77–84.

Atkins, S., K. Sidney-Annerstedt, K. Viney, T. Wingfield, D. Boccia & K. Lönnroth (2020) "Experiences of Conditional and Unconditional Cash Transfers Intended for Improving Health Outcomes and Health Service Use: A Qualitative Evidence Synthesis," *The Cochrane Database of Systematic Reviews*, 2020 (6).

Baird, S., C. McIntosh & B. Ozler (2011) "Cash or Condition? Evidence from a Cash Transfer Experiment," *The Quarterly Journal of Economics*, 126 (4), 1709–1753.

Banerjee, A., E. Duflo, N. Goldberg et al. (2015) "A Multifaceted Program Causes Lasting Progress for the Very Poor: Evidence from Six Countries," *Science*, 348 (6236), 12607991–126079916.

Cobb-Clark, D., S. Dahmann & N. Kettlewell (2021) "Depression, Risk Preferences and Risk-Taking Behavior," *Journal of Human Resources*, 57 (5), 1566–1604.

Deaton, A. (1992) *Understanding Consumption*, Oxford UK: Oxford University Press.

DellaVigna, S., A. Lindner, B. Reizer & J. F. Schmieder (2017) "Reference-Dependent Job Search: Evidence from Hungary," *The Quarterly Journal of Economics*, 132 (4), 1969–2018.

Egger, D., J. Haushofer, E. Miguel, P. Niehaus & M. Walker (2022) "General Equilibrium Effects of Cash Transfers: Experimental Evidence from Kenya," *Econometrica*, 90 (6), 2603–2643.

Genicot, G. & D. Ray (2020) "Aspirations and Economic Behavior," *Annual Review of Economics*, 12, 715–746.

Ghosal, S., S. Jana, A. Mani, S. Mitra & S. Roy (2020) "Sex Workers, Self-Image and Stigma: Evidence from Kolkata Brothels," *The Review of Economics and Statistics*, 104 (3), 431–448.

Haushofer, J. & J. Shapiro (2016) "The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya," *The Quarterly Journal of Economics*, 131 (4), 1973–2042.

Kahneman, D. & A. Tversky (1979) "Prospect Theory: An Analysis of Decision Under Risk," *Econometrica*, 47, 263–291.

Keller, A., J. Leikauf, B. Holt-Gosselin, B. Staveland & L. Williams (2019) "Paying Attention to Attention in Depression," *Translational Psychiatry*, 9 (1), 1–12.

Kőszegi, B. & M. Rabin (2006) "A Model of Reference-Dependent Preferences," *The Quarterly Journal of Economics*, 121 (4), 1133–1165.

——— (2007) "Reference-Dependent Risk Attitudes," *American Economic Review*, 97 (4), 1047–1073.

Laibson, D. (1997) "Golden Eggs and Hyperbolic Discounting," *The Quarterly Journal of Economics*, 112 (2), 443–478.

MacLeod, A. & E. Salaminiou (2001) "Reduced Positive Future-Thinking in Depression: Cognitive and Affective Factors," *Cognition & Emotion*, 15 (1), 99–107.

McKelway, M. (2021) "Women's Employment in India: Intra-Household and Intra-Personal Constraints," *Dartmouth, Working Paper*.

Prado, C., S. Watt & S. Crowe (2018) "A Meta-Analysis of the Effects of Antidepressants on Cognitive Functioning in Depressed and Non-Depressed Samples," *Neuropsychology Review*, 28 (1), 32–72.

Ribar, D. (2014) "How to Improve Participation in Social Assistance Programs," *IZA World of Labor*, 104.

Ya'akov, M., Z. Shtudiner, O. Suhorukov & N. Grisaru (2019) "Time and Risk Preferences and Consumption Decisions of Patients with Clinical Depression," *Journal of Behavioral and Experimental Economics*, 78, 138–145.