The Future in Mind: Aspirations and Long-term Outcomes in Rural Ethiopia*

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Abstract

Aspirations may condition the future-oriented choices of individuals and thus may play a role in the persistence of poverty or the effort to break out of it. We run a randomised control trial in remote, rural Ethiopia to explore this and evaluate an intervention which aims to change how poor people perceive their future opportunities, alter their aspirations and, through that, modify their investment decisions. A treatment group was shown video documentaries featuring individuals from similar communities who escaped poverty through their own efforts and serve as relatable role models. Five years after the screening took place, the treated households had increased future-oriented investments in agriculture, children's education and assets. The results can be explained by an increase in aspirations in terms of lifetime goals. Overall, this research uniquely provides evidence that a light-touch behavioural intervention can have persistent economic impacts on a poor population.

Keywords: aspirations, long-term, investment, role models
JEL codes: D03; I31

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1 Introduction

The persistence of extreme poverty has long concerned economists and policymakers. Recent theoretical research suggests that social and psychological factors may play a role in perpetuating it.[1] Interventions targeting specific psychological attributes can change beliefs and improve economic outcomes in the short term among low-income groups.[2] A few recent studies examine long-term effects of intensive clinical psychological treatments on economic outcomes of depressed adults (Baranov et al., 2020; Bhat et al., 2022) or men at risk of engaging in criminal behaviour (Blattman et al., 2023). But it remains unclear whether scalable, population-wide behavioural interventions can have lasting impacts. This paper addresses this gap: we show that a light-touch behavioural intervention has persistent economic impacts after five years on a random sample of a population mostly living in extreme poverty.

Our intervention is based on the idea that those living in extreme poverty may struggle to envision a better future, which lowers their aspirations and reduces motivation to invest. They may have had fewer successful role models from their community for inspiration (Appadurai, 2004; Durlauf, 1996; Genicot and Ray, 2020; Ray, 2006) and may face persistent social norms or values driving demotivating beliefs rooted in "zero-sum" thinking (Carvalho et al., 2023). Our intervention aims to increase individuals' economic aspirations and change their mental models of what they can become and achieve (Hoff and Stiglitz, 2016).[3] The intervention uses video documentaries showcasing successful role models from similar communities, drawing on psychological theories that people's aspirations are influenced by the outcomes of others like them (Bandura, 1977; La Ferrara, 2019).

We test this intervention in a field experiment in a remote, poor district in Ethiopia with 2,112 individuals (1,152 households). Some households (head and spouse) were randomly invited to watch video documentaries we made about individuals from similar areas who had succeeded in agriculture or business through their own efforts. In the videos, the role models describe how they improved their socio-economic position from being poor to being relatively successful, through setting goals, perseverance and hard work. We compare the treated group to a placebo group, who were randomly invited to watch an Ethiopian entertainment programme, and to a control group, who were simply surveyed. We collected data before the intervention, straight after the screening occurred, after six months, and again five years later.[4]

We find that this simple intervention significantly improves economic outcomes after five

1. See, for example, Appadurai (2013); Benabou (1996); Bisin and Verdier (2001); Dalton, Ghosal, and Mani (2016); Durlauf (1996); Genicot and Ray (2017); Lybbert and Wydick (2018).

2. Recent studies include Ashraf et al. (2022); Barker et al. (2022); Bossuroy et al. (2022); Campos et al. (2017); Ghosal et al. (2022); Heller et al. (2016); John and Orkin (2022); McKelway (2021). See Kremer, Rao, and Schilbach (2019) for a review.

3. We conceptualise aspirations as desired goals for the future, which motivate investment and effort in order to attain them (Bandura, 1977; Locke and Latham, 1990).

4. A working paper, Bernard et al. (2014), reported the six month results. This paper expands on and replaces that paper. Subsequently, other experiments using videos of role models have appeared, presenting short-run results on economic and other outcomes (Batista and Seither, 2019; Cecchi et al., 2022; Leight et al., 2024; Lubega et al., 2021; Orkin et al., 2023; Rojas Valdes, Wydick, and Lybbert, 2021).

years by increasing effort and investment.[5] First, five years after the experiment, treated households report higher labour supply and more use of agricultural inputs. They spend around one extra hour per day working on their own farms, are more likely to spend on modern crop inputs, and on animal feed and vet supplies, and have increased holdings of tools by 20 per cent. Second, treated households increase investments in human capital. At the five-year follow-up, treated households spend approximately 36 per cent more than other study participants on their children's education. Their children have attained more years of schooling: twice as many children who were of school-going age at the time of screening have completed full primary school five years later. Third, treated households experience meaningful changes in living standards: they report one and a half fewer weeks of food insecurity in the previous year, have accumulated 29 per cent larger holdings of durable goods, like cellphones and household equipment, and have improved housing quality, although consumption expenditure is not affected. We find suggestive evidence that economic behaviour began to change soon after the treatment. Using a shorter survey collected six months after the screenings, we find increases in savings and investment in education.

We find evidence consistent with these economic changes resulting from increases in the individuals' aspirations for the future, where aspirations are desired goals for the future, that motivate investment and effort. We use locally validated survey measures of aspirations (Bernard and Taffesse, 2014). These capture the level of income, assets, or children's education individuals hoped to achieve in their lifetime or expected they would achieve in ten years. The treatment group have higher aspirations and expectations after five years, relative to both placebo and control. There are positive, although noisier, effects straight after treatment and after six months, consistent with aspirations increasing slowly as people experiment with possible alternative futures (Appadurai, 2004) or learn-by-doing when demotivating beliefs start to shift (Carvalho et al., 2023). Individuals may start investing, seeing the returns of their investments, and aiming to do better in the future. We can also rule out the possibility that our intervention gave "false hope" or "led to frustration", lowering effort, a possibility highlighted by some existing models of aspirations (Dalton, Ghosal, and Mani, 2016; Genicot and Ray, 2017). We see positive effects on labour supply, one measure of effort, and small positive effects on subjective wellbeing, rather than negative ones.

We can exclude some plausible alternative mechanisms through which the intervention could have led to this outcome. We measure time and risk preferences, grit, information transmission, and beliefs about the returns to technology. We find no change in these. We do find some effects six months after exposure to the videos on measures of locus of control – beliefs about whether individuals or fate control people's lives. However, these do not persist after five years, unlike effects on aspirations. We view this as a less likely mechanism: if locus of control drove changes in investment, we would expect effects on investment to dissipate

5. Results reported in the introduction are significant relative to the control group at 5 per cent or less, and robust to multiple hypothesis testing.

alongside effects on locus of control. The design of the intervention also rules out further mechanisms. Unlike in other studies that rely on variation in exposure to real-life role models such as teachers or peers, participants receive no mentorship or support other than exposure to our videos (Kearney and Levine, 2020). Exposure to the screening itself or to outsiders or being selected for the intervention do not account for effects: a placebo group shown a local entertainment programme are unaffected relative to the control group. Lastly, we run a suggestive test for whether there are spillovers from treated to untreated individuals, for example if untreated individuals learn by observing treated households' changes in behaviour. We find little evidence of large spillovers, even though our tests face limitations.

This study contributes to the literature in three ways. We provide the first experimental evidence on the longer term (beyond two years) effects of an intervention targeting aspirations on economic investment and outcomes. Several theoretical models posit that aspirations can influence investment (Besley, 2016; Dalton, Ghosal, and Mani, 2016; Genicot and Ray, 2017) and several papers use observational data to document aspiration-investment associations (Janzen et al., 2017; Ross, 2019; Serneels and Dercon, 2021; Eble and Escueta, 2022). A growing number of papers test interventions to shift aspirations and economic outcomes in the short-run, including light-touch interventions similar to ours targeting households in disadvantaged circumstances (Leight et al., 2024; Lubega et al., 2021; Orkin et al., 2023) or entrepreneurs (Batista and Seither, 2019), as well as more intensive multi-week trainings to promote future-oriented behaviour, including elements building aspirations, encouraging delayed gratification and promoting ambitious mindsets (Cecchi et al., 2022; McKenzie, Mohpal, and Yang, 2022; Rojas Valdes, Wydick, and Lybbert, 2021), with mixed effects. We track 96 per cent of households after five years, a high fraction compared to recent long-term follow-ups of experiments in low- and middle-income countries (Bouguen et al., 2019).

Second, within the literature on the psychology of poverty, we add to the limited evidence on the long-term impacts of psychological interventions. A growing experimental literature, including in poor settings, examine the effects on decision-making of other psychological characteristics beyond aspirations (Alan and Ertac, 2018; Alan, Boneva, and Ertac, 2019; Ashraf et al., 2022; Blattman, Jamison, and Sheridan, 2017; Blattman et al., 2023; Bossuroy et al., 2022; Campos et al., 2017; Heller et al., 2016; Ghosal et al., 2022; John and Orkin, 2022; McKelway, 2021) and of clinically diagnosed mental health conditions (Angelucci and Bennett, 2024; Baranov et al., 2020; Barker et al., 2022; Bhat et al., 2022; Haushofer, Mudida, and Shapiro, 2020).[6] Our study provides the first longer-term evidence on how a light-touch non-targeted population-wide behavioural intervention affects economic outcomes, showing how overcoming households' internal psychological constraints can unlock investment.

Finally, we contribute to work on the effect of role models on investments. Existing work finds that exposure to role models (through in-person interactions or visual media) affects

6. Only Baranov et al. (2020), Bhat et al. (2022), and Blattman et al. (2023) study long-term economic impacts (respectively after seven, five, and ten years), all focusing on intensive psychotherapy interventions.

educational and labour outcomes, especially among youths (Greene, Sullivan, and Beyard-Tyler, 1982; Stout et al., 2011; Beaman et al., 2012; Jensen and Oster, 2009; Fairlie, Hoffmann, and Oreopoulos, 2014; Chong, Duryea, and La Ferrara, 2012; Lafortune, Riutort, and Tessada, 2018; Bjorvatn et al., 2020; Riley, 2022; Ahmed et al., 2024; Kipchumba et al., 2024).[7] We add to this literature by showing that exposure to role models has persistent effects on adults' labour supply, investment and asset wealth; by using an experimental design to cleanly identify causal links between exposure to role models, changes in aspirations, and behaviour; and by examining a range of psychological mechanisms through which role model effects might occur.

Our findings can be seen as a proof of concept, showing that this type of intervention targeting the psychological mechanism of aspirations may have persistent impacts, even if the magnitude of effects differs across contexts. We are cautious about the external validity of the findings. The study area was chosen because it was remote, with limited exposure to media, so even a single screening was a memorable experience, potentially contributing to the persistent and relatively substantial impacts. With these caveats in mind, these findings can also yield insights for designing effective poverty-reduction interventions: a carefully designed but low-cost intervention induces both psychological and economic changes that persist after five years, suggesting a promising avenue for research, intervention design, and adaptation across multiple contexts. A back-of-the-envelope calculation suggests the intervention was cost-effective, with durable asset increases alone outweighing the costs by a factor of two at a reasonable scale of implementation.

Section 2 describes the context, our sample and its characteristics at baseline. Section 3 motivates the experiment, with a conceptual framework to understand aspirations and descriptive evidence of aspirations which may limit investment in our setting. Section 4 discusses our intervention design, estimation strategy and tests for experimental integrity. Section 5 provides the results of the intervention on investment decisions; indicators of household well-being; aspirations; and alternative mechanisms, after five years. It also reports results on a subset of measures after six months, provides cost-effectiveness analysis, and tests for spillovers. Section 6 concludes.

2 Context, the sample and its characteristics

2.1 Context

Our study took place in Doba, a mountainous remote rural administrative district of Ethiopia, 380 kilometres east of the capital city of Addis Ababa. Most of Doba's population are Muslim and ethnically Oromo (88 per cent), the rest Orthodox Christian and Amhara.

At the time of the experiment, Doba was one of the poorer districts in the country: it was one of the first districts selected in 2005 for the national social protection programme targeted at the most chronically food-insecure districts in Ethiopia. Doba is also extremely remote: most surveyed villages were accessible only by 4x4 vehicle and some required camel transportation. Households had limited exposure to life beyond their district. At baseline

7. For reviews of the role model literature in economics see La Ferrara (2016) and Serra (2025).

there was limited exposure to television: only 11 per cent of respondents watched TV once a week or more, 28 per cent watched at least once a month and 62 per cent watched about once a year, if ever. Only 4 per cent of the households owned a cellphone, and no household owned a television. Doba residents would be likely to find a television show featuring characters like them memorable and to pay attention to the screening content.

During our study, Ethiopia was one of the world's fastest-growing economies, with GDP growth of almost 10 per cent annually. The country has halved the poverty headcount since 2000. But 31 per cent of households still lived in extreme poverty by the time of our endline, using the global benchmark of 1.90 USD 2011 PPP per person per day.[8] Even by Ethiopian standards, the households in our study remain extremely poor: at endline, 69 per cent had consumption per person per day below the $1.90 benchmark, and the rest not far above this level.

2.2 Sampling and data collection

We implemented the experiment in 64 villages. To generate this list, we took the Central Statistical Agency's list of 189 rural villages in Doba with a population of 50 to 100 households from the 2007 census and we randomly selected 84 villages. We found 16 screening venues close to those villages: classrooms or agricultural extension facilities with capacity for at least fifty individuals. We selected the 64 villages in our experiment based on logistical considerations, choosing the 64 villages closest to those screening venues such that we had four villages within walking distance of the 16 screening sites. The district is mountainous and remote, so we minimised walking distance to screenings to maximise compliance. Randomisation is within the villages, so this does not affect internal validity, even if the study sample is not representative for the district as a whole. Appendix Figure A.1 shows the 84 villages, marking the 64 selected villages.

Within each village, we compiled a list of all households with the assistance of the community (*kebele*) leader (who runs three to six neighbouring villages). We randomly sampled 18 households from each of the 64 villages to survey, with replacement for households that were away, ill or had just given birth.

The main sample for analysis consists of 1,152 households and 2,112 individuals surveyed at baseline (and any subsequent follow-up) in these 64 villages.[9] We visited villages for the baseline survey and intervention (round 1, between September and November 2010), the midline follow-up survey six months after the baseline (round 2, between March and May 2011), and a long-run follow-up (endline) survey five years after the baseline (round 3, between December 2015 and January 2016). Appendix Figure A.2 shows the timeline of the surveys.

Follow-up surveys were conducted at households' homes by enumerators blind to household treatment status. The household head answered questions on issues like household composition, assets and children's schooling, so most economic variables were collected at the household level. We also collect individual-level information separately from both the household head

8. Data from the World Bank's Poverty and Inequality Platform, https://pip.worldbank.org/.
9. In seven percent of households, the household head is single.

and spouse on psychological variables such as aspirations and preferences, as well as savings. Spouses were interviewed separately, usually by two different enumerators interviewing them simultaneously, either in or around their house. Appendix Section C details the construction of the variables used in our analysis.

Both midline and endline measure aspirations, expectations, other preferences and beliefs, and subjective wellbeing, as well as labour supply, effort and investment in education, savings and use of credit, using the same questionnaires, so these outcomes are consistent across rounds. The endline also captures investment in productive activities and assets and various dimensions of standard of living: food insecurity, consumption expenditure, asset wealth, housing quality, and subjective wellbeing. We collected a shorter set of outcomes in the short run because we did not think other outcomes, such as asset accumulation and consumption, could have changed, as households received no new resources.

2.3 Characteristics of the sample

Table 1 describes the economic lives of our sample at baseline. The sample consists of small farm households, on average 5.6 members. Crop agriculture and livestock-related activities, including product and animal sales, are the main sources of income. Adults spend on average five hours per day on these activities. Livestock holdings are modest, averaging 411 USD PPP per adult-equivalent, equivalent to just over one cow (370 USD PPP).

Holdings of tools are low, at 24 USD PPP per adult. Savings are limited: only 23% have any, with an average of 10.25 USD PPP among savers. Education levels are low, with adult men averaging 3 years and women averaging 1 year of schooling. Most of the generation before the respondents had no education at all: only 16 per cent of the respondents' fathers completed any years of education. Although enrolment levels have increased with free primary education policies, 42 per cent of children aged 7 to 15, school-going age, were not enrolled at baseline.

We split the sample by terciles of the value of durable assets at baseline, a proxy for wealth. This shows the type of investments commonly associated with accumulating wealth in this setting. Our measure of assets is associated with other measures of living standards, such as higher housing quality. In this setting, better-off households invest more in livestock (especially cattle), spend more time in work on their farms, save more, and invest more in education.

3 Motivation for the experiment

This section motivates the core hypothesis of this paper: that increasing aspirations for the future, via exposure to similar role models, increases effort and investment, and, in turn, living standards.

First, we outline theories in social science that argue that aspirations affect long-term decisions, and that aspirations are influenced by the experiences of similar others. In contexts of poverty, it has been argued people are less exposed to others like them who have changed their economic circumstances. As a result, they tend to form lower aspirations, which in turn limits their future-oriented investments. Local belief systems may further entrench low aspirations. We offer anthropological evidence from Ethiopia and elsewhere in line with these conjectures.

Table 1: Economic activities by terciles of durable assets

	(1) Whole sample	(2) Lower tercile	(3) Middle tercile	(4) Upper tercile	(5) p-value	(6) Observations
Assets						
Value of durable assets excluding tools (USD) per ad. equiv PPP	18.72	0.00	5.49	51.25	0.00	1119
Value of tools (USD) per ad. equiv PPP	24.32	16.28	17.63	38.18	0.00	1111
Value of house (USD) per ad. equiv PPP	371.53	228.67	323.08	531.02	0.00	1082
Total savings (USD) PPP	10.25	6.95	11.08	12.14	0.01	2020
Has any savings	0.23	0.22	0.23	0.25	0.15	2048
Livestock						
Value of livestock (USD) per ad. equiv PPP	411.49	281.09	348.13	590.91	0.00	1110
Value of cattle (USD) per ad. equiv PPP	366.87	235.44	296.43	524.95	0.00	1118
Value of sheep or goat (USD) per ad. equiv PPP	39.07	28.96	30.98	54.82	0.00	1118
Labour supply						
Household size	5.61	5.36	6.28	5.30	0.72	1119
Daily minutes in paid work per adult aged above 15	11.68	14.28	9.75	10.11	0.20	1109
Daily minutes on family farm per adult aged above 15	308.90	300.24	306.35	318.95	0.06	1110
Human capital investment						
Share of children at school in the 7-15 age-group	0.58	0.57	0.57	0.61	0.18	802
Schooling expenditure per child aged 7-15 (USD) PPP	17.47	16.92	17.65	18.41	0.53	1110
Highest education level among male adults (years)	3.43	3.02	3.22	4.03	0.00	1045
Highest education level among female adults (years)	1.08	0.88	0.97	1.45	0.00	1025
Respondents' demographic characteristics						
Years of education	1.33	0.94	1.15	1.95	0.00	2048
Listens to radio at least once a week	0.61	0.50	0.49	0.83	0.00	2044
Ever lived outside of current district six months	0.10	0.08	0.10	0.11	0.08	2045
Father has any education	0.16	0.13	0.15	0.20	0.00	2048

Notes: Column 1 is the sample mean. Columns 2-4 are sample means, conditional on the household being in the lower, middle and upper terciles of the value of durable assets (excluding tools) at baseline, an approximation for living standards. Columns 5 reports the p-value from a t-test of equality between the mean of the lower and upper tercile. Columns 6 reports the number of observations. Variables are measured at the household level at baseline, except savings and demographic characteristics, which are measured for both the household head and spouse. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile. Variables are defined in detail in Appendix C. The OECD adult equivalence scale gives weight 0.5 to each individual younger than 16 and weight 0.7 to all other adults that are not the household head. Durable assets include radios, mobile phones, jewellery, and furniture. Tools include ploughs, hoes, axes. The value of house is how much their house would cost to build today (in current prices), including materials and labour costs. Total savings refers to the value of savings held inside and outside the home. Livestock and durable assets are valued using self-reported hypothetical sale prices. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using the national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1.

Second, we sketch a simple economic model that defines aspirations in economic terms and show how an intervention that increases aspirations might affect economic decision-making. Appendix A offers a plausible formal treatment to derive predictions. Third, we describe our measures of aspirations. We find that in our specific poor and remote setting, poorer people in our sample have lower aspirations and less exposure to experiences through which they might form higher aspirations.

3.1 Aspirations: concept, origins, and implications

The role of aspirations in decision-making: Theories in social sciences argue that aspirations play an important role in long-term decision-making. Social psychologists theorise that aspirations provide motivation and regulation for current choices, including inspiring persistence through setbacks, and that achieving aspirations gives satisfaction (Bandura, 2001; Fishbach and Ferguson, 2007; Locke and Latham, 1990). In social anthropology, seminal work by Arjun Appadurai (2004) argues that aspirations profoundly shape people's trajectories, defining "the capacity to aspire" as a "navigational capacity": the ability to read and navigate

"a map of a journey into the future" (p. 76). Aspirations hence influence whether individuals consider or dismiss entire classes of options.

Social influences impact aspirations: Aspirations are influenced by both social processes and individual experiences (Hoff and Stiglitz, 2016). In psychology, aspirations are "socially learned": people observe the outcomes of other similar individuals and draw conclusions about what is possible for them (Bandura, 1977). Anthropological accounts highlight the role of exposure to how others in one's network have navigated their life choices (Appadurai, 2004; Willis, 1977; Wilson, 1987). In economics, Genicot and Ray (2020) survey how social influences may determine aspirations. The implication is that if people have not encountered role models similar to them who have changed their economic circumstances via their own actions, they may struggle to believe that people like them can achieve such changes. This motivates our intervention.

Aspirations in states of poverty: Anthropologists emphasise that high levels of poverty can result in distorted or limited aspirations, because the social processes through which individuals form aspirations differ radically with wealth. Appadurai (2004) argues:

> "The more privileged in any society simply have used the map of its norms to explore the future more frequently and realistically [...]. The poorer members [...] have a more brittle horizon of aspirations." (p. 69).

Richer people form higher aspirations because they have broader networks with more diverse experiences and more opportunities for experimentation enabled by more material resources.

Aspirations and belief systems: In contexts of existing deprivation, normative belief systems may entrench lower aspirations, limiting future orientation and economic progress. These belief systems shape how society allocates economic resources and opportunities, reinforcing existing structures through limiting any space for change (Durkheim, 1893). For example, zero-sum beliefs – that gains for one individual come at the expense of others – can lead to norms and values that depress effort and to lower welfare outcomes (Foster, 1965; Carvalho et al., 2023).[10] Fatalistic beliefs have been documented in poor communities across diverse societies (Elder, 1966; Ingersoll, 1966; Díaz et al., 2015; Whelan, 1996). These belief systems can shape people's mental models of what is possible for them, conditioning their aspirations (Hoff and Stiglitz, 2016; World Bank, 2015).

Anthropological accounts of low aspirations in rural Ethiopia: Scholars have commented on distorted or limited aspirations in deprived Ethiopian communities (Levine, 1965; Kebede, 1999; Wollie, 2009). Rahmato and Kidani (1999) documents Ethiopian proverbs such as "we live only for today", "this is a life of no thought for tomorrow", or "we have neither a dream nor an imagination" highlighting helplessness, fatalism and a lack of hope.

The role of local belief systems: While not unique or especially pronounced in Ethiopia,

10. Such ideas are also common in the West: before the enlightenment, change and progress were seen as a threat in Europe (Mokyr, 2016), and zero-sum thinking and its historical roots can help to explain the present-day US political divide (Chinoy et al., 2025).

certain belief systems conceptualise destiny as determined by divine forces rather than individual agency, constraining the scope for goal-setting and imagining alternative futures (Levine, 1965; Kebede, 1999; Wollie, 2009). The worldview prevalent in the study region emphasises deference to authority, tradition and social hierarchy; social conformity and cohesion; resistance to external influence (Alamayo, 2021).[11]

Evidence on belief systems that may condition aspirations: In our sample, we find quantitative evidence of fatalistic, deterministic belief systems. Our sample scores twice as high as Western samples on the classic Levenson (1981) measure of fatalism. On a scale capturing perceptions of the causes of poverty (Feagin, 1975), our sample are more likely to attribute poverty to structural or fatalistic explanations than Western samples (Hunt, 2004). In our sample, both measures of fatalistic beliefs are higher among poorer people.

3.2 Economic conceptual framework

A simple economic model of aspirations: In Appendix Section A, we provide one way to describe the role of aspirations in economic choices formally. The core idea is that goals and aspirations regulate behaviour, so dampened aspirations can reduce motivation for effort and investment. In line with other models (Dalton, Ghosal, and Mani, 2016; Genicot and Ray, 2017, 2020), we introduce a behavioural constraint through a reference point as a goal that incentivises investment and effort (see also Kőszegi, 2010). Effort and investment increases in the level of aspirations.[12]

The experiment within the conceptual framework: Our experiment actively tries to shift aspirations by providing exposure to role models from similar communities, who through their own agency created a different future. We aim to promote positive deviance toward change (as in Durkheim, 1893) but, by using local role models, we encourage avenues that are achievable and acceptable within local society.[13] We model an increase in aspirations as a shift in the reference point, leading to immediate increases in effort and investment. This increase in aspirations could be seen as a change in the mental model, offering a new point of reference of what one wishes to become or achieve (Hoff and Stiglitz, 2016), though other theoretical framings are also possible. Any changes in effort, investment and other outcomes are expected to occur through changes in aspirations, not alternatives such as time or risk preferences, or information transmission. This can be tested empirically.

Change dynamics: The extent and speed of changes will be an empirical issue. We would not expect large changes: the underlying resource constraint and social context of these

11. In the traditional Waaqeffannaa religion historically followed by many Oromo (the largest ethnic group in the study area), individuals are encouraged to uphold Safuu, the moral and ethical order, which governs relationships between humans, nature, and God (Waaqa). This religion has blended with ideas of predestination in local variants of Orthodox Christianity (*Fekade Egziabher*) and Islam (*Qadar*) (Gemeda and Rajani, 2018).

12. Our conceptual framework remains agnostic about where aspirations stem from, although the literature above highlights the role of limited social exposure, societal and religious influences, and poverty.

13. We aim to avoid negative outcomes from boosting aspirations to unrealistic levels, as discussed in Appadurai (2004), Genicot and Ray (2020) and McKenzie, Mohpal, and Yang (2022).

households have not changed. Changes in aspirations may not be immediate. People may need to experiment with possible alternative futures (Appadurai, 2004) or may gradually learn-by-doing when demotivating beliefs start to shift (Carvalho et al., 2023). If small initial changes in aspirations lead to small changes in wealth, these wealth increases may in turn increase aspirations (Dalton, Ghosal, and Mani, 2016; Kahneman, Knetsch, and Thaler, 1986). Learning from others through spillovers may also occur (Genicot and Ray, 2020).

Plausible outcomes of the experiment: We would expect treated households to put in more effort and investment to build a better future. We therefore measure whether the intervention changed plausible investments in this context: working longer hours on the farm, adopting modern farm inputs such as seed, fertiliser, or veterinary supplies, or diversifying more into livestock. Investment in education can also be seen as an investment in old age security (Nugent, 1985, Rossi and Godard, 2022, and specifically for Ethiopia, Woldehanna et al., 2008 and Crivello and van der Gaag, 2016).[14] Table 1 showed that better-off farm households have higher levels of all these investments.

We would expect people to be better off from these actions. However, how that would manifest itself is less self-evident. We measure consumption expenditure, purchase of assets such as durables and tools, housing quality and subjective well-being. We would expect that well-being to go up. Whether consumption would go up is more ambiguous *a priori*.Perceived well-being could increase without higher consumption, as households continue to move resources into the future, either via assets such as durable goods or housing, that offer a flow of consumption into the future, or via productive investments such as in tools, livestock or agricultural productivity. Formally, in the model in Appendix Section A, observed consumption in a particular period depends on the intertemporal substitution elasticity, as is standard when incentives to move resources to the future are increased.

3.3 Measuring aspirations in rural Ethiopia

From concept to measurement: To assess whether the experiment boosted aspirations, we must quantify levels of aspirations using measures that can be compared across respondents. To do so, we developed and validated survey measures of aspirations (Bernard and Taffesse, 2014). These capture the level of income, assets, or children's education individuals hoped to achieve in their lifetime. To measure each dimension of aspirations, respondents were asked "*What is the level of [X] that you would like to achieve?*" where *[X]* was: (i) annual income (from all agricultural and non-agricultural activities, or social protection programmes); (ii) value of assets (including house, furniture, consumer goods like a TV and fridge, and any vehicles); or (iii) oldest child's education. We select these domains because they are relevant to most respondents and many described an improved life in terms of advancements in these areas.[15]

14. To quote one father "If you study well, you can improve. I want [my children] to reach a better place through their education. If they [do], they can support me" (Crivello and van der Gaag, 2016, 18).

15. To help respondents conceptualise the level they aspired to, they were first asked "*What is the level of [X] you have at present?*". Respondents are able to estimate current assets and income. Their responses to our

Table 2: Aspirations by terciles of durable assets

	(1) Whole sample	(2) Lower tercile	(3) Middle tercile	(4) Upper tercile	(5) p-value	(6) Observations
Aspirations: what would you like to achieve?						
Aspired income (USD) PPP	22382.36	18012.02	21106.74	27358.05	0.00	2017
Aspired wealth (USD) PPP	12816.36	9143.45	11319.29	17365.90	0.00	2025
Aspired education (years)	14.06	13.90	13.88	14.39	0.00	2000
Expectations: what do you think you will achieve?						
Expected income (USD) PPP	5079.67	4507.98	4695.10	5962.70	0.00	2014
Expected wealth (USD) PPP	5034.33	3981.22	4687.73	6084.35	0.00	2011
Expected education (years)	13.49	13.30	13.28	13.86	0.00	1926

Notes: Column 1 is the sample mean. Columns 2-4 are sample means, conditional on the household being in the lower, middle and upper terciles of the value of durable assets (excluding tools) at baseline, an approximation for living standards. Columns 5 reports the p-value from a t-test of equality between the mean of the lower and upper tercile. Columns 6 reports the number of observations. Variables are measured at baseline for both the household head and spouse. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile. Variables are defined in detail in Appendix C. To measure aspirations, respondents are asked the levels of outcomes they would like their household to achieve, on three dimensions. Annual income is the amount of cash income the household earns from all agricultural and non-agricultural activities in a year. Wealth is durable wealth (including housing, vehicles, furniture and other valuable durables). Aspired education is measured as the 'years of education that you would like your oldest child to achieve'. Expectations are measured as the levels the respondent expects to reach in ten years, on the same dimensions. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using the national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1.

Reported aspirations: On these measures, respondents aspire to improve their current position over their lifetime (Table 2). Although the sample mean aspirations appear high, they may be reasonable given Ethiopia's rapid economic growth during the study period, discussed above. In 2020, national GDP per capita implies that a family of 5.6, the average in our sample, would have an annual household income of $14,522 in 2016 USD PPP. Households aspire to wealth levels over their lifetime just below that, and to have 1.5 times that national income level.[16] For the level of children's education, 60 per cent of respondents aspire for their oldest child to go beyond secondary school, with the remainder expecting them to complete secondary school or less. Are these realistic levels? That is harder to assess. Fruttero, Muller, and Calvo-González (2025) found in a recent review of aspirations studies that asking these questions in an "unconstrained" way as "desired" levels tends to get relatively high answers (see also La Ferrara, 2019). In line with this, we find that positive outliers strongly influence these large mean values. Indeed, once we look at median levels, a plausibly more realistic picture emerges: aspirations for household wealth and income are both $4,488 in 2016 USD PPP. This is roughly a third of average household income nationally, calculated from GDP per capita. Median aspirations gaps (aspiration level minus current level) are 55 per cent and 58

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.

16. As a benchmark, a large lump sum cash transfer from the NGO GiveDirectly increases annual household consumption by 75 per cent.

per cent of these median wealth and income aspirations respectively: in short, these aspirations seem reasonable, and not too distant from current levels.

Aspirations versus expectations: As a further sense check, we compare aspirations to measures of expectations and of the maximum people believe someone in their village could earn. We directly elicit respondents' expectations in the same domains, asking what they think they will reach in ten years instead of what they would like to achieve over their lifetime. As expected, expectations are lower than aspirations in all domains: average income (wealth) expectations are roughly one quarter (one third) of aspirations, while respondents aspire to their child having at least a year more of education than they expect. Measures are correlated ($\rho = 0.42$ in the indices at baseline). As intended, our measures of aspirations conceptually capture something close to the upper limits of outcomes respondents believe are possible. Respondents' aspirations exceed but remain close to their beliefs about the highest attainable outcomes someone in their village could have at the time of the survey, further suggesting aspirations are somewhat reasonable: the median ratio of own aspirations to this village maximum is 1.3 for assets and 2 for income. Thus our measures are related in sensible ways to more standard economic measures of expectations, for respondents themselves and for others.

Reliability tests: In Bernard and Taffesse (2014), we conducted a measurement study to establish reliability of measures. We repeated measurement of income, wealth and education aspirations in our pilot setting with the same respondents two weeks apart. Measures had correlation coefficient ranging from 0.77-0.98 within-respondent, provided experienced enumerators were used, which we subsequently did in this study. These reliability correlations are in line with Angrist and Krueger (1999)'s benchmarks for reliable income and education measures.

Aspirations and their correlates: Our aspirations measure also correlates in expected ways with other measures in our sample. In line with anthropological accounts of aspirations, aspirations levels are strongly correlated with wealth. Aspirations for income, wealth and education are all significantly higher for relatively better off households (Table 2, Column 5). The mean income (asset) aspiration in the upper tercile of households by wealth is 1.5 (1.9) times that in the lower tercile.[17] Also in line with theory, opportunities which might increase aspirations are correlated with wealth: education, father's education, living outside the district and listening to the radio are correlated with wealth. Unsurprisingly, a simple regression finds these characteristics all predict aspirations. These associations suggest, but do not prove, that aspirations are related to exposure to opportunities and role models. For this we turn to our causal design.

4 Experimental design and estimation strategy

4.1 Content of the video intervention

In psychology, social learning theory suggests that people often adjust their goals or aspirations through "vicarious experience": impactful moments that allow them to imagine what

17. We find similar differences across wealth terciles in the "gap" between aspirations and the current level reported in each of the dimensions.

it might be like to live someone else's life (Bandura, 1977). These experiences frequently arise when engaging with stories, which foster a sense of identification between the audience and the characters. The audience is "transported" into the narrative, envisioning themselves as the character they observe (Cohen, 2001, p. 251) (see also Green and Brock, 2000).[18] Role models are most inspiring when their success feels attainable to the audience, because role models are similar to them, and when role models demonstrate the steps to achieve success (Lockwood and Kunda, 1997; Marx and Ko, 2012).

Our intervention consisted of inviting randomly selected households to a screening session to watch four short documentaries narrating motivational life stories of real people, from a similar socio-economic background to the study participants. We worked with an Ethiopian production company to produce the films. The role models in the films improved their economic circumstances through hard work and by setting, working towards, and achieving goals. Each documentary is 15 minutes long and in Oromiffa, the language in the study site. Two stories are about male and two about female role models. Appendix Section B summarises two documentaries, the stories of Teyiba Abdella and Bashir Malim Yisak, and one placebo segment.[19]

The documentaries had four common themes intended to make audience members re-evaluate their own aspirations. First, they emphasise the importance of working hard. For example, Teyiba describes the start of her flour-trading business by saying "In those days I used to work like a donkey...carrying heavy loads on my back." Second, the role models highlight the importance of setting goals and planning, emphasising that progress is slow and they had to persevere through setbacks. Beshir describes how he made detailed plans over three years to save for and purchase his water pump. Both he and Teyiba describe future goals they are working towards, such as building a house or moving to the nearby town. The films vividly document the emotions accompanying both setbacks and success. Third, depicted individuals take actions which are possible for the audience and relatively normal for the villages where documentaries were screened, such as installing irrigation, or starting a shop. The characters succeeded largely through their own efforts, with some assistance from community members. For example, Beshir receives help from the local extension agent in purchasing a pump from Addis Ababa, although he saves and pays for it. But no role model relied on external support that would not be available to the audience. Any concrete information in the videos was unlikely to be new for viewers, although the documentaries may have made existing information more salient, which we test for in Section 5.2.3. Fourth, all the subjects take slightly different, "deviant", courses of action to those around them: starting a small business, diversifying their source of income, or improving their farming practices.

We selected the subjects of the documentaries by inviting agricultural extension agents

18. In economics, La Ferrara (2016) and Mani and Riley (2021) summarise recent examples of video-based narratives that aim to shift behaviour.

19. The documentaries, with English subtitles, and one of four placebo segments are available at https://www.youtube.com/channel/UCqfoNjCzt8YPjTRWQaMQfAg.

and NGO staff in other districts in the region to submit descriptions of life stories of potential role models who lived in their area.[20] All the subjects were ordinary rural residents, very similar to their audience, who were either initially poorer than those around them or of similar socio-economic status, so their achievements would seem attainable to our sample. In the six-month follow-up, 52 per cent of audience members thought the documentary subjects had initially been worse off than they currently were. However, 73 per cent said that the documentary subjects eventually became better off than they were currently.

At the time of the screening, being shown a video in a group may have been a rare event in these villagers' lives. To account for potential changes as a result of the screening alone, we also invited another group of households, in the same villages, to a "placebo" screening of an Ethiopian comedy TV show about rural life. The placebo consisted of four 15-minute segments of the comedy TV show that we selected for its entertainment value only.

4.2 Randomisation and compliance

We randomly allocate the 18 randomly selected households in each village to treatment, placebo or within-village control groups. Our main comparison, of the treatment households with the control group, identifies the effect of the aspirations intervention. Comparing the placebo to the control group examines if there is any effect of exposure to video media and outside facilitators if the media content doesn't target aspirations. We find little evidence of placebo effects.

Treatment was assigned at the household level. Both the household head and spouse received the same treatment and were invited to the same videos.[21] At the end of their baseline interview, each household head and spouse in treatment and placebo households received tickets for a screening session at a particular date and time in a few days time. Respondents were told that the screening was an entertainment show, tickets were non-transferable, and they could only attend the screening at the time and place on the ticket.

Screenings occurred in farmers' training centres or schools. As described in Section 2.2, we had 16 screening venues, with each venue located to be close to four of the villages: farmers' training centres or schools are usually in between villages to ensure maximum accessibility. We conducted screenings for the treatment group from four villages at once, and then separately for the placebo group from those four villages. Screenings for treatment and placebo groups were conducted at the same screening venue on the same or adjacent days, with no fixed order – neither group consistently viewed the screening before the other.[22] We used a projector and speakers connected to a power generator. On average, people walked 29 minutes to the

20. It was almost impossible that respondents would know anyone in the videos and there is no evidence that this happened.

21. 95 individuals were single or widowed so the household was only given one invitation.

22. Scheduling minimised interaction between treatment and placebo groups by ensuring that one group was either completing a post-screening survey or had left before the other arrived at the venue. A separate team administered the same survey to the control group simultaneously at their homes.

screening venue, so people not invited to the screenings were unlikely to walk to the centre and were not allowed entry without a ticket. Rooms were shuttered to ensure higher quality screening, which further ensured that no-one could watch from outside.

At the screening venue, facilitators checked farmers' identity and the date and time of the ticket before allowing entry. Compliance with our household-level randomisation was high. Only 2 per cent of the surveyed individuals or households did not comply with treatment allocation, by either missing their screening or going to the wrong one (Appendix Table A.8). There are no differences in compliance rates across treatment and placebo groups.

The design assumes no spillovers from the treated or placebo groups to the control group. In Section 5.5, we describe an additional "pure control" sample of non-treated villages we use to test for spillover effects. We find no systematic evidence of large spillover effects, so focus on the within-village randomisation for our main results.

4.3 Balance and attrition

Appendix Tables A.9 and A.10 present balance tests for individual and household variables, for both demographic characteristics and outcome variables measured at baseline. We present means for the entire sample present at baseline, including attriters, and separately for participants still present after five years, excluding attriters.

In general, we find balance across groups. We run various omnibus tests following Kerwin, Rostom, and Sterck (2024) over all individual level variables, all household level variables, and all baseline variables in Tables A.9 and A.10. We cannot reject the null hypothesis that our sample is balanced across groups. Results are consistent at baseline and endline, possibly because attrition is low.

In variables captured at the individual level (Table A.9), the only imbalance is in the expectations index, at the 10 per cent level. In variables at the household level (Table A.10), there are imbalances in the number of children. There is balance in overall household size, but the composition of households across age cohorts differs slightly by treatment. Treated households have slightly more children aged 7–10 and 11–15 and slightly fewer aged 0–6 than the other groups; differences are statistically significant at the 5 per cent level, except for the youngest age group. This imbalance appears in other variables linked to the number of children: the number of children in school and total minutes in school and studying across all children. In addition, treated households are more likely to have a non-organic roof (differences across groups are significant at the 5 per cent level) and have slightly more livestock (significant at the 10 per cent level). Our main specifications and robustness checks, described in the next section, add the baseline value of the outcome to account for imbalances.

Attrition is low. Most variables are measured at household level; 96 per cent of households present at baseline are found again after five years, which is notably high compared to other long-run follow-ups of randomised control trials in development economics (Bouguen et al., 2019). Similarly, 94 per cent of baseline respondents are re-interviewed after five years.

Few observed demographic characteristics, if any, predict attrition across any follow-up rounds (Appendix Table A.11). No observed demographic characteristics predict a household attriting across any follow-up rounds. A joint F-test fails to reject the null that any covariate, on its own or interacted with treatment and placebo indicators, explains attrition. Those invited to the documentary screening are slightly more likely to respond in the five-year follow-up, but this effect is only significant at the 10 per cent in the specification without covariates.[23]

4.4 Empirical strategy

Our main specification is:

$$y_{vi} = \delta T_{vi} + \rho P_{vi} + \theta y_{vi0} + X'_{vi0}\pi + \alpha_v + \varepsilon_{vi} \tag{1}$$

where y_{vi} is a household-level outcome for household i in village v, $T_{vi}=1$ if a household was invited to watch the documentary, $P_{vi}=1$ if they were invited to watch the placebo movie and the omitted category is within-village control households. α_v is a village-level fixed effect and X_{vi0} is a pre-specified vector of controls measured at baseline: the age, gender, marital status and highest school grade completed of the head of the household. We also control for the baseline value of the outcome y_{vi0}, when we collected data on it, making this an ANCOVA specification. We collect baseline values for all outcomes except input use and consumption expenditure. ε_{vi} denotes the heteroskedasticity-robust error term.[24] Continuous outcomes are trimmed above the 99th percentile.

For aspirations, respondents' time-use, beliefs and preferences, which we observe separately for household heads and spouses, y_{vi} is an individual-level outcome, and we control for age, gender, marital status and education of each individual. Standard errors for these outcomes are clustered at the household level, the unit of randomisation.

We use Equation (1) to test our predictions. We examine if, after five years, our intervention increased effort and investment in productive activities and assets; or effort and investment in education. We then analyse how this impacts various dimensions of standard of living. We capture impacts on food insecurity; consumption expenditure; ownership of "non-productive" durables; housing; and subjective wellbeing. We test whether the intervention works via increasing aspirations or if alternative mechanisms, such as risk and time preferences, information transmission, beliefs about returns to one's own effort, or beliefs about the role of fate and chance in outcomes play a role. Finally, using the shorter six-month follow-up, we examine if some economic behaviours and mechanisms had already changed soon after intervention.

Multiple inference adjustments: We adjust naive p-values as follows. We group related variables within table panels. A table panel corresponds to a concept in our conceptual

23. When estimating Lee (2009) bounds to assess robustness to differential attrition, our main results remain similar in magnitude and significance, assuming attrition is monotonic in potential outcomes (results available on request).

24. Results are broadly robust to clustering standard errors at the village level.

framework, such as labour effort, agricultural investment, productive assets, non-productive durables, housing assets, or consumption. We view the group of variables within a panel as testing the same underlying hypothesis. To correct for multiple testing, we use the Benjamini, Krieger, and Yekutieli (2006) resampling procedure to calculate sharpened q-values which correct p-values for multiple tests across outcomes within each panel.

To summarise impacts, we report impact estimates on standardised inverse-covariance-weighted indices (Anderson, 2008) constructed from all outcomes reported in the primary results tables. This results in five main indices, capturing agricultural investment, educational investment, welfare, aspirations, and expectations. Following Kling, Liebman, and Katz (2007), we also aggregate the standardised indices into a single omnibus index.

Prespecification and robustness tests: We pre-registered analysis for the five-year follow-up in February 2017, after data collection was completed but before analysis.[25] There was no preregistration for the analysis of six-month follow-up outcomes, reported on in Bernard et al. (2014), as this was conducted before pre-analysis plans (PAPs) were widely used in economics. However, we committed to our core hypothesis – that the role model videos could increase future-oriented investment via increasing aspirations – via the experimental design. Aspirations were the only measure we collected directly after the screening, as well as in later rounds. Working papers reviewing the theoretical literature and validating the aspirations survey measures were published in early 2011, both pointing to the experiment.[26]

The PAP for the five-year follow-up commits to analysis of all outcomes in the six-month follow-up, plus additional ones. The hypotheses and outcome construction in this paper are mostly consistent with the PAP. In Appendix Section D we list a few deviations from the PAP and explain the reasons for them. Between Appendices D, E and F, we report all the main results as pre-specified. Broadly, results change little due to deviations from the PAP.

The first and main deviation from the PAP is that we report ANCOVA results for those variables for which we collected baseline values (i.e. all variables except input use and consumption). Our pre-specified main specification did not control for the baseline value of the outcome. The pre-specified approach increases variance but is consistent across all outcomes, given that for some variables we did not collect baseline data. We had pre-specified running an ANCOVA specification, but as a robustness check. Our main robustness tables in Appendix E show results are mostly robust whether or not an ANCOVA specification is used.

In these tables, we also report Equation (1) but allowing for selection of controls via post-double-selection LASSO (PDS Lasso) (Belloni, Chernozhukov, and Hansen, 2014), where all variables in Tables A.9 and A.10 can be selected as controls. These variables include pre-specified controls X_{vi1}, other demographic variables, and baseline values of all outcomes. Any baseline

25. See `https://www.socialscienceregistry.org/trials/1483` for the trial registration.

26. The earliest versions of these papers are at `https://assets.publishing.service.gov.uk/media/57a08ac840f0b652dd0008d8/csae-wps-2011-03.pdf` (February 2011) and `https://essp.ifpri.info/files/2011/04/ESSP_WP47_MeasuringAspirations.pdf` (April 2011) respectively. The validation survey occurred in 2009.

variables that explain variation in a given outcome, including variables imbalanced at baseline, will be controlled for. In the text, we note a few places where results differ slightly between the ANCOVA, pre-specified and PDS Lasso specifications, but results are largely robust.

Relative to the PAP, we also make some adjustments to our multiple inference adjustment approach in line with current practice. We pre-specified the omnibus index, and the hypotheses related to the summary indices, but had not specified construction of the summary indices. We also make a few changes to our groupings of variables for multiple inference adjustment. We present the main results as pre-specified in Appendix Section D to show these changes do not affect our substantive conclusions. We discuss other minor changes relative to the PAP in Appendix Section D: we adopt a more standard trimming strategy, make small adjustments to six variables (noted in footnotes), and add seven variables (we note if variables are not pre-specified).

A final robustness check uses an alternative control group to test for spillovers, finding little evidence of these (see Section 5.5 and Appendix F).

5 Results

This section presents results five years after the intervention, based on the predictions from our conceptual framework. We then discuss results on a smaller subset of outcomes collected after six months and effects on psychological mechanisms which might explain effects.

All tables follow the same structure. They present estimates of the parameters in Equation (1): δ (Column 1), ρ (Column 2), a test for $(\delta - \rho) = 0$ (Column 3), and the mean of the dependent variable in the control group (Column 4). A significant treatment effect compared to the control group (Column 1) indicates that the intervention had an overall impact. A significant placebo effect (Column 2) indicates that attending a screening alone had an effect. We report robustness tests in Appendix Section E. We note in the main text where results differ in magnitude or significance in the robustness tests. Reported results are robust to correction for multiple hypothesis testing unless noted.

5.1 Effects on economic outcomes five years after the screening

5.1.1 Effort, investment and productive assets

In Table 3, we show that the intervention improved effort and investment in productive activities in these communities, notably related to agriculture. We measure time allocation at individual level on a typical day in the most recent October, the time of harvest. After five years, treated household heads and spouses work significantly more than both the control and placebo group (top panel). The effect is equivalent to about 8.6 per cent of the control mean, or nearly half an hour a day per spouse. As most households have one female and one male adult member, this is roughly an hour for all adult members per day. The increased work effort comes at the expense of leisure, which decreases by 5.9 per cent or nearly 50 minutes per day per spouse.[27]

27. Results are robust to alternative specifications (Appendix Table A.19).

The treatment increased investments in modern inputs, especially on the extensive margin (whether or not the household spent any resources on these inputs) (second panel, Table 3). Treated households are 10 percentage points more likely to have invested in modern agricultural inputs like improved seeds and inorganic fertilisers than the control group and 10 percentage points more likely to have invested in modern livestock inputs like feed or vet supplies. For modern agricultural inputs, this translates into a 22 per cent increase in overall spending (intensive margin) compared to the households in the control group, but we cannot reject the absence of difference with the placebo group. For livestock inputs, intensive margin effects are positive but not significant.[28]

We find effects on labour used on the family farm consistent with our earlier results on effort. We measure days of hired and family labour used in the most recent long rains in the agricultural inputs module. Treated households report using more family labour days on agriculture than the control group, consistent with results from the time allocation module.[29] They are less likely to hire any non-family labour in crop cultivation activities relative to the control group at the extensive margin. There is no change in land area under cultivation, potentially because land is allocated by local authorities with no possibility to buy or sell land, while rental markets remain limited despite recent legal changes.[30]

The third panel of Table 3 explores the intervention's impact on household productive asset holdings, defined as those that may be used in agriculture or businesses. Treated households have higher values of productive tools compared to control and placebo households, significant relative to controls.[31] The value of their livestock holdings are about 5 per cent higher than in the control and the placebo group, but the difference is not statistically significant.[32]

We combine all outcomes from the first three panels of Table 3 into a single agricultural investment index. Treated households significantly increased this index of investment by 0.21 and 0.18 standard deviations relative to the control and placebo group, five years after exposure to the role models in the videos. Results on the index are robust across specifications. Overall, the results in Table 3 are consistent with our conceptual framework.

28. The outcomes in this panel were not measured at baseline so this is not an ANCOVA specification. The results remain robust when using PDS Lasso, except the difference in the share of households adopting modern crop inputs relative to the placebo group declines from 6 to 4 percentage points and moves from marginal significance (at the 10 per cent level) to not being statistically significant (Column 6, Appendix Table A.19). The extensive margin analysis was not pre-specified.

29. However, this effect is not robust to multiple hypothesis testing, and the difference relative to the placebo is not significant. Results are positive but not significant in the PDS Lasso specification.

30. For a discussion of the legal frameworks, see Deininger, Ali, and Alemu (2008). We do not display a pre-specified variable studying land area rented or sharecropped as there was very little variation. Only 14 households rented any land in and four households rented out any land at the five-year follow-up.

31. Results are robust to alternative specifications (Appendix Table A.19).

32. Results are similar but marginally significant without controlling for the baseline value of the outcome relative to the placebo, while coefficients shrink but remain positive in the PDS Lasso specification (Appendix Table A.19). We had pre-specified grouping these asset variables with other assets in Table 5 but now group them here to distinguish productive assets – consistent with future-oriented investment – from other non-productive durable assets.

5.1.2 Educational investments

In poor settings like in this part of Ethiopia, investing in children as old-age security (Nugent, 1985) is still common (see Rossi and Godard, 2022, and Woldehanna et al., 2008 for a discussion on Ethiopia). It is plausible that households consider it part of their investment portfolio, especially for their long-term future, given the lack of pension options. We assess education-related investments and outcomes through enrolment and grade attainment, time in school and studying, and school-related expenses.

We examine school-related expenses at household level. For other education-related outcomes, we look at two cohorts of children. These cohorts were pre-specified and correspond to the primary and post-primary school-going ages at the time of our five-year follow-up (see Appendix Figure A.2 for a timeline). "Cohort 1" are aged 16 to 20 (post-primary school-going age) at the five-year follow-up and 11 to 15 (upper primary school-going age) at the time of the intervention. "Cohort 2" are aged 7 to 15 (primary school-going age) during the five-year follow-up and 2 to 10 during the intervention.[33] We study all households in the sample, including 71 households without children in this age range, to make sure our results are comparable with our other findings.

The intervention increased investment in children's education among children of post-primary school-going age at the five-year follow-up ("Cohort 1"). These children were all of school-going age at intervention. The first panel of Table 4 shows the treatment increases the number of children in a household aged 16 to 20 enrolled in school at endline by 35 per cent relative to the control group. In the control group, 0.17 children in this age group per household are enrolled, compared to 0.23 children per households in the treatment group. The treatment effect compared to the placebo group is not statistically significant, but of similar magnitude.[34] Children aged 16 to 20 in treated households spend, on average, 33 minutes more per day attending school than the control group (31 minutes relative to the placebo group) and 9 minutes more studying (although differences are only significant relative to the control group).[35] Most notably, there is an increase in education attainment in this group: in the control group, 0.07 children in this age group per household have completed primary school, compared to 0.16 children per households in the treatment group.[36] The increase in attainment is nearly a doubling relative to the placebo and control group, albeit from a very

33. We pre-specified studying children from age 6, not age 7, but correct this to align with the age at which children start school.

34. Results are mostly robust across the ANCOVA and pre-specified specifications, except the treatment vs placebo effect on the number of children aged 16-20 in school moves from significance at the 10 per cent level in the pre-specified specification to not being statistically significant in the ANCOVA specification. Coefficients shrink slightly and lose significance in the PDS Lasso specification (Appendix Table A.20). Results on enrolment for both cohorts are also robust to an alternative specification at the individual household-member level using the probability of being enrolled as the outcome, with the individual's age as a control.

35. Results on time in school are robust across all specifications; results on time studying are not robust in the PDS Lasso specification (Appendix Table A.20)

36. This result is robust across all specifications (Appendix Table A.20). This variable was not pre-specified.

Table 3: Effort, investment and productive assets

After five years	(1) Treatment	(2) Placebo	(3) Treat. vs. placebo	(4) Control mean (SD) Total obs.
Household head and spouse's labour effort:				
Daily minutes working	28.01***	8.87	19.14**	326.16
	(7.80)	(7.97)	(7.95)	(200.01)
	[0.00]***	[0.26]	[0.03]**	1754
Daily minutes in leisure	-48.62***	-30.06***	-18.55*	830.59
	(9.98)	(10.41)	(10.32)	(187.38)
	[0.00]***	[0.01]***	[0.07]*	1773
Agricultural investment:				
% with any spending on modern crop inputs	0.10***	0.04	0.06*	0.58
	(0.03)	(0.03)	(0.03)	(0.49)
	[0.01]**	[0.58]	[0.29]	1089
Spending on seed or fertiliser (USD) PPP	7.33**	3.80	3.53	33.49
	(3.07)	(3.32)	(3.31)	(43.54)
	[0.05]**	[0.58]	[0.38]	1078
% with any spending on feed or vet supplies	0.10***	-0.04	0.14***	0.45
	(0.03)	(0.03)	(0.03)	(0.50)
	[0.01]**	[0.58]	[0.00]***	1089
Spending on feed or vet supplies (USD) PPP	2.68	-1.84	4.52	29.30
	(4.81)	(4.81)	(4.63)	(70.92)
	[0.66]	[0.91]	[0.38]	1081
% with any spending on hired crop labour	-0.05**	-0.02	-0.03	0.36
	(0.02)	(0.02)	(0.02)	(0.48)
	[0.05]**	[0.58]	[0.38]	1089
Spending on hired crop labour (USD) PPP	-1.30	-4.97	3.67	54.16
	(5.45)	(5.51)	(5.42)	(93.01)
	[0.81]	[0.59]	[0.50]	1078
Value of family crop labour (USD) PPP	33.33*	1.27	32.06	387.81
	(19.73)	(19.39)	(20.08)	(258.03)
	[0.15]	[0.95]	[0.29]	1079
Area cultivated (hectares)	0.01	-0.01	0.02	0.55
	(0.02)	(0.02)	(0.02)	(0.30)
	[0.65]	[0.91]	[0.38]	1071
Assets:				
Value of livestock (USD) PPP	112.75	23.24	89.51	2018.22
	(122.97)	(117.11)	(114.28)	(1921.09)
	[0.36]	[0.84]	[0.51]	1053
Value of tools (USD) PPP	21.27**	12.77	8.50	106.02
	(10.77)	(12.30)	(12.87)	(126.90)
	[0.10]*	[0.60]	[0.51]	1049
Summary index:				
Agricultural investment index	0.21***	0.03	0.18***	-0.00
	(0.06)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.96]	[0.01]***	1082

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences between parameters reported in the first two columns. The comparison group is households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. Standard errors are in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each group (panel) of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. All columns control for village fixed effects and characteristics of the household head (or the individual respondent for effort outcomes): age, years of education, an indicator for being single, and an indicator for being male. Regressions control for baseline outcomes, except for agricultural investment. Variables are defined in Appendix C. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous outcomes above the 99^{th} percentile. For the first panel, the unit of observation is the individual household head or spouse for effort. Time in work and leisure is reported for a typical day in October by the household head and expressed per adult. For the bottom three panels, the unit of observation is the household. Heteroskedasticity-robust standard errors are reported, except for effort where they are clustered at household level. Crop inputs include seeds, fertilisers, and pesticides. Livestock inputs include animal feed and veterinary supplies. Spending on family crop labour and hired labour is the product of the average village daily wage and the number of person-days of family or hired labourers in the most recent long rains season, respectively. The agricultural investment index is a weighted average of all these outcomes, with leisure time re-coded as negative, following Anderson (2008). The q-values for the agricultural investment index are calculated across all other summary indices reported in Table 7.

Table 4: EDUCATIONAL INVESTMENTS

After five years	(1) Treatment	(2) Placebo	(3) Treat. vs. placebo	(4) Control mean (SD) Total obs.
Cohort 1: Children of post-primary school-going age at endline (aged 11–15 at the time of the intervention)				
Children aged 16-20 in school	0.06*	0.00	0.06	0.17
	(0.03)	(0.03)	(0.04)	(0.41)
	[0.08]*	[0.94]	[0.13]	1078
Daily minutes in school for children aged 16-20	33.08**	2.40	30.69**	58.64
	(13.48)	(11.37)	(13.82)	(149.88)
	[0.03]**	[0.94]	[0.05]*	1070
Daily minutes studying for children aged 16-20	9.25**	1.69	7.56	17.82
	(4.68)	(4.31)	(5.05)	(52.12)
	[0.06]*	[0.94]	[0.13]	1063
Children aged 16-20 that attained 8th grade	0.09***	0.01	0.07**	0.07
	(0.03)	(0.02)	(0.03)	(0.26)
	[0.00]***	[0.94]	[0.04]**	1078
Cohort 2: Children of primary school-going age at endline (aged 2–10 at the time of the intervention)				
Children aged 7-15 in school	0.03	-0.02	0.05	1.22
	(0.08)	(0.08)	(0.08)	(1.18)
	[0.67]	[0.82]	[0.52]	1078
Daily minutes in school for children aged 7-15	23.47	-13.63	37.11	527.12
	(30.16)	(29.03)	(30.22)	(437.21)
	[0.65]	[0.82]	[0.52]	1060
Daily minutes studying for children aged 7-15	17.34*	9.78	7.56	91.29
	(8.91)	(8.56)	(9.14)	(115.61)
	[0.16]	[0.76]	[0.52]	1061
For all children				
Schooling expenditure (USD) PPP	6.97**	1.17	5.81**	19.17
	(2.84)	(2.53)	(2.95)	(32.73)
	[0.01]**	[0.64]	[0.05]**	1065
Summary index:				
Educational investment index	0.25***	0.04	0.21***	0.00
	(0.07)	(0.07)	(0.08)	(1.00)
	[0.00]***	[0.96]	[0.01]***	1082

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences between Columns 1 and 2. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each group (panel) of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The unit of observation is the household. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile. All columns control for village fixed effects and characteristics of the household head: age, years of education, an indicator for being single, and an indicator for being male. Heteroskedasticity-robust standard errors are in parentheses. Regressions control for baseline outcomes. We label "Cohort 1" those children aged 16 to 20 at the five-year follow up, who were 11 to 15 at the time of the intervention. We label "Cohort 2" those children aged 7 to 15 at the time of the five-year follow-up, who were aged 2 to 10 at the time of the intervention, so some but not all were of school-going age. We examine all households in the sample, including 71 households who have no children in this age group in any of the rounds, to ensure the sample is comparable with other results. Variables are defined in Appendix C. Daily minutes of an activity are the sum of schooling-age household members' daily minutes. School expenditures is the amount spent on uniforms, stationery and books, textbooks, and donations to the school for the whole household. The educational investment index is an inverse-covariance-weighted average of all outcomes reported in the table, following Anderson (2008). The q-values for the educational investment index are calculated across all other summary indices reported in Table 7.

low base. At baseline, only 39 of our control group households, 7 per cent, report having children aged 11 to 15 who have completed upper primary school.

The second panel of Table 4 shows more suggestive effects on children of primary school-going age at the five-year follow-up ("Cohort 2"). Some, but not all, of these children were all of school-going age at intervention. There are no significant increases in enrolment. Primary education enrolment rates increased from 57 to 65 per cent in the control group between baseline and the five-year follow-up, partly due to local authorities tracking and enrolling primary-age children, so potentially the treatment had no additional effect. There are small increases in time spent in school (22-45 minutes) and studying (7-17 minutes), of similar magnitude to the older age groups.[37]

In the third panel of Table 4, we show that treatment significantly increases schooling expenditures five years after the intervention. Schooling expenditures (measured as spending between on uniforms, stationery, textbooks, and school fees or donations) in the treatment group are 36 per cent higher than in the control group and 30 per cent higher than in the placebo group. This result is robust across specifications.

Overall, the treatment increases an index of all outcomes in Table 4 by 0.25 and 0.21 standard deviations relative to the placebo and control group respectively, five years after exposure to the video intervention. This is robust to the alternative specifications (Appendix Table A.18). This is consistent with the hypothesis that exposure to the videos led to higher investment, but in an even longer-term investment than in agriculture.

5.1.3 Consumption, durable goods and well-being

Table 5 shows the impact on indicators of the standard of living five years after the screenings. We find that after five years the intervention increased wealth in the form of consumer durables and housing, and improved some indicators of food insecurity and subjective wellbeing. It did not increase indicators of current food and basic non-food consumption as we measured them.

Treated households perceive themselves to be less at risk of hunger (top panel, Table 5). Households were asked how many months in the past year they had difficulty satisfying food needs. Treated households have had fewer of these periods: they faced 0.33 and 0.38 fewer months in the previous year with difficulty satisfying food needs relative to the control and placebo groups, respectively. The control group faced 2.71 months with these difficulties, reflecting high levels of poverty in the area.[38] However, there is no difference between groups on a qualitative scale, the United States Department of Agriculture's food insecurity questionnaire (Bickel et al., 2000), capturing, for example, how frequently households skip meals or run out of money to buy food. This scale may be less suited to households who consume largely from

37. Effects on minutes studying are significant relative to control in all specifications but smaller, not robust to multiple inference, and not significant relative to placebo. Effects on minutes in school are also positive but only significant in alternative specifications (Appendix Table A.20).

38. At baseline, we measured the number of months a household relied solely on its own food production, rather than months of food insecurity. We use this measure as a baseline control.

own production.[39]

There are few effects on food or frequent non-food consumption and marginal increases in non-food infrequent expenses, such as on clothing, services or ceremonies (second panel, Table 5). Treated households also reported higher values for a self-reported measure of general economic position relative to both the control and placebo group, though this increase is only significant at the 10 per cent level.[40]

As was discussed in Section 3.2 or more formally in Appendix A, how consumption is affected by the intervention, even after five years, depends on individuals' preferences. We predict that the intervention increases lifetime wealth. If income effects dominate, this might increase current consumption. On the other hand, treated individuals may continue to move spending to the future (via savings, or productive investments of various forms) if (intertemporal) substitution effects dominate, reducing current consumption. Hence, the effects on consumption of the intervention are theoretically ambiguous; the findings are consistent with the substitution effect balancing out the income effect, at least in the time frame considered and for our measures of food and frequent non-food consumption. But, importantly, none of these measures include any estimate of the service flow value from consumer durables or housing. These goods may have been accumulated since the intervention and are likely to have a higher income elasticity, where an income effect may dominate.

In fact, treated households report a higher stock of consumer durables such as furniture, kitchenware or phones, aggregated in our results as durable assets (third panel, Table 5). They report 29 per cent higher value of these assets than the control group and 32 per cent higher than the placebo group, suggestive of more spending on goods with a higher income elasticity and therefore a perceived lifetime income effect. Treated households have invested more in the quality of their housing: they report an increase in the estimated value of their house (measured as the cost of rebuilding it, in materials and labour) that is 27 and 21 per cent higher than the control and the placebo groups respectively.[41] This result is consistent with direct observations by our enumerators: treated households are more likely to have been found to have their own toilet facility in all specifications than the control group, although differences are not significant relative to the placebo group. There are no effects on the probability of having a non-organic roof, also observed by enumerators, possibly because this is an expensive investment.

Treated households also score significantly higher on a Cantril ladder of self-reported wellbeing relative to the control group (fourth panel, Table 5). Treated participants score about a quarter of a step higher than control and placebo groups, although there is no significant effect

39. Results are robust in all alternative specifications (Appendix Table A.21).

40. Variables in this panel were not measured at baseline, but results are robust in the PDS Lasso specification (Appendix Table A.21).

41. These effects on durables and housing are robust to alternative specifications, except that in the PDS Lasso specification, the difference in the value of durable assets between treatment and placebo is smaller and marginally not significant and coefficients on housing quality shrink slightly in magnitude while remaining statistically significant (Appendix Table A.21). The housing value, non-organic roof and own toilet variables were not pre-specified.

Table 5: Consumption, durable goods and well-being

After five years	(1) Treatment	(2) Placebo	(3) Treat. vs. placebo	(4) Control mean (SD) Total obs.
Food security:				
Food security index: z-score	-0.07	-0.10	0.03	0.48
	(0.06)	(0.06)	(0.06)	(0.92)
	[0.28]	[0.22]	[0.61]	1076
Months of food insecurity	-0.33**	0.04	-0.38***	2.71
	(0.15)	(0.15)	(0.14)	(2.13)
	[0.05]*	[0.77]	[0.02]**	1038
Consumption:				
Food consumption (USD) per ad. equiv. monthly PPP	-1.98	-2.29	0.32	53.91
	(2.05)	(1.92)	(2.07)	(29.98)
	[0.33]	[0.58]	[0.88]	1076
Frequent non-food (1m recall USD) per ad. equiv. PPP	0.44	0.04	0.40	4.08
	(0.28)	(0.28)	(0.30)	(3.69)
	[0.19]	[0.94]	[0.25]	1076
Non-food consumption (12m recall USD) per ad. equiv. monthly PPP	0.70	-0.54	1.24**	7.47
	(0.51)	(0.43)	(0.48)	(6.35)
	[0.21]	[0.58]	[0.05]*	1079
Consumption of sin goods (USD) PPP	0.20	0.04	0.17	0.80
	(0.13)	(0.12)	(0.13)	(1.66)
	[0.19]	[0.94]	[0.25]	1078
General economic position (scale 1 to 4)	0.09*	0.00	0.09*	2.10
	(0.05)	(0.05)	(0.05)	(0.73)
	[0.19]	[0.94]	[0.21]	1088
Non-productive durables and housing:				
Value of durable assets excluding tools (USD) PPP	20.71**	-1.65	22.36**	70.55
	(9.97)	(8.77)	(10.17)	(127.39)
	[0.08]*	[0.85]	[0.06]*	1049
Value of house (USD) PPP	384.94***	83.72	301.22***	1384.27
	(90.70)	(81.93)	(89.10)	(1235.57)
	[0.00]***	[0.43]	[0.00]***	1020
Non-organic roof	0.01	0.03	-0.01	0.68
	(0.03)	(0.03)	(0.03)	(0.47)
	[0.59]	[0.43]	[0.64]	1036
Own toilet facility	0.07*	0.04	0.02	0.38
	(0.04)	(0.03)	(0.04)	(0.49)
	[0.08]*	[0.43]	[0.64]	1039
Wellbeing:				
Best life	0.22**	0.10	0.12	4.83
	(0.11)	(0.11)	(0.11)	(1.80)
	[0.09]*	[0.40]	[0.58]	1901
Happiest life	0.12	0.12	0.00	6.05
	(0.14)	(0.14)	(0.14)	(2.19)
	[0.38]	[0.40]	[0.98]	1885
Summary index:				
Welfare index	0.15**	0.01	0.14**	-0.00
	(0.07)	(0.07)	(0.07)	(1.00)
	[0.03]**	[0.96]	[0.04]**	1090

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences between Columns 1 and 2. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each group (panel) of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The unit of observation is the household, except for subjective well-being, which are for both household head and their spouse. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile. All columns control for village fixed effects and characteristics of the household head (or of the individual-respondent for the wellbeing outcomes): age, years of education, an indicator for being single, and an indicator for being male. Heteroskedasticity-robust standard errors are in parentheses (for the wellbeing outcomes heteroskedasticity-robust standard errors are clustered at the household-level). Regressions control for baseline outcomes, except for outcomes in the consumption panel that were not collected at baseline. Variables are defined in Appendix C. The food security index is a z-scored weighted sum of answers to the United States Department of Agriculture's food security questionnaire adapted for Ethiopia (Hadley et al., 2008). It indicates the degree of food security. Months of food insecurity is the number of months in the last 12 that the household had problems satisfying their food needs. Food consumption (7 day recall) and non-food consumption (30 day and 12 month recall) are reported per adult equivalent (PAE) and converted into monthly figures. Adult equivalents are constructed using the OECD scale. Non-food consumption with a 30 day recall is regular expenses e.g. for toiletries, transportation, mobile phone, energy, cigarettes and tobacco, repair, tailor, barber. Non-food consumption with a 12 month recall is irregular expenses e.g. for clothing, utensils, bedding, school expenses, health expenses, festivals, church and community contributions. General economic position is measured on a scale from 1 to 4, where 4 is that the household is "doing well and able to meet needs by own efforts" and 1 is that the household being "unable to meet their needs". Durable assets is the number of assets owned multiplied by the replacement value of each asset. Value of the house is how much their house would cost to build today. The roof variable is coded to reflect relative quality of the building materials and the sanitation facilities are coded to reflect the degree of privacy. Subjective well-being uses a (Cantril, 1966) ladder. Respondents are asked where they are on a ladder where 10 is the best and 1 the worst possible life, or where 10 is the happiest and 1 the least happy life. The welfare index is an inverse-covariance-weighted average of all outcomes reported above in the table, with months of food insecurity in the last year recoded to be negative, constructed following Anderson (2008). The welfare index averages over the household head's subjective well-being outcomes. The q-values for the welfare index are calculated across all other summary indices reported in Table 7.

when they are asked the same question in relation to happiness rather than life satisfaction.[42]

In Appendix Table A.5, we report on measures of revenue generated from household economic activities. While these constructs are noisy, as in much work on subsistence farmers, we still find that total gross revenue in the treated group increased 11 per cent five years after intervention. The difference between treatment and placebo is significantly different from zero at 10 per cent, while the difference between treatment and control is not statistically significant.

Overall, these patterns suggest that treated households have (modestly) improved their standard of living, in addition to their effort and investments. The treatment effect on an index of all outcomes in Table 5 (combining the outcomes reported in the top four panels) is positive and statistically significant: the index is 0.15 and 0.14 standard deviations higher than the control and placebo group respectively, five years after exposure to the video intervention.[43]

5.2 Where do these results come from?

5.2.1 Early impact

Our video intervention began to change household behaviour soon after the screenings. In Figure I, we report on a shorter survey, collected six months after the experiment, which shows some future-oriented behaviours had already changed.[44] To facilitate comparability, coefficients are standardised z-scores, computed by subtracting the control mean and dividing by the control standard deviation. Effects in the units of the outcomes are reported in Appendix Table A.6. The questionnaires are the same between rounds except where discussed below.

In the bottom left of Figure I, we show that treatment significantly increased the stock of savings relative to both the placebo and control groups soon after the intervention (see Appendix Table A.3 for the results in table form). Savings behaviour is a good short-run indicator of increased propensity to invest, as it is unlikely poor households could immediately make new asset purchases given limited resources. Only 39 per cent of the control group had savings, averaging 24.37 USD PPP. In the treatment group, the amount of savings nearly doubled in percentage terms six months after intervention, although there is little change in whether households have any savings.

Those effects do not persist in the long-run. This suggests households may have accumulated cash savings in the short run and invested them in relatively lumpy assets such as durables, livestock and housing, which we see in Table 3. Indeed, five years later, savings remain very modest for all groups, as compared to other assets. Control means at endline for savings are 18 USD in 2016 PPP versus livestock (2018 USD PPP), house (1384 USD PPP), durables (71 USD PPP) and tools (106 USD PPP). This suggests households ultimately seek

42. Effects are robust when not controlling for the baseline value of the outcome. Coefficients are still positive, but smaller and not significant, in the PDS Lasso specification (Appendix Table A.21).

43. This increase is robust to removing the control for baseline outcomes, but somewhat smaller and marginally significant in the PDS Lasso specification only relative to the control group (Table A.18).

44. Bernard et al. (2014) reported results from this short-run survey. This paper includes all main outcomes reported there and replaces that paper. Most outcomes are in Figure I, except hypothetical variables measuring potential demand for credit, reported in Appendix Table A.6.

not to hold savings in cash. There are few formal financial institutions through which they can earn returns and high inflation. They prefer to invest in assets.

We also observe an increase in loans in the short term, but this effect is smaller in percentage terms than effects on savings and also does not persist. This suggests households had repaid these loans by endline.

The treatment had already induced investments in education early on (right panel of Figure I). We first analyse Cohort 1 from Table 4: those aged 11 to 15 (upper primary school-going age) at the time of screening. We find suggestive evidence that households started investing more in their children's education six months after screening. We find a 13 per cent increase in the number of children aged 11 to 15 enrolled in school in the treatment group relative to the control group, though not statistically significant (p-value=0.13). There is a 17 per cent increase in the time spent studying (p-value=0.08) and an 8 per cent increase in time spent in school (p-value=0.32) relative to the control mean. This cohort eventually accumulates more years of education by the five-year follow-up.

We separately analyse the outcomes of children aged 7 to 10, of lower primary school-going age at the time of screening. This is a subset of Cohort 2 in the five-year follow-up in Table 4. Cohort 2 in Table 4 consists of children aged 2 to 10 at the time of screening. At the six-month follow-up we only collected educational outcomes for children of school-going age (7 and above). So those aged 2 to 6 at the time of the screening are missing education data at the six-month follow-up but are included in Cohort 2 in the five-year follow-up because they are of primary school age. In this younger cohort, we find a significant increase in enrolment relative to the control group, equivalent to about 18 per cent of the control number of children enrolled, and an increase in time spent in school (p-value=0.16) but no effect on time studying.

Treated households increased education spending after six months, with a 17 per cent increase in school-related household expenditures relative to the control group, but no significant difference relative to the placebo. This effect persists at endline.

We do not find strong evidence that households had already changed their labour supply decisions in response to the intervention. They had marginally increased time spent on the family farm. But the effects after six months are smaller than those reported in Table 3, so that differences are not significant relative to the control group but only relative to the placebo group. This is potentially because rounds cannot be perfectly compared. The time use data in the five-year follow-up survey were collected in relation to a day in October, the busy harvest season. The six-month follow-up was related to a day in March, when work on the farm is likely to be less intensive, largely land preparation for the rains in June, so we may be less likely to see strong effects at this time.[45]

In sum, households had already made some changes in future-oriented behaviour after six months. We do not measure all variables we capture in the long-term survey in this shorter-term

45. The five-year measure of labour supply also included off-farm self-employment, which we did not collect at the six-month follow-up.

follow-up. But where we have similar variables in both rounds, we observe consistent patterns of behaviour change. Many of the initial changes households made at six months – in education investment and asset accumulation – persist and become more pronounced after five years.

5.2.2 Impact on aspirations and expectations

Next, we provide some evidence on the potential psychological mechanisms at play. We collected measures of aspirations and expectations for the household on income, wealth and children's education, from both spouses. We also collected measures of their reports of the household's current position on these dimensions, at baseline, straight after the screening, after six months and after five years.

Figure II displays effects on indices of aspirations and expectations. Appendix Table A.1 reports results for indices and their individual components. Section 3.3 describes the measures.

Aspirations: Five years after the intervention, we observe statistically significant increases in the aspirations index – between 0.12 and 0.15 standard deviations – relative to both the placebo and control groups.[46] These are driven by increases in all dimensions of aspirations, though not all are individually significant (Appendix Table A.1). The magnitudes are modest: for education, the effect is half the baseline gap between the poorest and richest terciles; for income and wealth, the effects are around one-third and one-fifth of the corresponding gaps (Table 1). Crucially, effects stem from the role-model content in the documentaries, not from the screening itself: the placebo group shows no long-term changes, and differences between treatment and placebo are statistically significant, in all specifications.

We find smaller, less precise effects immediately after screening and at the six-month follow-up, although we cannot statistically reject that effects are the same in each round. This hints at some dynamic empirical pattern in the data. As alluded to in the conceptual framework in Section 3.2, aspirations may evolve gradually as people experiment with possible alternative futures (Appadurai, 2004) or learn-by-doing when demotivating beliefs start to shift (Carvalho et al., 2023). People may begin investing, observe returns, and revise their goals upward. This is consistent with the model in Dalton, Ghosal, and Mani (2016), where aspirations depend partly on wealth. Similarly, Orkin et al. (2023) find that aspirations of Kenyan households adapt and increase when wealth increases.

Aspirations gaps: Some models focus not on absolute aspiration levels but on the gap between aspirations and current status (Genicot and Ray, 2017). Our results using this "aspirations gap" – the difference between desired and current levels – are qualitatively similar (Appendix Table A.2).[47]

Expectations: We can compare treatment effects on aspirations to effects on more standard measures of expectations in the same domains, asking what respondents think they will

46. These are robust to alternative specifications in Appendix Table A.22, except that the aspirations effect relative to the control is positive but not significant in the PDS Lasso specification.

47. For the current level of education, we use the respondents' own education level. Recall from section 3.3 that these beliefs about current position are strongly correlated with actual values reported in the survey.

reach in ten years instead of what they would like to achieve over their lifetime. Empirically, as discussed in Section 3.3, levels of expectations are lower than aspirations for all rounds, but the measures are strongly correlated within respondents.

Effects on aspirations and expectations evolve in a similar way. Straight after screening, there are only small and noisy effects on expectations relative to the placebo.[48] Like aspirations, effects are small and positive after six months and large, positive and statistically significant after five years. We cannot reject that effects on aspirations and expectations indices relative to the placebo are the same in each round. This provides a sense check, showing that beliefs about the future change similarly to aspirations.[49]

Overall, five-year effects on aspirations are stronger and more robust, but some impacts were visible soon after the intervention. These findings support our hypothesis: the intervention increased aspirations, which in turn influenced forward-looking behaviour – including effort and investment – with effects sustained over time.

5.2.3 Alternative mechanisms

Although our results support the aspirations hypothesis, we test alternative mechanisms that might be affected by our intervention and yield similar changes in economic behaviour. We consider three plausible alternative mechanisms, as well as social desirability bias caused by the study itself. Appendix Section C.4 details the construction of the measures used to test these alternative mechanisms.

Time and risk preferences: The intervention could have increased the discount factor (β in our model in Appendix Section A), leading to the observed increase in future-oriented behaviour, as the future is more valued.[50] But we do not find that time preferences have shifted (top panel, Table 6). Six months after intervention, there is no change in the share of patient, impatient, or very impatient respondents, categorised using the measure in Ashraf, Karlan, and Yin (2006), or of present-biased respondents. After five years, there is a small negative treatment effect on the share of time-inconsistent respondents that are patient now and impatient later, but this effect does not occur six months after intervention and is not robust to multiple hypothesis testing.

While risk does not enter explicitly the theoretical framework, it could be trivially extended. For example, if future returns are risky, then lower risk aversion would induce more effort and investment in the future. The increased salience of a plausible future through the intervention may have reduced risk aversion, leading to the observed effects. But we also find no changes in risk preferences, using a Binswanger (1980) adaptation (top panel, Table 6).[51]

48. There are some effects relative to the control, but this is driven by an effect of the placebo. This disappears by six months and does not lead to any effects on behaviour).

49. Our theoretical framework does not make clear predictions on behaviour of expectations, for reasons discussed in Appendix Section A.

50. Alternatively, Gabaix and Laibson (2017) theorise that improving the extent to which households can visualise the future may lead to more patient behaviour.

51. In fact, if anything, our risk aversion measure has increased relative to the placebo group after six months,

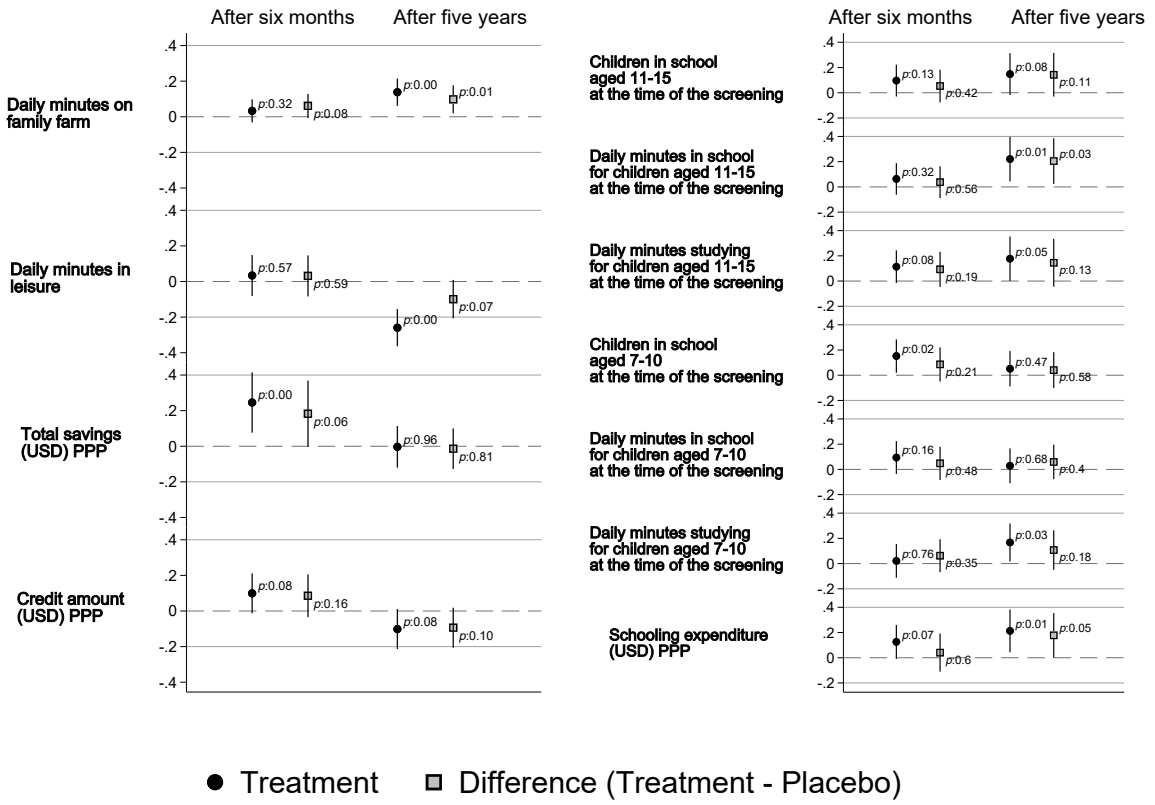

Figure I: Treatment effects on economic outcomes observed six months and five years after the screening

Notes: OLS estimates of within-village treatment effects on economic outcomes across survey rounds relative to the control and placebo groups. Outcomes are collected six months or five years after the screenings. The comparison group is households from the 64 treated villages that were not invited to any screening. The outcomes units are standardised z-scores, computed by subtracting the control mean and dividing by the control standard deviation. Effects in the outcome units are reported in Appendix Table A.6. All columns control for village fixed effects, the baseline value of the outcome, and characteristics of the respondent: age, years of education, an indicator for being single, and an indicator for being male. The unit of observation is the individual respondent (household head or their spouse) for the outcomes on the left panel, and the household for the outcomes in the right panel. Standard errors are robust to heteroskedasticity (or clustered at the household-level for the left panel). Bars are 95 per cent confidence intervals. Square-shaped markers report the difference between the treatment and placebo effects. Variables are defined in Appendix C. Time spent on work and leisure is collected for a typical day in March for the six-months follow-up, and for October for the five year survey for the head and spouse, reported by the household head and expressed per adult. Total savings is the amount saved both in and outside the home by each respondent. Credit amount records the total amount of loans taken from any formal or informal source in the past six months by each respondent. Cohort 1 are children who were 11 to 15 at the time of the screening and 16 to 20 at the five-year follow-up. Cohort 2a are children who were 7 to 10 at the time of the screenings, a subset of Cohort 2 in Table 4, children aged 7 to 15 at the time of the five-year follow-up. Cohort 2a is not directly comparable to cohort 2 in Table 4, because some of the children in cohort 2 were not of primary school-going age at the time of six-months follow-up and as they would have been between 2 and 6 years old; we did not collect data for children in this age range. We examine all households in the sample, including 71 households who have no children in this age group in any of the rounds, to ensure the sample is comparable with other results. Daily minutes of time spent studying or in school are the sum of schooling-age household members' daily minutes. School expenditures is the amount spent on uniforms, stationery and books, textbooks, and donations to the school for the whole household.

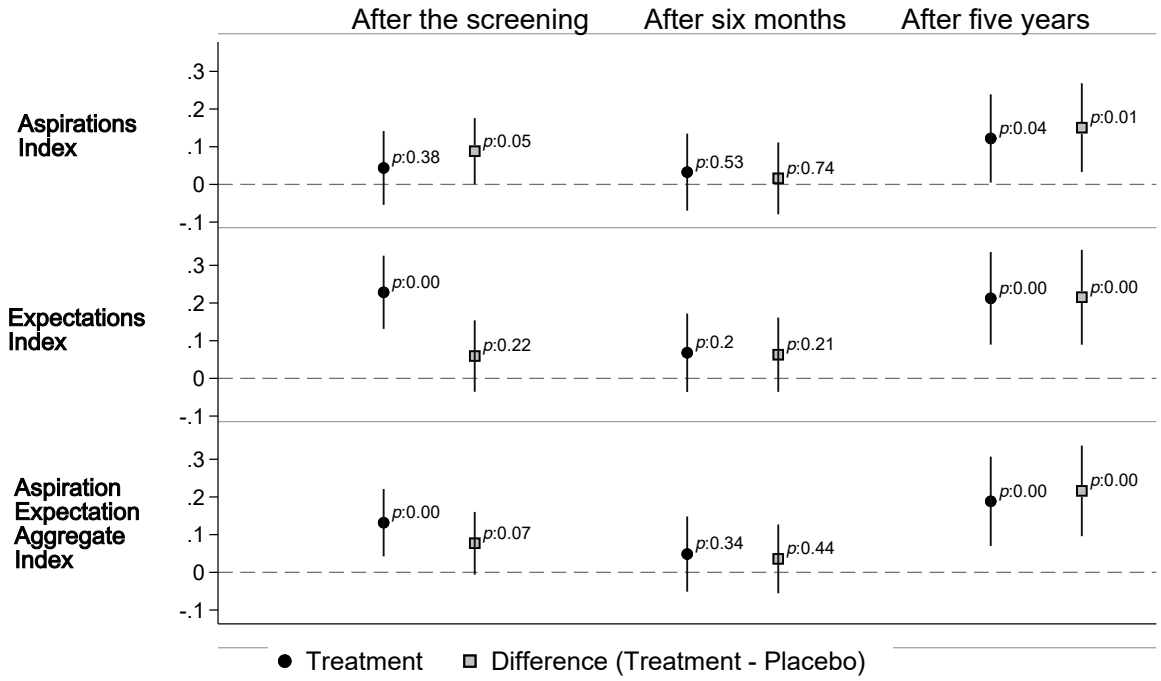

Figure II: Treatment effects on the aspirations and expectations indices.
Notes: OLS estimates of within-village treatment effects on aspiration and expectations indices across survey rounds relative to the control and placebo groups. "After the screening" reports on data collected right after the screening. The remaining columns report on outcomes collected after six months and five years. The comparison group is households from the 64 treated villages that were not invited to any screening. All columns control for village fixed effects, the baseline value of the outcome, and characteristics of the respondent: age, years of education, an indicator for being single, and an indicator for being male. The unit of observation is the individual respondent. Heteroskedasticity-robust standard errors are clustered at the household level. Bars are 95 per cent confidence intervals. Square-shaped markers report the estimated difference between the treatment and placebo effects. The aspirations index is an Anderson (2008) index combining what individuals would like to achieve in their life in terms of reported income, wealth, and years of education for their eldest child. The expectations index similarly combines what individuals think they will achieve in ten years time on the same three dimensions. The aspirations and expectations aggregate Anderson (2008) index combines six dimensions of reported income, wealth and years of education for their eldest child, for aspirations and expectations. Appendix Table A.1 reports results across all three indices and their individual components.

Perceived returns to own effort and causes of success: The intervention may have altered people's broad beliefs about their ability to change their own outcomes, or, in economic terms, beliefs about the return to their own effort. Six months after the screening, we find small increases (of around 2 per cent) in internal locus of control (second panel, Table 6), and weak positive effects on a similar measure, the extent to which people believe poverty is an issue of individual agency.

However, there are no persistent effects on either measure of locus of control after five years. In the treatment group, both measures revert back to the same level as at baseline. This suggests locus of control does not account for long-term changes in investment. If this were the mechanism, we would expect short-term effects on investment as locus of control rises

although the effect is not robust to multiple test correction and does not persist after five years.

Table 6: Testing mechanisms

	After six months				After five years			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.
Risk and time preferences:								
Risk aversion: most to least risk averse (1 to 5)	0.03	-0.12	0.14*	3.20	0.03	-0.04	0.06	2.53
	(0.08)	(0.08)	(0.08)	(1.51)	(0.09)	(0.09)	(0.09)	(1.54)
	[0.89]	[0.36]	[0.45]	2025	[0.79]	[0.83]	[0.51]	1873
% that is patient	-0.01	-0.01	0.00	0.30	-0.02	-0.00	-0.01	0.17
	(0.02)	(0.02)	(0.02)	(0.46)	(0.02)	(0.02)	(0.02)	(0.37)
	[0.89]	[0.67]	[0.96]	2054	[0.48]	[0.83]	[0.51]	1899
% that is somewhat impatient	-0.02	-0.02	0.00	0.15	-0.02	-0.00	-0.02	0.11
	(0.02)	(0.02)	(0.02)	(0.36)	(0.02)	(0.02)	(0.02)	(0.31)
	[0.89]	[0.44]	[0.96]	2054	[0.43]	[0.83]	[0.51]	1899
% that is most impatient	0.03	0.04	-0.01	0.55	0.04	0.01	0.03	0.73
	(0.03)	(0.03)	(0.03)	(0.50)	(0.03)	(0.02)	(0.02)	(0.45)
	[0.89]	[0.36]	[0.96]	2054	[0.37]	[0.83]	[0.51]	1899
% that is present biased	0.02	0.04	-0.02	0.34	0.03	0.05*	-0.02	0.53
	(0.03)	(0.03)	(0.03)	(0.47)	(0.03)	(0.03)	(0.03)	(0.50)
	[0.89]	[0.36]	[0.96]	1999	[0.48]	[0.48]	[0.51]	1872
% that is patient now and impatient later	-0.00	0.00	-0.00	0.22	-0.05**	-0.02	-0.03	0.19
	(0.02)	(0.02)	(0.02)	(0.41)	(0.02)	(0.02)	(0.02)	(0.39)
	[0.98]	[0.98]	[0.96]	1999	[0.20]	[0.83]	[0.51]	1872
Perceived returns of own effort:								
Internal locus of control	0.26**	-0.05	0.31**	12.95	0.02	0.06	-0.04	12.26
	(0.12)	(0.12)	(0.12)	(2.10)	(0.11)	(0.11)	(0.11)	(1.90)
	[0.10]*	[0.65]	[0.04]**	2053	[0.86]	[0.87]	[0.94]	1899
Grit index	0.04	-0.06	0.10	0.00	-0.05	-0.05	0.00	0.02
	(0.06)	(0.06)	(0.06)	(1.00)	(0.06)	(0.06)	(0.06)	(0.99)
	[0.56]	[0.43]	[0.16]	2064	[0.86]	[0.87]	[0.94]	1909
Individual causes of poverty	0.25*	0.23*	0.02	9.19	0.03	0.01	0.03	9.14
	(0.14)	(0.14)	(0.14)	(2.41)	(0.14)	(0.13)	(0.13)	(2.03)
	[0.11]	[0.28]	[0.91]	2033	[0.86]	[0.97]	[0.94]	1883
Perceived external causes of success:								
Chance locus of control	-0.02	-0.03	0.01	13.36	0.01	-0.05	0.05	12.67
	(0.16)	(0.15)	(0.16)	(2.71)	(0.15)	(0.15)	(0.15)	(2.35)
	[0.92]	[0.87]	[0.93]	2050	[0.96]	[0.96]	[0.74]	1899
Fate causes of poverty	-0.24*	-0.02	-0.22	7.19	-0.03	0.00	-0.03	6.70
	(0.14)	(0.14)	(0.14)	(2.31)	(0.11)	(0.10)	(0.10)	(1.79)
	[0.16]	[0.87]	[0.22]	2039	[0.96]	[0.96]	[0.74]	1887
Awareness and perceived returns of agricultural technologies:								
Information index					-0.02	-0.07	0.04	0.00
					(0.07)	(0.07)	(0.07)	(1.00)
					[0.73]	[0.34]	[0.93]	1104
Expected fertiliser yields index					0.10	0.10	0.01	-0.00
					(0.07)	(0.07)	(0.07)	(1.00)
					[0.29]	[0.32]	[0.93]	1085

Notes: OLS estimates of within-village treatment and placebo effects after six months (columns 1-2) and after five years (columns 5-6) of the intervention. Columns 3 and 7 test for differences in parameters obtained in previous two columns. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 and 8 display the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and respondent's age, years of education, an indicator for being single, and an indicator for being male. All regressions control for the baseline value of the outcome, except for the outcomes in the "Awareness and perceived returns of agricultural technologies panel" that were not collected at baseline. Heteroskedasticity-robust standard errors clustered at the household-level are in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Variables are defined in Appendix C. The unit of observation is the individual respondent (household head or their spouse), except for information and fertiliser beliefs indices (which are at the household-level). The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous outcomes above the 99^{th} percentile, though indices aggregate all non-missing outcomes. The locus of control variables are based on Levenson (1981) and capture if people see outcomes as contingent on their behaviour (internal locus of control) or as a result of chance, luck or fate (chance locus of control). The causes of poverty variables are based on the Attributions for Poverty scale (Feagin, 1975) to measure people's perceptions of the causes of poverty among people in general, rather than only in their own lives. We use survey-based instruments to calculate risk (Binswanger, 1980) and time preferences (Ashraf, Karlan, and Yin, 2006). Grit includes answers to two survey questions about how the respondent would characterise themselves. The information index is constructed from indicator variables that take value one if the household head reported having performed some of the behaviours that were described in the documentaries. Appendix Table A.4 reports treatment effects on the components of this index. The expected fertiliser yields index is based on the household heads' expected increase in output from the application of different quantities of fertiliser for maize and sorghum in an hypothetical good or bad season. These are standardised Anderson (2008) indices.

but then effects on investment fading away as effects on locus of control fade away. This is not the pattern we see. In contrast, aspirations and expectations and investment move together, increasing slightly in the short term, with larger effects after five years. We also find limited effects on two conceptually related measures, the extent to which people believe outcomes are caused by chance or that poverty is caused by fate.

We additionally consider an adapted measure of grit (perseverance and passion for long-term goals Duckworth et al., 2007; Alan, Boneva, and Ertac, 2019), which we had not pre-specified. These measures could broadly enter our model by increasing the actual returns to one's effort. We find no evidence of effects on our measure of grit after six months or five years.

Information and expected returns to innovation: In the bottom panel of Table 6, we investigate two additional mechanisms. Firstly, we measure if treated households adopted fifteen activities mentioned in the videos, such as purchasing pumps or using specific technologies. We find no effects on any of fifteen dummy variables capturing if households engage in an activity described in the videos (Appendix Table A.4), nor effects on a summary index across these fifteen variables (second last row of Table 6).[52] This suggests that the households might have been aware of these activities even without the videos, as they are common within their villages.

Secondly, we explore whether exposure to the documentaries influenced households' beliefs about the returns to modern agricultural technologies, like fertilisers, despite these not directly featuring in the videos. We find no effects on households' beliefs about the returns to fertilisers.

Experimenter demand effects or reporting issues: Finally, one might worry that self-reported outcomes may be affected by biases. To avoid enumerators treating households differently, we ensured that enumerators were blind to treatment status (as mentioned in Section 2.2).

One may worry that treated households report outcomes they think would please the enumerator, or inflate reports so they appear to have made more progress to achieving aspirations. While we cannot totally rule these out, some patterns in our results suggest these issues are not driving our results. We can largely rule out experimenter demand effects simply from pleasing outsiders. While showing videos at a ticketed showing was unusual in such a remote setting, this applied both to the placebo and treated group, as the placebo screening mirrored the treatment exactly except for the video content. If demand effects occurred, it is likely we would see at least some placebo effects, but we do not.

However, it is possible that the aspirations treatment encouraged specific types of responses not suggested by the placebo, creating demand effects. Here too, some results suggest this is unlikely. First, treated households do not report higher values on some modules where the intervention might suggest particular answers might be desirable. There are no effects on

52. This finding helps to shed light on a potential puzzle in the results. Education investment is one key area of change due to our intervention in both the short and long term. However, the videos made little or no reference to schooling as a means of improving one's economic position. The absence of an information effect helps to square this: the intervention may have shifted goals and aspirations within the domains the treated households thought relevant for them, and not simply in areas that were mentioned during the video as used by the role models.

savings or land under cultivation in the five-year follow-up, although both are clearly indicators of hard work. We did not find increased adoption of any of the activities or practices shown in the videos. Second, households report consistent increases in similar indicators in different modules of the survey. They report increased flows of spending on durable assets in the consumption module and increased asset stocks in the asset module. They report increased labour supply in both the time allocation module and agricultural inputs module. It is highly unlikely that households could identify that these different modules measured similar concepts to give inflated answers in both modules. Third, we see stronger effects in the endline than the midline, but it seems unlikely that experimenter demand effects would persist or increase over a five-year horizon. Finally, we examine non-pre-specified measures of living standards that were directly observed by the enumerator (Table 5). We see significant effects on the presence of a separate toilet just for the household relative to the control group (although not relative to the placebo group), suggesting improvements in living standards that cannot be accounted for by reporting biases. We also measured if enumerators found households had non-organic roofs, but find no effects here. This is not a perfect test as lack of effect does not indicate reporting bias: households may have invested, but in other types of assets, especially as these assets are lumpy and expensive. Thus, while we cannot fully rule them out, the pattern of our results suggests experimenter demand issues are unlikely.

Relatedly, the treatment may have made respondents report a more positive and optimistic description of their outcomes at present. However, we see few effects on modules which would be likely to be affected by increased optimism. We also see few effects on well-being in the endline, nor on household reports of their current position at the start of the aspirations and expectations module.

Ideally, we would have collected administrative data as a further check on the findings. But our attempts were thwarted. The smallest administrative unit for which administrative data is collected on relevant variables is the sub-district or *kebele* and not the village. While enumerators collected data on some relevant variables at this level, we did not treat enough people in a *kebele* to test for effects, with a median of only one per cent of households treated. Since 2018, this area has been inaccessible due to insecurity, preventing further data collection.

In sum, we cannot *prove* that the behavioural change is caused by the aspirations shift, but we definitely cannot reject it as the most plausible explanation: we find little support for these alternatives, though other psychological pathways cannot be ruled out.

5.3 Discussion of main results

Overall, the results support our predictions. Our intervention showed documentaries featuring individuals who overcame poverty to achieve success telling their stories. In line with social learning theory in psychology, this has exposed the treated group to individuals whose initial life conditions they could relate to and whose success they could see as reachable.

The intervention led to positive changes in aspirations and economic outcomes. We finally

combine all the summary indices (agricultural investment, educational investment, welfare, and the aspirations and expectations aggregate index) into a single omnibus index, which finds the intervention yields a positive and significant effect on outcomes of 0.26 standard deviations relative to the placebo and 0.24 standard deviations relative to the control, five years after exposure to the role models in the videos (Table 7). Results are robust in all alternative specifications. We find little or no heterogeneity in treatment effects on our summary indices across all the baseline measures of heterogeneity we had pre-specified (Appendix Figure A.4).[53]

The patterns of results in Section 5 are consistent with households being induced to aspire to and emulate what better off households in their communities do, even though they had lived with them well before the intervention. Treated households engage more in the kind of activities and investments the top tercile in Table 1 were doing at baseline, such as investing in livestock and working more on the farm, rather than specifically doing what was portrayed in the videos. We also see increases in effort; locally common productive activities, crops and livestock; and investment into children's education that persist for five years after the intervention.[54]

Our results counter concerns about some of the possible negative consequences of boosting aspirations and aspirations gaps. Ray (2006), Genicot and Ray (2017), and McKenzie, Mohpal, and Yang (2022) suggest boosting aspirations may give "false hope" (encouraging households to take decisions that make them worse off) or make people "frustrated" as aspirations have been raised too high, leading to less investment and effort, with negative welfare consequences. We see little evidence of this: we find more investment and higher standard of living, with no negative effects on well-being.

5.4 Cost-effectiveness analysis and benchmarking

Appendix Section G reports intervention costs and back-of-the-envelope benefit-cost ratios at both the experiment's (small) scale and a reasonable delivery scale, suggesting the intervention was highly cost-effective. At the experiment's scale, the intervention costs $62 (USD 2016 PPP) per household, driven by high fixed costs of producing documentaries for a small group and showing videos to few households per village. At a larger scale, it could cost $10 (USD 2016 PPP) per household, using cost estimates from an experiment screening videos to 57,750 households via agricultural extension agents in a similar area (Bernard et al., 2019b). Our back-of-the-envelope benefit-cost ratios consider only benefits in asset wealth after five years, excluding children's educational attainment or food security, and only realised benefits

53. Bernard et al. (2019a) had used the six-month follow-up to study heterogeneity by the gender of the children. The intervention improved outcomes for both boys and girls equally, but did not reduce the gender gap (Appendix Table A.23). Girls continued to lag behind boys, with no convergence over the five-year period.

54. In an exploratory analysis, we also test whether effects vary by the terciles of durable assets used to categorise our sample in Table 1. We find that effects on the aspirations and expectations aggregate index are smaller among households in the lowest tercile of durable assets relative to those in the middle tercile (results available upon request). This would suggest that the intervention is not boosting aspirations amongst the poorest amongst a generally poor group. However, in Figure A.4, there is no differential effects on aspirations or expectations when those with below median assets are compared with the others, so we remain cautious about this result.

Table 7: Summary indices in within-village analysis

After five years	(1) Treatment	(2) Placebo	(3) Treat. vs. placebo	(4) Control mean (SD) Total obs.
Agricultural investment index	0.21***	0.03	0.18***	-0.00
	(0.06)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.96]	[0.00]***	1082
Educational investment index	0.25***	0.04	0.21***	0.00
	(0.07)	(0.07)	(0.08)	(1.00)
	[0.00]***	[0.96]	[0.00]***	1082
Welfare index	0.15**	0.01	0.14**	-0.00
	(0.07)	(0.07)	(0.07)	(1.00)
	[0.03]**	[0.96]	[0.04]**	1090
Aspiration index	0.12**	-0.03	0.15***	0.02
	(0.05)	(0.05)	(0.05)	(1.00)
	[0.02]**	[0.96]	[0.00]***	1904
Expectations index	0.21***	-0.00	0.22***	0.01
	(0.05)	(0.05)	(0.05)	(1.00)
	[0.00]***	[0.96]	[0.00]***	1900
Aspirations and expectations aggregate index	0.19***	-0.03	0.22***	0.01
	(0.05)	(0.05)	(0.05)	(1.00)
	[0.00]***	[0.96]	[0.00]***	1904
Omnibus index	0.26***	0.02	0.24***	-0.00
	(0.06)	(0.06)	(0.07)	(1.00)
	[0.00]***	[0.96]	[0.00]***	1091

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences in parameters obtained in first two columns. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and characteristics of the household head (or spouse for individual-level outcomes): age, years of education, an indicator for being single, and an indicator for being male. Regressions control for the baseline value of the outcome, except for the regressions on the agricultural investment index, welfare index, and omnibus index, which control for the respective index constructed from all the available components available at baseline. Heteroskedasticity-robust standard errors (or clustered at the household-level for individual-level outcomes) are in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The unit of observation is the household, except for the aspirations and expectations indices (which are are observed for both household head and their spouse). The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous outcomes above the 99^{th} percentile, though the indices aggregate all non-missing outcomes. The outcomes are inverse-covariance-weighted averages standardised relative to the within-village control group, following Anderson (2008). The agricultural investment index includes all outcomes reported in Table 3, with household's daily minutes in leisure being recoded to be negative. The educational investment index includes all outcomes reported in Table 4. The welfare index includes all outcomes reported in Table 5, with months of food insecurity in the last year recoded to be negative. The welfare index averages over the household head's subjective well-being outcomes. The aspirations and expectations indices are made of the reported aspirations (expectations) for income, wealth and years of education for children. The omnibus index aggregates the agricultural investment, educational investment, welfare, and aspirations and expectations aggregate standardised indices into a single index, following Kling, Liebman, and Katz (2007). We use the household head's aspirations and expectations aggregate index for the omnibus index.

without assumptions about future flows from asset wealth. On most asset wealth measures, the intervention would have paid for itself, even at the experiment's small scale. Assuming the intervention is conducted at reasonable scale and conservatively considering only durable assets (where effects are robust across specifications), benefits per household are twice costs. Considering durable, productive, and livestock assets, benefits are thirteen times costs, though effects on productive assets and livestock were noisily estimated and not always statistically significant. Considering all assets including housing, the benefit-cost ratio is even higher.

These effects are in percentage terms similar to other studies of psychosocial interventions. Appendix Figure A.5, with further explanation in Table A.27, provides a comparison with some recent papers that tested similar psychosocial interventions, without trying to conduct a complete meta-analysis, as interventions and time-scales are difficult to compare. An evaluation in Kenya is close to our paper but only involves short-term outcomes (Orkin et al., 2023); others involve psycho-social interventions but that are more involved than our light-touch intervention such as Batista and Seither (2019), Blattman, Jamison, and Sheridan (2017), Blattman et al. (2023), Baranov et al. (2020), Cecchi et al. (2022), John and Orkin (2022) and Lubega et al. (2021). These usually occur over multiple weeks or months and target a number of psychological constructs other than or in addition to aspirations, such as patience/delayed gratification, planning and self-efficacy. We also included the BRAC-inspired graduation programme, involving a large asset transfer and other intensive support (Banerjee et al., 2015). This clearly has larger impacts on assets and living standards than ours. Finally, we include estimates of effects on aspirations from studies of exposure to role models (Beaman et al., 2012; Macours and Vakis, 2014; Rojas Valdes, Wydick, and Lybbert, 2021).

For outcomes for which we could compare the impacts in percentage terms (aspirations and expectations, effort in terms of time worked, assets and investments, and educational expenditure), our effects remain within the confidence intervals of most of these interventions, while the asset transfer programme has considerably larger effects. The novelty of our finding remains that this intervention is more light-touch and is evaluated over a longer time horizon than most of these papers (except for Blattman et al., 2023 and Baranov et al., 2020) and yet we find long-term persistence across different economic outcomes.

5.5 Lack of spillovers

Here we test for potential spillover effects between treated and other households. Social learning theory and economic models of how aspirations change suggest that treated respondents who improve their economic position may become role models for others, changing their aspirations and/or future-oriented behaviour (Bandura, 1977; Genicot and Ray, 2017). Indeed, our main results suggest aspirations are influenced by observational learning from the experiences of other people, in the videos. So treated respondents may influence others who observe them or share content with non-participants. Less positively, untreated people may be resentful or discouraged if not selected for an intervention.

We construct a group of 10 pure control villages to test for spillover effects. In these villages, we collect data with 18 randomly selected households per village in the five-year follow-up. The pure control villages are not randomly selected but we argue they provide a plausible alternative counterfactual for the treatment villages. They come from the list of 84 villages randomly selected from our sampling frame of 189 villages with 50-100 households in the district, as was described in Section 2.2. As discussed there, our treatment villages were chosen based on logistical considerations: we found 16 large screening venues and chose the 64 villages closest to those screening venues such that we had four villages within walking distance of the 16 screening sites. The next 10 closest villages make up the pure control group. They would have been selected for the experiment, had we formed quintuplets or sextuplets instead of quadruplets of villages per screening site (see also Appendix Figure A.1). While not randomly selected, these villages appear similar to villages included in the experiment (Appendix F reports balance tests). The design is shown in Appendix Figure A.3.

Our test for spillovers compares the within-village control group to the pure control group (the specification is also in Appendix F). The within-village control group live in the same villages as the treated group and could be affected by any spillovers. We focus on the summary indices we use to summarise our main results, but standardise relative to the pure control group.

We find little systematic evidence of spillovers.[55] There are significant differences between the within-village control group and pure control group on only one of five indices (Column 3, Appendix Table A.24). If anything, we find some evidence of positive spillovers for the aspirations and welfare index, which could potentially attenuate our main estimates. The estimates of all other treatment effects are very close to those in our main specifications although the control group is different (Columns 1 and 4, Appendix Table A.24).

Spillover effects may take longer than five years to occur or are small in magnitude, requiring a larger and longer-term experiment to detect them. But in this setting, we do not have strong evidence that changes in aspirations are shaped by day-to-day experiences of what others do or think, as implied by Carvalho et al. (2023) or Genicot and Ray (2020). Maybe this is not surprising: as Table 1 showed, the respondents have always lived with better-off people displaying similar behaviours to those in the videos. The videos may be a particularly resonant and memorable "vicarious" experience not provided by encountering successful people in one's daily life.

6 Conclusion

We randomly invited individuals in a poor and isolated area to a one-hour video documentary in which four people from similar backgrounds to the audience tell their life story of getting out of poverty. After five years, we find persistent effects on whether households invest for the future, and some indicators of their standard of living. These results are meaningful. The size of the effects are not very large – a few dollars more spending on education, some more

55. We pre-specified further potentially less well-powered tests for spillovers, reported in our working paper at https://cepr.org/publications/dp18492. These show little systematic evidence of spillovers.

durable assets – but in percentage terms they are not insubstantial. Something is triggered that affected forward-oriented behaviour. We find evidence consistent with a change in aspirations being the main psychological mechanism.

A back-of-the-envelope calculation suggests the intervention was cost-effective. Counting only the robust increase in durable assets, the benefit-cost ratio after five years is about 2-to-1 for a reasonable scale of implementation. In percentage terms, our effects align with those of similar psycho-social interventions – though they remain below those of more intensive anti-poverty programs – yet exhibit remarkable persistence.

Is this intervention giving false hope? We cannot fully judge this. But treated households see positive effects on assets and standard of living, suggesting overall positive impacts. And we did not suggest to individuals – rightly or wrongly – what path would lead them out of poverty, unlike most interventions that offer 'solutions' in microcredit, health or education. We only invited our treatment group to listen to stories told by individuals from similar backgrounds.

We want to be cautious. This is a remote and isolated area, and it is not clear that these effects would be present or persistent elsewhere. At least this study offers a proof of concept, that such light-touch easily scalable interventions may have persistent positive effects.

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Online Appendix

for

The Future in Mind:

Aspirations and Long-term Outcomes in Rural Ethiopia

by

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Contents

Appendix roadmap

Section A presents a formal model to conceptualise aspirations and the effect of raising them on economic behaviour. Section B describes two of the individual stories featured in the documentaries alongside with a description of the content of the placebo screenings. Section I shows detailed estimates on the aspirations and expectations measures across rounds and additional results. Section C provides details on variable construction. Section G.2 provides details about the study design, compliance, balance, and attrition. Section D outlines any of the ways in which the analysis presented departs from our original Pre-Analysis Plan. Section F presents tests for spillovers. Section G provides details of a back-of-the-envelope estimate of the intervention's cost-effectiveness.

A Theoretical appendix

This appendix section provides a formal model to derive prediction on how an intervention targeting an exogenously induced change in aspirations might affect economic decisions.

A.1 Aspirations in a reference-dependent model

We explore the effect of a change in aspirations, or the reference point, on effort and investment, in a simple multi-period model of allocating effort and resources for future benefit versus consuming more or enjoying more leisure now.

Existing economic models of aspirations formation and its consequences capture the idea that achieving goals may yield utility (Heath, Larrick, and Wu, 1999; Dalton, Ghosal, and Mani, 2016; Genicot and Ray, 2017). These models use reference-dependent utility (Kőszegi and Rabin, 2006) and interpret goals as reference points.

Aspirations enter our model as a reference point: instantaneous utility $v(c_t,l_t;a_t)$ is defined not just over consumption c_t and leisure l_t, but is anchored by the aspirations one has for one's economic position a_t. More specifically, we assume that $v(c_t,l_t;a_t)=u(c_t,l_t)+z(c_t-a_t)$, with $u_{c_t},u_{l_t}>0$ and $u_{c_tc_t},u_{l_tl_t}<0$.[56] The function z can be seen as a loss-gain function: not fulfilling one's aspirations reduces welfare, so $z(c_t-a_t)\leq 0$ if $c_t\leq a_t$. Or equivalently, starting from below and getting closer to one's goal increases one's utility. Overachieving, when $c_t>a_t$, is assumed to be adding utility or $z(c_t-a_t)>0$. This loss-gain function is assumed to be increasing and concave in c_t, i.e. $z_{c_t}>0$, $z_{c_tc_t}\leq 0$.

We consider a unitary household, with an infinite time horizon, maximising discounted lifetime utility at each moment t, $W_t=\sum_{s=0}^{\infty}\beta^s v(c_{t+s},l_{t+s};a_{t+s})$, with the discount factor being $0<\beta\leq 1$. At the start of each period t, the household has revenue y_t and assets A_t available, based on decisions at $t-1$. Total resources A_t+y_t in each period t can be allocated to either consumption or used to produce future revenue. Revenue at $t+1$ is obtained from allocating both effort $e_t=1-l_t$ and capital $k_t=A_t+y_t-c_t$ in period t. The transition equation for future

56. We use throughout the notation $\frac{\partial g(x_t)}{\partial x_t}=g_{x_t}$ for any function g.

revenue is $y_{t+1}=f(k_t,e_t)$, with $f_{k_t},f_{e_t}>0$ and $f_{k_tk_t},f_{e_te_t}<0$. Allowing for some depreciation δ from using capital, the transition equation for assets is $A_{t+1}=(1-\delta).k_t$.

Maximising W_t, subject to the two transition equations for revenue and assets defined for each period $t+s$, allows us to derive the following Euler equations from the first order conditions governing decisions about consumption c_t and leisure l_t:

$$u_{c_t}+z_{c_t}=\beta.(1+f_{k_t}-\delta).(u_{c_{t+1}}+z_{c_{t+1}}) \tag{2}$$

$$u_{l_t}=\beta.f_{e_t}.(u_{c_{t+1}}+z_{c_{t+1}}) \tag{3}$$

Equation (2) governs choices between consumption today versus saving and investing for future consumption; Equation (3) governs taking leisure today or putting in effort with a return tomorrow. These are familiar Euler equations, except for the terms defined by the loss function. Without the loss function, the model yields the standard intertemporal results, whereby the marginal utility of present consumption (or leisure) will equal the discounted marginal utility of future consumption generated from returns to savings (or effort).

A.2 Model predictions from a change in aspirations

The model predicts that a change in aspirations can affect decisions about consumption and leisure. If aspirations for the future (a_{t+1}) increase at t, current effort and/or investment will increase. The intuition is captured by considering how an increase in a_{t+1} affects the Euler equations. z is a concave function in its argument $(c_{t+1}-a_{t+1})$ for a given a_{t+1}. Thus $\frac{\partial z_{c_{t+1}}}{\partial a_{t+1}}>0$. For a given past level of aspirations, a_t, an increase in aspirations for the future, a_{t+1}, will boost the right-hand side of both Equation (2) and (3). For both equations to hold simultaneously after this change, the left-hand side of each equation needs to go up too and/or the other terms on the right hand side need to go down. To restore equality in Equation (3), a reduction in leisure today is required: investment in the future through effort will increase u_{l_t} and reduce the marginal product of labour f_{e_t}. To restore equality in Equation (2) the household will need to consume less, and save and invest more at t so that future consumption increases. In turn, this decrease in present consumption will increase the left-hand side of Equation (2), as consuming less at t will increase marginal utility u_{c_t}, as well as z_{c_t}. More savings will also reduce the marginal product of capital f_{k_t} on the right side of (2) and reduce $u_{c_{t+1}}$ until equality across both Equation (2) and (3) is restored.

It follows that someone with lower aspirations for the future will limit investment and effort relative to someone otherwise identical in all other characteristics but with higher aspirations.

The model yields a more ambiguous prediction on how an upward shift in aspirations would affect consumption. Equation (3) offers a rule for the path of consumption, not for the level in each period. Boosting aspirations will boost future wealth, as there is more incentive to shift

resources to the future for a given discount rate. In turn, increased future wealth will boost consumption at some point in the future. Given the stronger incentives to save and invest, whether higher aspirations will also lead to higher levels of consumption in the near future will depend on individual preferences, in particular the inter-temporal substitution elasticity and other features of the underlying utility function (Deaton, 1992). In particular, the change in aspirations for the future increases the opportunity cost of consuming today. This generates both a *income effect* – the value of lifetime assets increases because they yield higher returns in the future – and a *price effect* – the opportunity cost of consumption at any moment in time increases as well. The income effect allows for more consumption at any moment in time, but the price effect will encourage moving consumption to the future. Preferences will determine when the former will outweigh the latter across the consumption path.

There are some implications of the assumptions of our model. First, we remain agnostic about where the reference point comes from, beyond that it is not a decision variable, in line with standard treatments in behavioural economics. The main text offered links to the psychological and anthropological literature – suggesting links to past experiences, as well as norms and values. If the reference point could be set as part of the optimisation problem, then it follows that if there is a gain from overachieving, then, to maximise utility, the reference point would be set to be as low as possible, which would be a trivial result. We also abstract from any endogenous revision of aspirations within the model, such as in response to past attainment. Second, our assumptions imply a loss from underachieving, with marginal losses increasing for higher levels of underachievement. This setup is consistent with Dalton, Ghosal, and Mani (2016)'s assumptions for underachieving, while Genicot and Ray (2017) assume a gain from overachieving, i.e. when $c_t > a_t$, but no effect from underachieving (i.e. frustration does not come at a cost). Our assumption and these other formulations of utility around the reference point yield the same underlying intuition: if aspirations are low relative to what could be achieved, boosting aspirations will provide incentives to put in more effort.

Finally, we note the model does not make predictions on the behaviour of expectations. The close empirical relationship between the measures of aspirations and expectations in our data makes it difficult to distinguish these concepts from one another or draw any conclusions about how they relate. Theoretically, predictions about the effects of the videos on expectations are complex. Expectations may simply capture beliefs about the outcomes of economic choices. They then likely change only gradually in response to the videos: the videos affect aspirations, which drive changes in economic choices and outcomes, and these changes in choices and outcomes indirectly change expectations. On the other hand, expectations may be another proxy for reference points (Kőszegi and Rabin, 2006). They then have a dual role: they may be affected by the videos and 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. Our limited measures of expectations mean studying this is beyond the scope of this paper.

B Summary of documentaries and placebo

The treatment consisted of four documentaries about two men and two women. The four documentaries and an example of the placebo segments are described below. The videos are available at: https://www.youtube.com/channel/UCqfoNjCzt8YPjTRWQaMQfAg.

Beshir Malim Yisak, in the video "The Fast Journey"

Beshir Malim Yisak is a 27 year old farmer, married, with two children. He has no formal education but is considered a model farmer in the area for his achievements in a short period. Five years ago, in an area where most of the inhabitants usually breed cattle, Beshir started crop production. He consulted an agricultural expert in a local NGO about good farming practices and implemented everything he learned. He planted vegetables and sold them at the market, and bought a pair of oxen after a good harvest. Three years later, Beshir used savings to purchase a water pump from Addis Ababa, with the help of the agricultural extension agent. He rented additional land to expand his farm as he could water a larger area. He planted papaya, sugar cane and maize and increased his productivity by improving his soil fertility. He gradually built up a large herd of cattle. He started keeping bees for honey. During 2007, when tree planting was encouraged by village administrations, he produced and distributed seedlings to seven peasant associations and a local NGO in the area. Extension agents and fellow farmers speak of him as an innovator and hard worker.

Teyiba Abdella, in the video "The Life-Transforming Flour Trade"

Most people in Teyiba Abdella's district are involved in cultivating crops and livestock and in trade. Teyiba is engaged in both trade and farming. Her parents refused to bless her marriage, so Teyiba and her husband, Aliya Yousuf, started their married life with hardly any income or assets. Their fellow villagers contributed one birr each to help them start their life together. Using the neighbours' contributions as seed money, Teyiba began trading wheat flour. She used to walk three hours to market carrying 50 kilograms of flour on her back. A woman who owns a flour mill in the market town observed her efforts and offered her credit to purchase flour. After selling the flour she obtained on credit, Teyiba paid back her debt and saved her profits. Because she paid her debts on time, the miller started giving her up to 100 kilograms of flour on credit. Teyiba also began trading eggs and chickens and bought a donkey to carry loads to the market. Then she and her husband opened their own shop. They built themselves a house and bought land in the nearby village to build another house. Teyiba's husband does most household chores while she runs the businesses. Other villagers used to criticise Teyiba for being the main breadwinner, but she rejected their criticisms. People in the village now have a high regard for her hard work and commitment. Teyiba's husband admires her strength and believes she is a great role model for people in their village.

Ayelech Fikre, in the video "Immortal Treasure"

Ayelech Fikre inherited only a hectare of flood-damaged, mountainous, rocky land from her father, who was disappointed she was a girl. The land had never been successfully cultivated before, making it nearly impossible to produce crops for sustenance. However, Ayelech refused to accept the limiting beliefs imposed on her by her father and developed huge ambitions for her land. At one point, she married a highly spiritual man who relied on prayer rather than labour and planned to sell her land, but she was able to divorce him and work the land independently despite mockery from neighbours. She faced a severe flood that destroyed her land, but this motivated her to develop innovative soil conservation strategies. Though illiterate with no scientific training, Ayelech designed channels to control and utilize floodwaters for farming – the first in her area to do so. She built wells as "water banks" for water storage. Today, her restored land is a profitable model farm where she grows various crops including citrus trees. She says she can cultivate anything except salt and diesel. Ayelech now travels to other regions sharing expertise in crop production, animal husbandry, afforestation, and soil conservation. The FAO awarded her a certificate and financial prize for her achievements, and before his death, even her father recognized her accomplishments despite his initial doubts.

Waki Feyyera, in the video "The Exemplary Achievement"

Waki Feyyera is a farmer with eight children who began farming with half an acre of land and one ox. He has since expanded his holdings to 9.2 acres. Waki worked with agricultural development workers and completed training at an agricultural institute, learning newer techniques including soil conservation through crop shifting. His success led the local administration to provide him with additional farmland. He purchased a water pump enabling him to grow mangos, coffee, and sugarcane in his irrigated garden. During a severe drought, Waki continued farming and produced a large potato crop while other farmers were forced to leave. Waki adopted modern beekeeping, increasing yields fro less than four kilos to 40 kilos per hive. He also grew dairy and beef cattle herds, converting the dung into biogas for energy. Waki established a mill employing three local men and his farm employs permanent workers, including four boys he took into his family and sent to school. His exemplary farming made him socially and politically influential, leading to his election as local chairman. Waki attributes his success to education and aims to establish an agricultural centre for producing improved seeds and seedlings.

Example segment from placebo treatment

The clip's title "Boru Bari", literally meaning "Tomorrow Morning", is meant to suggest the idea that "tomorrow is another day". It is a humorous take on rural life. The main character describes his current life to a journalist. He says everything is great but he looks unhappy. When pushed, he explains the reason with great hesitation, albeit humorously: his wife is having an extra-marital affair. Like the documentaries, the segment is in Oromiffa.

C Data and measures

This section provides additional details on the construction of variables used in the paper.

C.1 Conversions from Ethiopian birr (ETB) to USD PPP

The survey collected data in Ethiopian birr (ETB) at the time of each survey. All monetary values in the tables and figures are displayed in 2016 USD PPP, to match the time of the five year endline. To convert baseline and six month follow-up values to 2016 prices, we divide the reported values in ETB by the monthly non-food national consumer price index (CPI) series (averaged over the months in which our survey took place and rebased so that it was equal to 1 in January 2016, the midpoint in our endline survey). We use the Central Statistical Authority publicly-available CPI reports.[57] For baseline, we divide the monetary values by 0.514. For midline, we divide the monetary values by 0.592.

To convert 2016 values to USD PPP, we use an exchange rate of 8.67 ETB per 1 USD PPP, the World Bank PPP conversion factor for private consumption in 2016. The price level ratio of PPP conversion factor (GDP) to ETB market exchange rate for 2016 was 0.41.[58]

C.2 Agricultural investment

C.2.1 Modern crop and livestock inputs

Spending on crop inputs includes expenditure on seeds (bartered or purchased), fertiliser, herbicides, tractor hire and other non-labour inputs in the last long rains season.[59]

To capture labour inputs in agriculture, we record the number of person-days of family and hired labour for crop agriculture in the last long rains season. This is collected by plot and crop and summed. To value this labour, we multiply by the median male wage for each village across all crop-related activities (i.e. seeding, planting, weeding, harvesting). Female wages are rarely measured, reflecting that most wage labour in agriculture is male. If a village wage is not available, we use the *kebele*-level (district-level) wage. If there is no *kebele*-level wage reported, we use the sample median of 50 ETB per day (about $5.76 PPP per day) for that *kebele*.

Spending on livestock and poultry inputs includes expenditure on the purchase of inputs required for livestock in the past 12 months: feed, veterinary supplies, and hired labour.

C.2.2 Land

Total land area under cultivation is the area cultivated by the household across all plots in the last long rainy season. It excludes land rented out but includes land rented or sharecropped in. Areas are given in local units and converted into hectares.

57. `https://web.archive.org/web/20191115152931/http://www.csa.gov.et/price-indices/consumer-price-index/category/109-cpi-2016?limitstart=0`, accessed 17/08/2021.

58. `https://data.worldbank.org/topic/economy-and-growth?view=chart`, accessed 27/08/2019.

59. We only have prices for seed purchases by some households. We use either the household-level purchase price or, if not available, the sample level median of seed price. We use the price of white teff seeds for tikur teff, grass pea for cow peas, zengada for oats, and an average of wheat and barley seeds for wasira.

C.2.3 Labour supply

Labour supply and time allocation were measured using a household roster, completed by the household head for all household members for a typical day. Minutes worked includes work on the family farm, looking after livestock or other family business, as well as paid work for someone not in the household. Minutes of leisure include play time or general leisure (including time taken eating, drinking, bathing, sleeping). The category not reported involves all domestic chores (such as fetching water, firewood, cleaning, cooking, washing, shopping) and caring for others, including children and ill household members. For endline, for example, the household head was asked for each household member: "Typically, during October 2015 (*Tikimt* 2008 Ethiopian Calendar), how many hours per day did [household member] spend on the following activities?". We collect fractions of an hour, so express the total time in minutes per adult per day. As we expect the household head to be most knowledgeable about themselves and their spouse, we only include their data. Labour supply effects are robust to including all adult members of the household (available upon request).

Time allocation is collected for a period close to the survey time to minimise recall bias. It is collected for October for the five-year follow-up. This is the harvest period, a busy time on the farm. It is collected in March for the six-month follow-up. This is during the short rains, when there are activities on the farm (such as land preparation and pasture regeneration) but likely less intense than during the harvest period.

C.2.4 Livestock

The value of livestock and poultry is the sum of the value of all livestock varieties owned by the household. We construct prices using the sale prices reported by households for each variety of livestock. If the household has not sold the type of livestock it owns in the last 12 months, we use the first available median of sale prices from other households at the village, *kebele* or screening site level. If a price is above the 99th percentile, we replace it with the first available of the median price at the village, *kebele* or screening site level.

C.3 Educational investments

We analyse education outcomes for two main groups of children, defined by whether they were of primary or above-primary school age. "Cohort 1" comprises children aged 16-20 at the five-year follow-up (aged 11-15 at the time of the intervention). "Cohort 2" includes children aged 7-15 at the five-year follow-up (aged 2-10 at the time of the intervention), some of whom were of school-going age. At the six-month follow-up, we focus on the subset of Cohort 2 aged above seven at that time, for whom educational outcomes were collected ("Cohort 2a"). Panel B of Appendix Figure A.2 shows the cohorts at the time of the surveys. We examine all households in the sample, including 71 households who have no children in these age group in any of the rounds, to ensure the sample is comparable with other results. Results remain unchanged if we restrict the sample only to households with children in those age-groups (available upon request).

C.3.1 School enrollment

In each survey round, school enrollment is measured if the child was reported to have enrolled in school at the beginning of the school year. The baseline asks about enrolment at the beginning of the 2009/2010 school year, the six-month follow-up refers to 2010/2011, and the five-year follow-up to 2014/2015. We count the number of children enrolled for each household in a specific cohort.

C.3.2 School attainment

We measure attainment if the child is reported to have upper primary school if they have completed Grade 8. We count the number of children that attained upper primary school for each household in a specific cohort.

C.3.3 Time in school and time enrolled

Daily minutes of an activity are the sum of schooling-age household members' spent on that activity. In each round, respondents report the number of hours per day the child typically spent on different activities. We use two measures: (i) hours spent at formal school (including travel) and (ii) hours studying outside school (homework or extra tuition). The reference periods are March 2010 at baseline, March 2011 at the six-month follow-up, and October 2015 at the five-year follow-up. We add up those hours for all children in a specific cohort.

C.3.4 Schooling expenditures

At baseline and the six-month follow-up, households report total spending between September and December on uniforms, stationery, textbooks, and school fees or donations. At the five-year follow-up, they report the amount spent on any school-related items in the past 12 months.

C.4 Beliefs, preferences, and information

C.4.1 Time preferences

We use the measurement tool from Ashraf, Karlan, and Yin (2006), in a similar context with participants with low literacy levels. We ask individuals to consider a situation in which they were about to receive a gift. They are first asked three questions in the "near-term" frame:

1. Would they prefer the gift of 100 ETB today or could instead choose to receive a gift of 125 ETB in one month?
2. If they answer 100 ETB to question 1, they are asked if they prefer 100 ETB today or 150 ETB in one month.
3. Individuals are then asked how much they would need to receive to wait one month for the payment instead of receiving 100 ETB today, with a ceiling of 1,000 ETB, implying a discount factor of at least 0.1.

We create three indicator variables as crude measures of an individual's discount rate, the extent to which they discount rewards when they are in the future:

- Patient: Individuals who select 125 ETB over 100 ETB in Question 1.

- Slightly Impatient: Individuals who select 150 ETB over 100 ETB in Question 2 (but did not select 125 ETB over 100 ETB).
- Very Impatient: Individuals who need to receive over 150 ETB.[60]

We also capture whether individuals' choices are consistent with quasi-hyperbolic discounting (rather than exponential discounting). We ask the first two questions, but over a more *distant* time frame (one vs two months). As in Ashraf, Karlan, and Yin (2006), we create two indicators. Those who are coded as "Present-biased" or "Hyperbolic" choose the immediate reward in the near term frame and the delayed reward in the distant frame. Those who are coded as "Patient now and impatient later" choose the delayed reward in the near term frame and the immediate reward in the distant frame. This could arise if individuals have funds now, but think it is likely they will be liquidity-constrained in two months time (for example, due to seasonality). The proportion of our sample (in the control group) who are present biased is 34 per cent six months after the baseline, compared to 28 per cent in Ashraf, Karlan, and Yin (2006), and the proportion who are "Patient now and impatient later" is 22 per cent, compared to 20 per cent. The remaining 44 per cent of our sample gave responses consistent with exponential discounting.

We note increases in the portion of the sample who are impatient over time (it increases from 68 to 80 per cent over five years) and who are present-biased (from 34 to 53 per cent). This could be because we neglected to alter our measures to account for inflation. The increase in impatience is consistent with the monetary reward for waiting being worth less in real terms at endline than at the six-months follow-up. This would likely affect all treatment groups similarly, so should not jeopardise estimation of treatment effects.

While measures of risk and time preferences are based on hypothetical questions, not incentivised measures, recent work suggests this does not affect answers (Ubfal, 2016; Falk et al., 2018).

C.4.2 Risk preferences

We use a survey-based measure of risk preferences based on Binswanger (1980). In the main measure presented in text, we ask participants about a hypothetical maize sale. We ask which of five hypothetical payments respondents would choose for this maize, if the payout was determined by a coin toss. In the first payout, they would be certain to be paid 300 ETB for one 50kg bag of maize. In the second, they would have an equal chance of receiving 200 ETB or 400 ETB. After that, there are three more payouts, which increase in both mean and variance, as shown in Table A.7. We treat this choice as a categorical variable, with values of 1 for those who made the most risk averse choice and 5 for those who chose the most risk neutral to risk loving option.[61]

60. We recode 47 observations over the three rounds who give inconsistent answers as missing. They prefer 100 ETB in the first two questions but choose less than 150 ETB in one month for the third question. We view them as misunderstanding the question.

61. We prefer this to estimating risk preference parameters assuming a specific functional form for the utility function, as this relies on all households making decisions under uncertainty in the same way.

C.4.3 Locus of Control, Perceptions of Poverty, and Grit

All measures take values from 1 ("*Strongly Disagree*") to 4 ("*Strongly Agree*"). Scores are the sum of items in the scale, standardised and reported as z scores. For all scales, we assess the reliability of each item. Items that met any of the following criteria were removed: low corrected item-total correlation (0.25); increased Cronbach's α if item removed; low item variation (80 per cent identical responses on the item). If respondents did not answer all items in a sub-scale, we code the items they do not answer as missing and adjust their score to generate a homogeneous score range using an appropriate multiplier. However, if a respondent is missing over 60 per cent of the items of a sub-scale or has given the same answer to all items on the scale, we replace the sub-scale score as missing.

Locus of control: We construct two sub-scales from a subset of items from the Internal Powerful Others Chance (IPC) scale (Levenson, 1981). Higher values on the Internal scale indicate that respondents see outcomes as contingent on individual behaviour. Higher values on the Chance scale indicate that respondents see outcomes as a result of chance, luck or fate (chance locus of control).

Causes of poverty: Similarly, we construct two sub-scales of the Attributions for Poverty scale (Feagin, 1975) which capture if individuals use Individualistic or Fatalistic explanations for poverty. Higher values on the Individualistic scale mean individuals attribute poverty to individual characteristics. Higher values on the Fatalistic scale mean individuals attribute poverty to chance or luck.

Grit: We construct a standardised index of grit from two measures in the vein of Alan, Boneva, and Ertac (2019). The first asks level of agreement with the statement "I do a thorough job". The second asks level of agreement with the statement "I make plans and follow through with them".

C.4.4 Information

We explore if farmers take the same actions shown by subjects of the documentaries. We construct an inverse-covariance-weighted average following Anderson (2008), standardised relative to the within-village control group. This index comprises the following variables, each equal to 1 (and 0 otherwise) if the household:

1. earns any income from trading
2. attends community meetings to discuss agricultural issues
3. receives or seeks visits by an agricultural expert
4. takes advice on land preparation from agricultural expert
5. takes advice on seeds from agricultural expert
6. takes advice on fertilisers from agricultural expert
7. number of visits received by an agricultural extension worker (the only non-binary component of the index)

8. visited a demonstration plot in last year
9. grows cash crops
10. uses a water pump
11. builds stone bands and terracing
12. applies water conservation/water harvesting practices
13. applies crop rotation
14. uses any irrigation technique
15. uses cattle in crop activities

C.4.5 Expected fertiliser yields:

We elicit expectations about the increase in output from the use of modern (phosphate-based) fertilisers. We asked a list of questions to the household head to elicit how many kilograms of output they would expect to produce on an hectare of their land if 0, 50, 100, 150 kilograms of fertiliser were applied. We varied whether the hypothetical season was good or bad (in terms of agronomic conditions) and whether the crop produced was maize or sorghum. To combine these answers, we first estimate the elasticity of expected output relative to fertiliser by regressing the answers to these questions on the four quantities of fertiliser for each respondent, by crop and hypothetical season, to generate four expected yields per respondent (i.e. expected yield from an extra kilo of fertiliser in a good/bad season for sorghum/maize). Next, we combine these four expected yield estimates into a single index, following Anderson (2008).

C.5 Consumption and well-being

C.5.1 Consumption

All consumption variables are constructed in USD PPP and transformed into adult equivalent units, where adult equivalent household members are constructed using the OECD scale. The household head is 1, each other adult is weighted 0.7 and each child under 16 is weighted 0.5.

Food consumption is the sum of the value of food consumed from various sources over the past 7 days divided by 7 and multiplied by 30 to obtain a monthly estimate. This includes food purchased, received via barter, gifts, loans, wages in kind and self-production. Following Beegle et al. (2012), for purchased food items, we use reported prices, and for food received via barter, gifts, loans and wages in kind and self-produced food items, we construct prices using the first available level of price of purchased food from the following: household-level price, screening site level median, median from the neighbouring *kebele*, sample level median.

Non-food small-item consumption is the sum of frequent non-food consumption, with a recall period of one month. Items included are: toiletries, transportation costs, mobile phone costs, energy, cigarettes and tobacco, repair, tailor, barber, other services and other small purchases (less than 100 ETB, or $11.5 PPP).

Non-food lumpy consumption is the sum of expenses made over the past 12 months (divided by 12 to obtain monthly estimates), from the following list of items: clothing and footwear,

utensils, beddings, school expenses, health expenses, funerals, weddings, religious expenses, contribution to community projects, land taxes and other large purchases (more than 100 ETB, or $11.5 PPP).

C.5.2 Subjective well-being

The subjective well-being is measured using two items indicating best and happiest life. Best life is measured by showing respondents a picture of a ladder with 10 steps (Cantril, 1966). They are told the top of the ladder represents the best possible life for them and the bottom step represents the worst possible. They are then asked, "*Where on the ladder do you feel you personally stand at present?*" The above question was repeated to measure happiest life, with the top and bottom of the ladder representing the happiest and least happy possible life.

C.6 Savings, credit and revenue

Savings and credit

We define an individual's savings as the total cash savings each respondent has in all possible savings places: banks, co-operatives, voluntary savings and loan groups, traditional *iqqub* (a version of a rotating credit and savings association), or with a friend or relative. Savings were captured individually for the household head and their spouse and are analysed at individual level, as men and women commonly save some amounts separately both within the household and in savings groups. Results are robust to aggregating all savings across the household and conducting analysis at household level.

To measure credit, we ask about the total value of loans larger than 15 ETB in nominal values (1.7 USD PPP), including from co-operatives, banks or micro-finance institutions, money-lenders, *iqqub* and friends or family. We consider only the value of the loan when it was first taken out.

Revenue

The revenue aggregate includes revenues received from wage labour, livestock and poultry, crops, non-farm activities and transfers and remittances.

Revenue from crops is computed as price times quantity sold in the last agricultural season. We capture production in both the short and long rains seasons. Quantities produced were replaced to missing if the yield per crop was higher than designated cut-off values.[62] We use Central Statistical Agency (CSA) data from the Oromiya region for December 2015 and January 2016 to compute the price of crops. The process for computing prices is:

1. Of the 42 crops in our dataset, we were able to find exact matches for 25 crops: White teff, maize, zengada, horse beans, groundnuts, sesame, lentils, chat, bananas, potatoes,

62. Cut-off values are listed in brackets for each crop: wheat (2 tons/ha), bananas(2 tons/ha), white teff (2 tons/ha), horse beans (1.6 tons/ha), coffee (1.6 tons/ha), barley (1.8 tons/ha), sorghum (3.2 tons/ha), maize (4 tons/ha), sesame (1.5 tons/ha), lentils (1.5 tons/ha), chat (2.5 tons/ha), haricot beans (3.5 tons/ha), onions (10 tons/ha), sugar cane (37 tons/ha).

sugar cane, pineapple, avocado, onion, spinach, garlic, chickpeas, cow peas, orange, godere, sweet potato, tomato, haricot beans, fenugreek, green pepper

2. For six crops, there were more varieties in the CSA data than in our questionnaire, hence we take an average of the prices for the various varieties for each crop: sergegna teff, barley, wheat, sorghum, linseed and coffee.
3. For the following seven crops, we could not find exact matches or varieties in the CSA data, and used the best available approximation for these crops (listed in brackets): sinar/gerima (oats), adenguare (chick peas), red pepper (green pepper), wasira (average of wheat and barley), guaya/grass peas (white peas), finger millet (African millets) and tobacco (chat).
4. We could not find price observations in the Oromiya region for the following crops, and hence used the price data points from other regions: mango (Tigray region) and oats and sunflower seeds (Amhara region).

Revenue from livestock rearing and produce includes all revenues from sale of livestock and poultry and the production of livestock goods in the last 12 months. We use the self-reported sale prices to construct the livestock revenue variable. We include rental out of livestock, dairy, wool, egg sales.

Revenue from wage labour is the sum of agricultural and non-agricultural wages received by the household over the past 12 months. Non-farm revenue is the number of months an activity was operated for times the monthly revenue from this activity, for a list of common activities. Transfer revenues are revenues from all sources of transfers: pension, remittances, public works and other assistance programs, and other transfers e.g. burial associations, in the last 12 months. It is computed as the number of months the household received transfers from this source times the average monthly transfer from this source.

D Deviations from the Pre-Analysis Plan (PAP)

This study was pre-registered on the AEA RCT Registry (ID: AEARCTR-0001483) under the title "The Future in Mind: Aspirations and Forward-Looking Behaviour in the Short and Long Run in Rural Ethiopia." Pre-registration occurred on February 15, 2017. Pre-registration was only for the analysis of the long-run data.

We conducted the treatment and six-month follow-up in 2010-11 (see timeline in Figure A.2). Analysis in the working paper reporting on the short run data (Bernard et al., 2014) was not pre-specified, as pre-analysis plans were not yet common. The AER registry was set up in 2013 and papers like Olken (2015) or Casey, Glennerster, and Miguel (2012) were not yet published and only just circulating. So the intervention's effects on aspirations and outcomes measured in the short run were already known and publicly available when prespecifying analysis in the long run. We then conducted data collection for the five-year follow-up data (from December 2015 to January 2016). Pre-registration occurred after data collection but before analysis of the five-year follow-up data.

We have made some changes in analysis relative to the pre-analysis plan. We show all results as pre-specified, either in this appendix or Appendix E or F, and changes do not materially affect our results.

First, the main tables now report ANCOVA results for most outcome variables, those for which we collected baseline values. The pre-specified specification omitted baseline outcomes as controls. We report robustness tests for this change in Appendix E. As discussed in the main text, there are only four coefficients where the ANCOVA and PAP specifications differ: the treatment vs placebo effect (i) on the value of livestock and (ii) on the number of children aged 16-20 in school, and the treatment vs control effect on (iii) time spent in school for children aged 7-15 and (iv) on having an organic roof. These all lose significance in the ANCOVA specification relative to the pre-specified specification, where they are marginally significant.

Second, we now apply the same specification for the six-month and five-year follow-up analysis, an ANCOVA specification comparing treatment, placebo and within-village control groups. We had pre-specified different specifications for the two follow-ups. For the six-month follow-up, we pre-specified a specification comparing treatment, placebo and within-village control groups, although without controlling for the baseline value of the outcome. This is shown in Appendix Section E, as discussed above. For the five-year follow-up, we specified the regression we run to test for spillovers (Equation (4) in Appendix F) as the main specification. This compares treatment, placebo and within-village control groups to the pure control group. We have reduced the emphasis on tests for spillovers in the main paper so do not use this as our main specification. The results with this alternative control group are shown in Appendix Section F: the main treatment effects and (lack of) placebo effects are robust regardless of which control group is used.[63]

Third, the PAP pre-specified trimming observations four standard deviations above the sample mean for continuous outcome variables, a non-standard approach used in the short run working paper (Bernard et al., 2014). We revised this to trim values above the 99th percentile. We do not show results for the pre-specified approach, as it is non-standard.

The fourth and fifth changes are outlined in detail in the sections below. Fourth, we made some minor adjustments to the pre-specified construction of aspirations, expectations, education, risk preference and time preference variables, none of which change results. We also added six variables and dropped five variables, outlined and justified below in Section D.1. We note if variables are not pre-specified in footnotes in the main text.

Fifth, for multiple hypothesis test correction, we group outcomes using a slightly different (and hopefully clearer) organising framework, but without excluding pre-specified outcome variables (beyond a few specific cases explained below).

At the end of this section, we report results as laid out in the preanalysis plan, showing that the deviations do not affect our substantive conclusions. Importantly, coefficients and sample sizes are very slightly different between the PAP results and those in the paper. The PAP results use the pre-specified non-ANCOVA specification, so observations which do not have baseline values of the outcome are not dropped, while the paper results use an ANCOVA specification. In a few cases variable construction differs.

63. We note that the PAP inaccurately stated that the pure control villages were randomly selected. In fact, these villages were selected based on logistics, as described in Section 5.5.

D.1 Changes to pre-specified outcome variables

Education

- We had specified analysing effects on children aged between 6 and 15 at endline (Cohort 2). We instead report effects on children aged between 7 and 15 at endline as children can only start school at 7. There are no effects on the number of children enrolled whether we consider children aged 6-15 (Table A.13, constructed as in the PAP, at the end of this appendix) or 7-15 (Table 3, paper).
- We added a variable for whether children aged between 16 and 20 at endline (Cohort 1) had completed eighth grade.
- We omitted a measure of absenteeism from school due to data quality issues.

Investment

- We added an analysis of extensive margin investment in inputs (in line with pre-specified extensive margin analysis for savings and credit).
- We dropped land area rented or sharecropped as there was little variation. Land is not privately owned and at the time of the study, there was still ambiguity in this area about whether land rental was legal (Deininger, Ali, and Alemu, 2008).

Welfare

- We added variables capturing the value of the house and indicators for having a non-organic roof and own toilet (which were directly observed by enumerators).

Aspirations and expectations

- We do not report a pre-specified dimension of aspirations and expectations, related to social status, and exclude it from the aspirations and expectations indices. Bernard and Taffesse (2014) find challenges with internal reliability of this measure during their validation study. They found a correlation coefficient of only 8 per cent when the same households were twice asked questions on aspirations on social status within a two-week period, considerably worse than for aspirations and expectations for income, wealth or education, potentially indicating a lack of understanding of the concept.[64]
- In Figure II and elsewhere in the paper, we construct the aspirations and expectations indices using the same methodology as the other summary indices, following Anderson (2008), and not using subjective weights for the importance of each dimension to respondents, as we pre-specified, to enable comparison across indices of the magnitude of effects.
- Appendix Table A.12 shows effects on the aspirations and expectations indices, constructed as per the PAP. Effects are positive and statistically significant regardless of index construction. Effects with pre-specified variable construction are slightly smaller than effects in Table A.1 in the paper.

Alternative mechanisms

64. Beaman et al. (2012) also dropped a dimension with lower internal reliability from their index of measures of aspirations.

- We improved the construction of risk and time preferences measures in line with current practice in the behavioural literature.
  - For *risk preferences*, we ask participants which of five hypothetical payments they would choose for a hypothetical maize sale, if the payout was determined by a coin toss. The first payout is certain, then payouts increase in both mean and variance. In the paper, we report risk preferences as a categorical variable from 1 to 5, where 5 is the least risk averse. We pre-specified an indicator equal to one if the respondent answered 5, which loses some information.
  - We dropped a second measure, a gamble, where individuals bet on the outcome when someone flips a coin. Results are similar using this measure or the maize-related one.
  - For *time preferences*, we used a simple questionnaire from Ashraf, Karlan, and Yin (2006) for populations with limited numeracy, as described in Section C.4.1. In the paper, we construct the measure as in that paper. We pre-specified an alternative construction of the measure which makes stronger assumptions[65] and also does not enable us to examine present bias.
  - The results do not change regardless of which method of constructing the variables we use: results in Table 6 in the paper and Appendix Table A.15 using the PAP definitions are very similar, with no effects of the documentaries on these preferences.
- We dropped two pre-specified indices which combined, first, the internal locus of control plus individual causes of poverty index, and, second, the chance locus of control plus fate causes of poverty index. There were only two variables in each index and we felt they did not add information. Table A.12 shows there are no effects on either index.
- We added an adapted measure of grit, perseverance and passion for long-term goals (Duckworth et al., 2007; Alan, Boneva, and Ertac, 2019), which has become prominent in the economics literature since the PAP.
- We simplified the construction of the information index capturing if households mimicked activities shown in the videos. The PAP contained errors in describing the information index. (i) Four variables (grain milling, using a pump, an extension advice sub-index, using irrigation) were listed twice, and one variable (growing maize, teff, mango, sugarcane, coffee) was almost exactly the same as another one (growing cash crops). (ii) We pre-specified including variables related to whether households took up extension advice in a sub-index, but then specified including this index within the information index alongside other dummy variables. Instead, we construct the index from all pre-specified dummy variables reflecting

65. The pre-specified measure was to construct the subjective discount factor $\beta=1/(1+\delta)$, where δ is the rate of time preference. We ask if respondents would prefer receiving 100 ETB now or 125 ETB in one month. To those who chose 125 ETB, one ETB in one month is worth between 0.8 and 1 ETB today: they have a monthly discount actor between 1 and 0.8. The measure assigns them the mid-point of 0.9. If they chose 100 ETB, they then choose between 100 ETB now or 150 ETB in one month. If they choose 150 ETB, they have a monthly discount factor between 0.8 and 0.667 and are assigned the midpoint of 0.733, and so on for later choices. This measurement assumes a linear utility function, and will estimate a discount rate which is biased upwards (and a discount factor which is downward biased) if the function is concave.

if households undertook any behaviours shown in the videos.

D.2 Multiple inference adjustment

Current approach: We adjust naive p-values as follows. We group related variables within table panels. A table panel corresponds to a concept in our conceptual framework, such as labour effort, agricultural investment, productive assets, non-productive durables and housing assets, or consumption. We view the group of variables within a panel as testing the same underlying hypothesis. To correct for multiple testing, we use the Benjamini, Krieger, and Yekutieli (2006) resampling procedure to calculate sharpened q-values which correct p-values for multiple tests across outcomes within each panel.

To summarise impacts, we report impact estimates on standardised inverse-covariance-weighted indices (Anderson, 2008) constructed from all outcomes reported in a table in our main exhibits. This results in five main indices, capturing agricultural investment (Table 3), educational investment (Table 4), welfare (Table 5), aspirations, and expectations (Figure II), plus an omnibus index. Following Kling, Liebman, and Katz (2007), we also aggregate the standardised indices into a single omnibus index (Table 7).

Pre-specified approach: The PAP pre-specified the omnibus index and five hypotheses, three primary and two secondary. These largely map onto the five indices tested in the paper. The primary hypotheses and variable groups were:

(H1) The intervention will increase self-beliefs (group 1: aspirations; group 2: expectations; group 3: locus of control; group 4: beliefs about causes of poverty);

(H2) The intervention will increase labour supply and human capital investments (group 5: labour supply; group 6: educational investment);

(H3) The intervention will increase forward-looking economic behaviour (group 7: savings and credit behaviours; group 8: investment in inputs).

The secondary hypotheses and groups were:

(H4) The intervention will not affect other psychological channels (group 9: risk aversion, discount rates, information transmission by adopting activities in videos);

(H5) The intervention will affect household welfare (group 10: consumption; group 11: food security; group 12: asset stock; group 13: subjective well-being; group 14: revenue).

Deviations from PAP: First, we use the approach of creating summary indices plus an omnibus index to summarise results on multiple similar variables, which reflects current practice in development economics. The PAP had proposed the omnibus index, but proposed selection of one focal outcome per variable group as a summary outcome, a method which is not widely used.

Second, we slightly alter which variables are grouped together in tables and hence indices. We realised groupings could be better mapped to standard concepts in psychology and economics:

- On further reading of the psychology literature (Bandura, 2001), locus of control and beliefs about causes of poverty (groups 3 and 4) are different psycho-social concepts than

aspirations and its foundations. So we discuss them as alternative psychological channels i.e. they are now in Hypothesis 4, where we pre-specified them as being in Hypothesis 1.

- Labour supply, spending on inputs, and use of land are all inputs into productive activities so should be grouped together, i.e. labour supply (group 5) is now part of Hypothesis 3, where we had pre-specified it as part of Hypothesis 2.
- Productive assets (livestock and productive durables, i.e. tools, part of group 12) are an input into production and should be grouped with other inputs in Hypothesis 3. We had pre-specified all assets as part of Hypothesis 5.
- Savings and credit behaviours (group 7) are not an input into productive activity in the same way as other inputs in Hypothesis 3. Most variables we specified were about financial behaviours rather than flows. So we keep these variables as a separate group.
- Revenue (group 14) is not commonly used as an indicator of living standards in the same way as consumption, food security, assets or wellbeing. So we keep these variables as a separate group.

But other than these changes, the main exhibits map fairly closely onto these pre-specified hypotheses. Figure II on aspirations and expectations shows most variables in Hypothesis 1; Table 4 shows education variables for Hypothesis 2; Table 3 captures variables on investment in inputs from Hypothesis 3 plus labour supply and productive assets; Table 5 on welfare shows variables related to Hypothesis 5; and Table 6 on alternative mechanisms shows variables related to Hypothesis 4. Savings and credit behaviours (group 7) are shown in Appendix Table A.3 and revenue (group 14) is shown in Appendix Table A.5), with results described in text.

E Robustness tests

The section presents estimates from two alternative specifications, our pre-specified "non-ANCOVA" specification and one selecting controls via post double selection LASSO. Columns 1 and 2 report the estimates in the main paper for comparison (Equation (1)).

Columns 3 and 4 report our pre-specified "non-ANCOVA" specification. This is the same as Equation (1), except it does not control for the baseline value of the outcome, as we do not have baseline data for all variables. This specification maintains closer comparability across outcomes and with the comparison to pure control villages (where we do not have baseline outcomes). For some variables, the first and second columns are blank, because we do not have baseline values of the outcome variables and cannot run an ANCOVA specification. For those outcomes, the main tables show the pre-specified specification (without controls for baseline outcomes), which appears here in Columns 3 and 4.

Columns 5 and 6 report a specification selecting controls via post double selection LASSO (Belloni, Chernozhukov, and Hansen, 2014), where all variables in Tables A.9 and A.10 can be selected as controls. These variables include pre-specified controls X_{vi1}, other demographic variables, and baseline values of all outcomes. We partial out village-fixed effects and, when available, the baseline value of the outcome, following the recommendation in Cilliers,

Elashmawy, and McKenzie (2024).

F Spillovers

Our main results use an individual-level randomisation in 64 villages. The control group live in villages where others receive either treatment or placebo. This subsection describes an alternative specification which tests for spillovers. We find little evidence of spillovers.

Spillover analysis specification: We compare households in 64 treated villages with households in 10 pure control villages, selected as described in Section 5.5:

$$y_{iv} = \delta^s T_{iv} + \rho^s P_{iv} + \varphi C_{iv} + X'_{iv0}\pi^s + \alpha^s_v + \varepsilon_{iv} \tag{4}$$

X_{iv0} are the same pre-specified controls as the main specification, Equation (1), which are likely time-invariant and allow us to include the values collected in the pure control group at the five-year follow-up. We also add a list of pre-specified village-level controls. α^s_v denotes screening-site fixed effects, which replace village-level fixed effects in Equation (1). C_{iv} is an indicator equal to one for within-village control households in treatment villages who were not invited to watch the documentary or the TV show. φ is the difference between the within-village control and the pure control group, which measures the extent to which control households in treatment villages were (indirectly) affected by the treatment. The superscript "s" is added to the parameters δ and ρ to distinguish them from the previous within-treatment village estimates. In this specification, these households are compared to the base group of households in pure control villages. Standard errors are clustered at the village level.

Lack of spillover effects: We find little systematic evidence of spillovers. There are significant differences between the within-village control group and pure control group on only one of five indices (Column 3, Table A.24). If anything, we find some evidence of positive spillovers for the welfare index, which could potentially attenuate our main estimates. Our estimates of treatment effects are very close to those in our main specifications although the control group is different (Columns 1, 2 and 4, Table A.24).

Balance tests across villages: Table A.25 reports balance tests on pre-specified village-level variables comparing the 64 treated and 10 untreated villages.[66] There are some differences between treatment and pure control villages, potentially because the status of treatment villages is not randomly allocated. While an omnibus tests does not reject the null hypothesis that treated and pure control villages are the same, the number of villages is small so our tests for balance are not strong. However, differences do not seem to reflect a systematic pattern, for

66. We exclude pre-specified variables with insufficient variation, such as post-primary school presence, electricity access (absent in all but one village), radio signal availability (nearly universal), the share of non-Orthodox Christians (0 in all but one village), and television ownership rates. We also included the five distance variables, which we had pre-specified as village-level controls, calculated from village coordinates rather than village-level surveys.

example that the treated villages are more prosperous. Pure control villages have less irrigated land and forests, are more likely to grow sorghum as the main crop, have a smaller share of non-agricultural business entrepreneurs, and are further away from a river.

G Cost effectiveness analysis

In this appendix, we report on the costs of the intervention, both at the study scale and at a reasonable implementation scale, and the implied benefit-cost ratio for a subset of outcomes proxying household overall wealth.

G.1 Cost of the intervention

At the scale of the study: The experiment was designed to ensure internal validity, not to limit costs, so per head costs at the scale of the experiment are higher than if the intervention were delivered at reasonable scale. The documentaries were produced by professional documentary makers at a total cost of $41,910.25 (USD 2016 PPP) but were only shown to 960 households. We treat these as the relevant *fixed costs* of the intervention *at the experiment's scale*. These documentaries were screened at 16 different sites across a large administrative area, to small audiences deliberately limited for the experiment, adding substantial staffing, transport and projection costs. At the scale of the intervention, we estimate these from our accounts as $17,990.16 (USD 2016 PPP). These are the relevant *variable costs at the experiment's scale*. Half of these costs were transport costs, and approximately a quarter each staffing costs and projection equipment hire costs. Per household, this suggests a variable cost of $18.73 (USD 2016 PPP) and a fixed cost of $43.64 (USD 2016 PPP), yielding a total cost per household at the scale of the experiment of $62.37 (USD 2016 PPP), shown in the first panel of Table A.26.

At reasonable scale: It is unlikely a policy intervention of this nature would be implemented at such small scale. These documentaries could in principle be shown to very large audiences, bringing these fixed cost per household down considerably. Variable costs could be reduced significantly by including larger fractions of the population in each village and hiring less skilled staff than survey enumerators (or using existing community-based workers).

We hence also show costs and implied benefit cost ratios at a "reasonable" scale, when some of the obvious scale economies are being exploited. We reference a cost-effectiveness analysis of an experiment implemented with the NGO Digital Green in Ethiopia in 2017, which uses video screenings facilitated by extension agents to disseminate agronomic content (Bernard et al., 2019). This experiment targeted 57,750 farmers, and had a variable cost per farmer of $9.47 in USD 2016 PPP. If our intervention had reached a similar scale to this larger experiment, the fixed cost per household would drop to $0.73 USD 2016 PPP. This yields a total cost per household at reasonable scale of $10.20 USD 2016 PPP. Remote digital delivery could significantly reduce costs, but our experiment did not assess this delivery channel, making it a promising area for further research.

G.2 Cost-effectiveness of the intervention

We focus on robust effects related to assets expressed in monetary terms, which proxy for overall lifetime household wealth. This likely underestimates benefits as several robustly significant outcomes are not expressed in monetary terms, such as educational attainment or the reduction in food insecurity.

We show three benefit-cost ratios: considering only gains in durables; considering gains in durables, livestock and productive assets; and considering gains in those three types of assets plus housing values. On average, households gain $20.71 USD 2016 PPP in durable assets, robust across specifications (Table 5 and A.21). Additional benefits in liquid assets, such as livestock ($112.75 USD 2016 PPP), and productive assets ($21.27 USD 2016 PPP) could be counted but these results are not significant in all specifications (Table 3 and A.19). Finally, there are significant benefits in terms of increased house values ($384.94 USD 2016 PPP, (Table 5 and A.21). We only count the gains after five years in values, not the likely future flows from the assets involved.[67] Table A.26 shows benefit-cost ratios for per household costs at the experiment's scale ($62.37 USD 2016 PPP) and at a reasonable scale of actual delivery ($10.20 USD 2016 PPP).

We conservatively estimate that, run at a reasonable scale, the intervention generates about a two dollars increase in durable assets per dollar spent (Column 2 and first row of second panel, Table A.26). The relatively high gains for housing drives the benefit-cost ratio up considerably, even at the experiment's scale.

67. Since our aim is to proxy for household lifetime wealth, we do not include consumption. At endline, we found a very small positive effect that was not statistically significant.

H Additional figures

Map of study

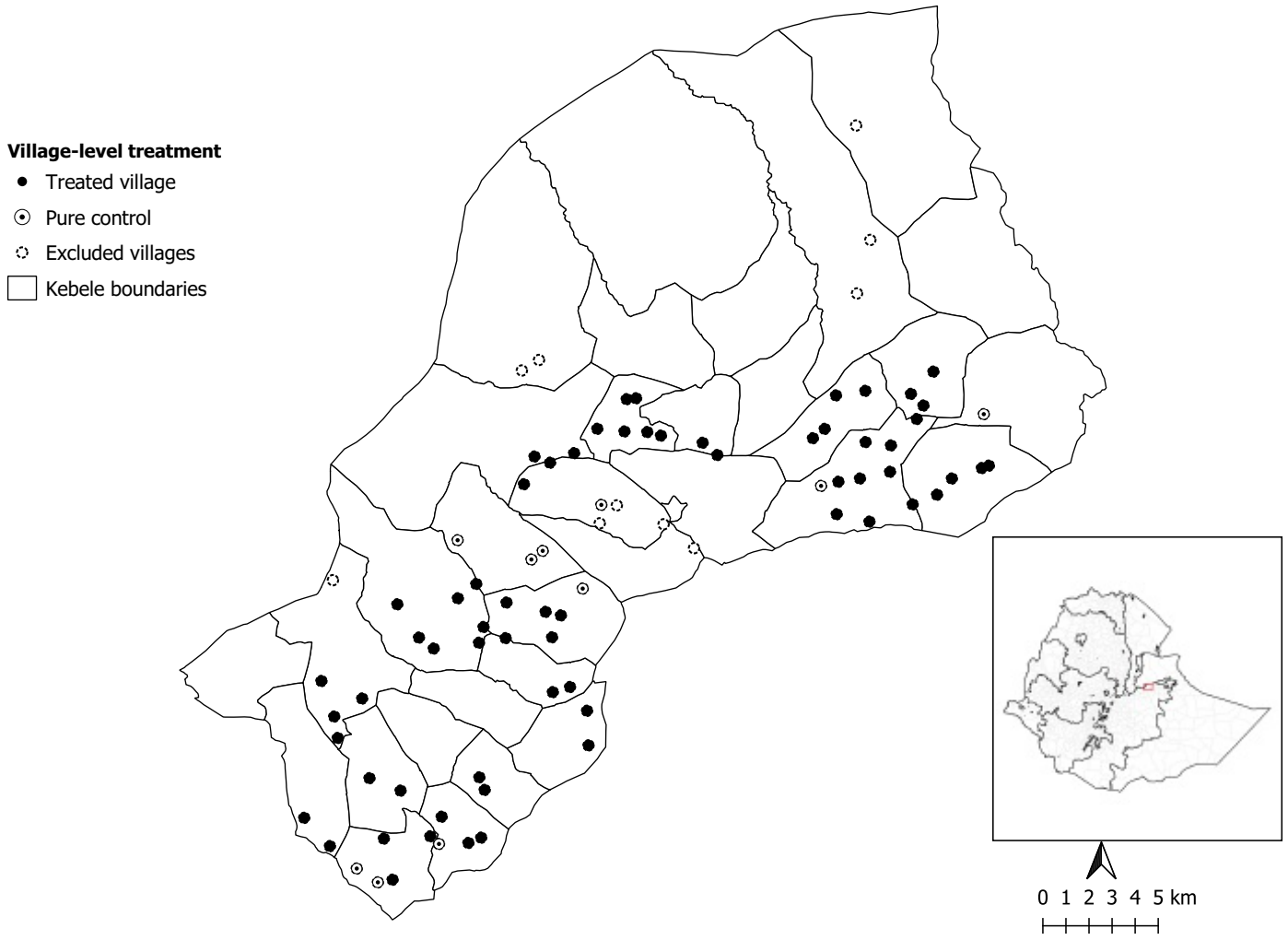
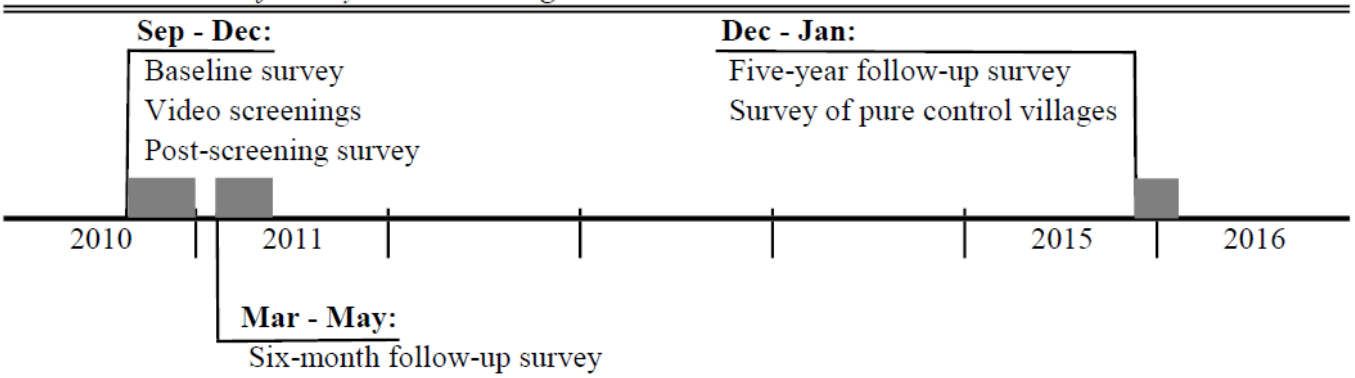


Figure A.1: Villages in the study

Notes: Black dots correspond to the treated villages. White and black dots corresponds to the pure control villages used in the spillover analysis. White dots correspond to the villages that were part of the original sampling frame but are not included in the analysis.

Study timeline

Panel A: Timeline of surveys and screenings:



Panel B: Schooling-age cohorts at the time of the surveys:

Cohort	*Age in 2010/11*	*Age in 2015/16*
1	11-15	16-20
2	2-10	7-15
2(a)	7-10	12-15

Figure A.2: Timeline of the study

Notes: Panel A shows the overall study timeline. Grey horizontal bars denote the periods where a survey or the screening intervention took place. Panel B shows the cohort ages of children between baseline and the five-year follow-up. These cohorts are used to define educational outcomes.

Experimental integrity

Design, sample, and compliance

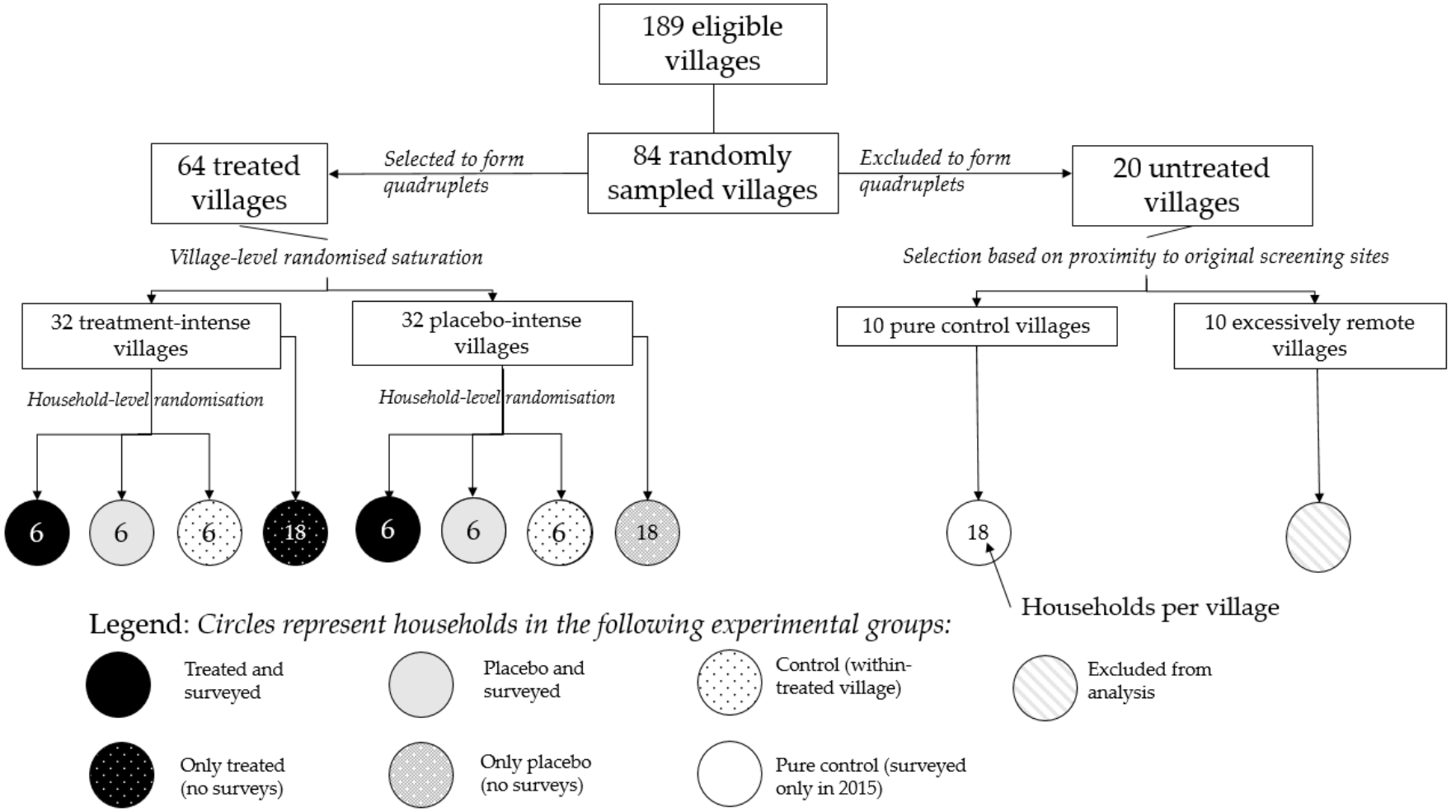


Figure A.3: Study design

Notes: Diagram of the sampling and randomisation into different experimental groups. Rectangles indicated villages, whereas the circles indicate households. Numbers inside the circle represent the number of households in each experimental group per village.

Heterogeneity

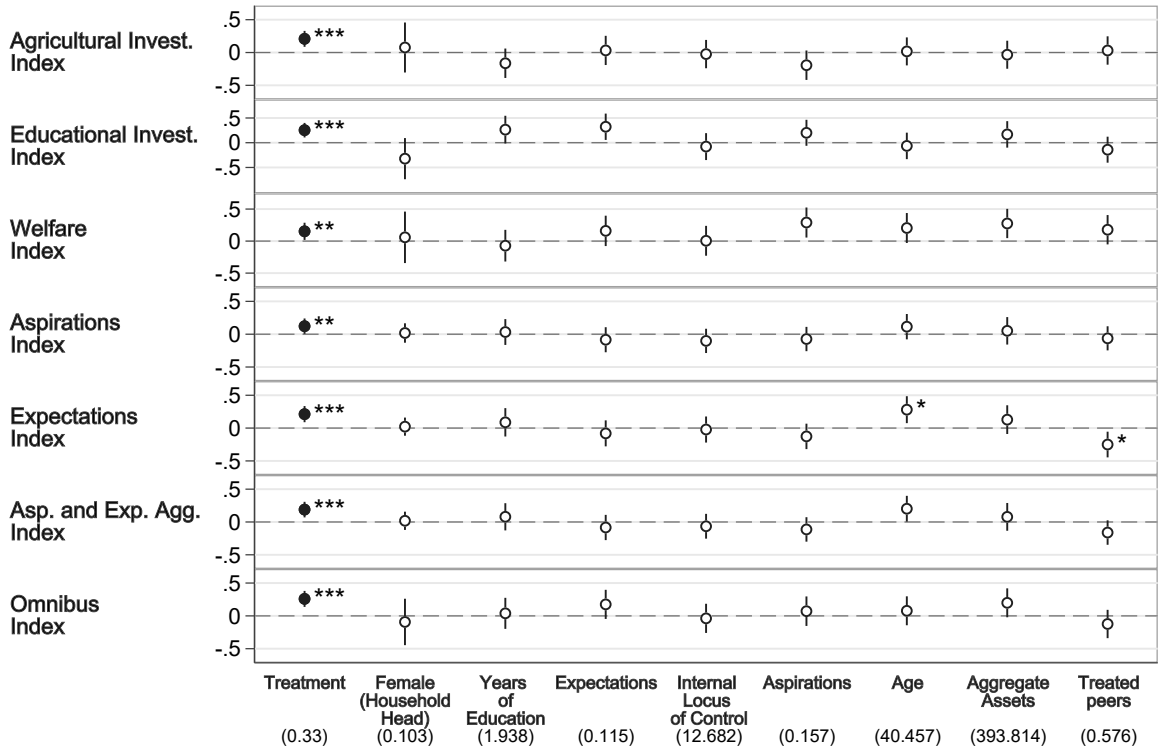


Figure A.4: Heterogeneous treatment effects on summary indices

Notes: Each coefficient represents a separate OLS regression using data from five years after the intervention. The outcomes are the summary indices in Table 7. The first column shows within-village intention-to-treat effects, controlling for a placebo-group indicator. The comparison group is households from the 64 treated villages not invited to any screening. In the second column, we control for gender. For individual-level indices (aspirations, expectations, and the combined aspirations-expectations index), we use an indicator for female respondents and its interaction with the treatment. For household-level indices, we use an indicator for female-headed households and its interaction with the treatment. Subsequent columns control for baseline values above the median and their interaction with the treatment indicator. For household-level outcomes (agricultural investment, educational investment, welfare, and omnibus), the dimension of baseline heterogeneity (on the x-axis) is reported by the household head. For individual-level outcomes (aspirations, expectations, and combined indices), the dimension of baseline heterogeneity is reported by the individual (spouse or household head). For aggregate assets, only household-level data are used to test differences based on whether household assets are above or below the median. The internal locus of control measure is detailed in Appendix Section C.4.3. Aggregate assets include non-productive assets, productive assets, savings, and livestock. "Treated peers" are close social connections invited to watch role model videos. Standard errors are robust to heteroskedasticity and clustered at the household level for the aspirations/expectations indices. Bars represent 95 per cent confidence intervals based on naive p-values. For heterogeneous effects, we use FDR-adjusted q-values for multiple testing corrections on interaction terms across outcomes, based on (Benjamini, Krieger, and Yekutieli, 2006). Stars indicate significance according to the minimum q-value for each hypothesis. We do not include FDR-adjusted q-values for multiple interactions across different dimensions, as we did not have the power for such tests.

Comparison of effect sizes with other interventions

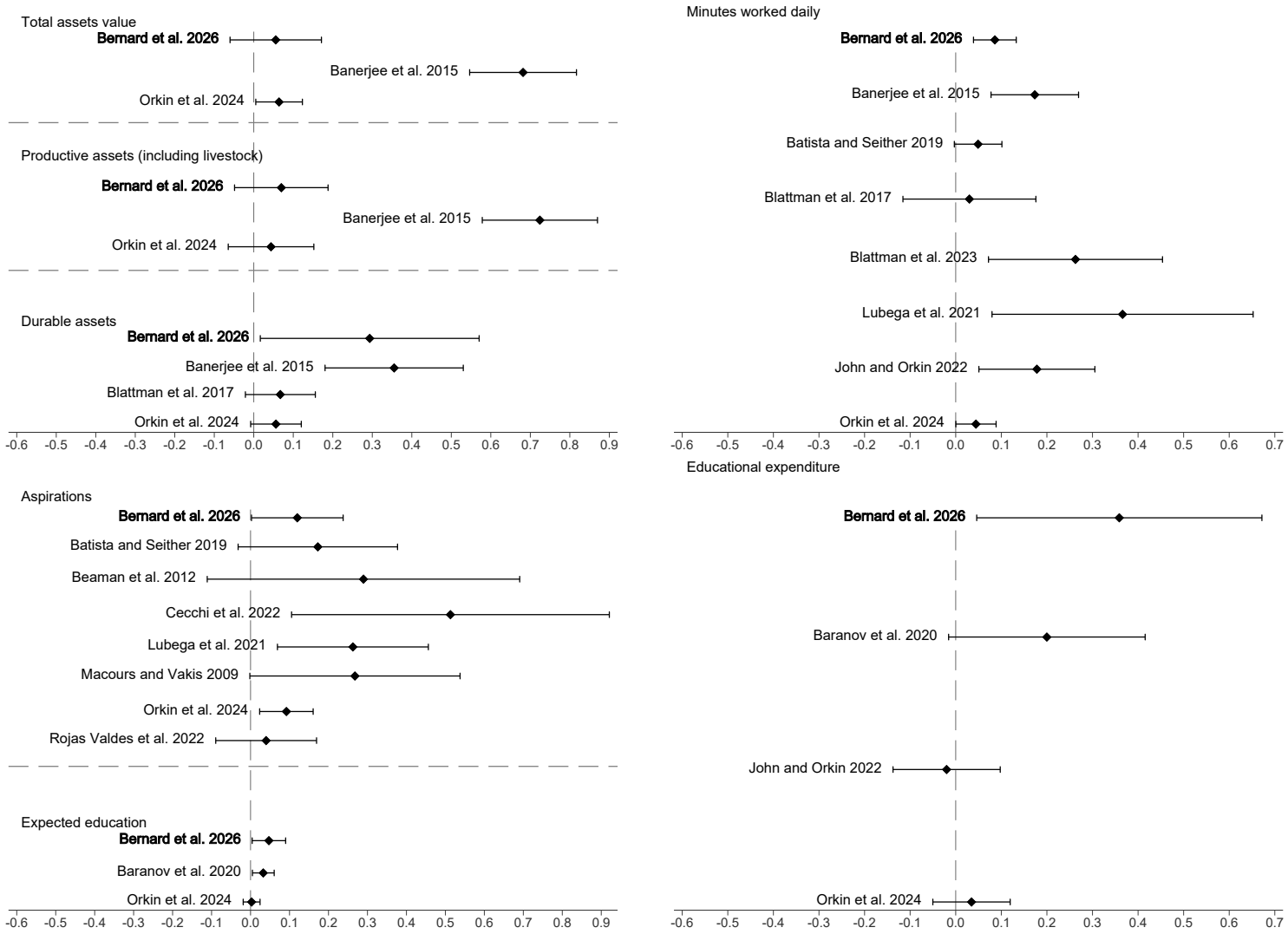


Figure A.5: Effect size expressed as a fraction of the control group mean across studies

Notes: Treatment effects expressed as a fraction of the control group mean across comparable outcomes in recent studies of psycho-social and anti-poverty interventions. Each line shows the OLS estimate (divided by the control mean) and their corresponding 95 per cent confidence interval for that outcome. Further details on the variables for each study are in Appendix Table A.27.

I Additional tables

Table A.1: Aspirations and expectations after the screening, after six months, and after five years

	After the screening				After six months				After five years			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.
Summary indices:												
Aspiration index	0.04	-0.04	0.09**	0.00	0.03	0.02	0.02	0.00	0.12**	-0.03	0.15**	0.02
	(0.05)	(0.05)	(0.04)	(1.00)	(0.05)	(0.05)	(0.05)	(1.01)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.38]	[0.33]	[0.10]	2003	[0.53]	[0.92]	[0.74]	2059	[0.04]**	[0.96]	[0.01]**	1904
Expectations index	0.23***	0.17***	0.06	0.00	0.07	0.01	0.06	-0.00	0.21***	-0.00	0.22***	0.01
	(0.05)	(0.05)	(0.05)	(1.00)	(0.05)	(0.05)	(0.05)	(1.00)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.00]***	[0.22]	2000	[0.51]	[0.92]	[0.63]	2055	[0.00]***	[0.96]	[0.00]***	1900
Aspirations and expectations aggregate index	0.13***	0.05	0.08*	0.00	0.05	0.01	0.04	0.00	0.19***	-0.03	0.22***	0.01
	(0.05)	(0.04)	(0.04)	(1.00)	(0.05)	(0.05)	(0.05)	(1.01)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.32]	[0.10]	2003	[0.51]	[0.92]	[0.67]	2059	[0.00]***	[0.96]	[0.00]***	1904
Aspirations: what would you like to achieve?												
Aspired income (USD) PPP	1769.95	-3091.51	4861.46*	23993.62	804.84	1973.81	-1168.97	21697.51	2505.32	-643.61	3148.93*	15446.71
	(2855.82)	(2432.07)	(2578.11)	(57202.10)	(2342.38)	(2335.89)	(2471.39)	(45062.36)	(1773.81)	(1593.19)	(1701.71)	(27746.43)
	[0.80]	[0.31]	[0.09]*	1969	[0.73]	[0.40]	[0.83]	2022	[0.22]	[0.69]	[0.10]*	1864
Aspired wealth (USD) PPP	-267.69	-3839.81**	3572.12**	13717.86	-1455.44	-1775.59	320.15	14542.17	1584.05	919.14	664.91	11978.72
	(1926.34)	(1784.27)	(1561.63)	(38805.20)	(1649.39)	(1558.37)	(1545.68)	(31209.03)	(1301.17)	(1338.82)	(1395.59)	(21518.47)
	[0.89]	[0.09]*	[0.07]*	1975	[0.57]	[0.40]	[0.83]	2034	[0.22]	[0.69]	[0.63]	1867
Aspired education (years)	0.16	0.07	0.08	14.12	0.27*	0.13	0.15	14.05	0.29*	-0.10	0.39**	14.26
	(0.12)	(0.12)	(0.12)	(2.39)	(0.14)	(0.14)	(0.14)	(2.61)	(0.16)	(0.16)	(0.16)	(2.60)
	[0.59]	[0.54]	[0.48]	1943	[0.18]	[0.40]	[0.83]	1936	[0.19]	[0.69]	[0.05]**	1769
Expectations: what do you expect in ten years?												
Aspired income (USD) PPP	1769.95	-3091.51	4861.46*	23993.62	804.84	1973.81	-1168.97	21697.51	2505.32	-643.61	3148.93*	15446.71
	(2855.82)	(2432.07)	(2578.11)	(57202.10)	(2342.38)	(2335.89)	(2471.39)	(45062.36)	(1773.81)	(1593.19)	(1701.71)	(27746.43)
	[0.80]	[0.31]	[0.09]*	1969	[0.73]	[0.40]	[0.83]	2022	[0.22]	[0.69]	[0.10]*	1864
Aspired wealth (USD) PPP	-267.69	-3839.81**	3572.12**	13717.86	-1455.44	-1775.59	320.15	14542.17	1584.05	919.14	664.91	11978.72
	(1926.34)	(1784.27)	(1561.63)	(38805.20)	(1649.39)	(1558.37)	(1545.68)	(31209.03)	(1301.17)	(1338.82)	(1395.59)	(21518.47)
	[0.89]	[0.09]*	[0.07]*	1975	[0.57]	[0.40]	[0.83]	2034	[0.22]	[0.69]	[0.63]	1867
Aspired education (years)	0.16	0.07	0.08	14.12	0.27*	0.13	0.15	14.05	0.29*	-0.10	0.39**	14.26
	(0.12)	(0.12)	(0.12)	(2.39)	(0.14)	(0.14)	(0.14)	(2.61)	(0.16)	(0.16)	(0.16)	(2.60)
	[0.59]	[0.54]	[0.48]	1943	[0.18]	[0.40]	[0.83]	1936	[0.19]	[0.69]	[0.05]**	1769

Notes: OLS estimates of within-village treatment and placebo effects right after the video screenings (columns 1-2), after six months (columns 5-6), and after five years (columns 9-10). All columns control for village fixed effects and characteristics of the respondent: age, years of education, an indicator for being single, and an indicator for being male. Regressions control for baseline outcomes. Columns 3, 7, and 11 test for differences in parameters obtained in previous two columns. Column 4, 8, and 12 displays the control mean, standard deviation, and number of observations across rounds. The comparison group is households from the 64 treated villages that were not invited to any screening. After six months we have more observations than after the screening because we could not complete the post-screening surveys with 22 individuals that missed the screening and 81 individuals that attended them but left before the end of the videos. Heteroskedasticity-robust standard errors are clustered at the household-level in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Variables are defined in Appendix C. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1. The unit of observation is the individual respondent (household head or their spouse). The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous outcomes above the 99^{th} percentile, though the indices aggregate all non-missing outcomes. Aspirations and expectations are defined in the note to Figure II. The aspirations and expectations indices are indices (inverse-covariance-weighted averages) of these three dimensions, constructed following Anderson (2008), standardised relative to the within-village control group. The aspirations and expectations aggregate index is made up of the reported income, wealth and years of education for children, for both aspirations and expectations.

Table A.2: Aspirations and expectations gaps after the screening, after six months, and after five years

	After the screening				After six months				After five years			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.
Summary indices of gaps:												
Aspirations (minus current at baseline) gap index	0.04	-0.05	0.09**	0.00	0.02	0.01	0.01	-0.01	0.09	-0.03	0.11**	0.01
	(0.05)	(0.04)	(0.04)	(1.00)	(0.05)	(0.05)	(0.05)	(0.99)	(0.06)	(0.06)	(0.06)	(0.98)
	[0.35]	[0.28]	[0.05]**	2003	[0.69]	[0.90]	[0.81]	2059	[0.12]	[0.92]	[0.04]**	1903
Expectations (minus current at baseline) gap index	0.27***	0.19***	0.08	0.00	0.06	-0.01	0.07	-0.01	0.18***	-0.01	0.19***	-0.00
	(0.05)	(0.05)	(0.05)	(1.00)	(0.05)	(0.05)	(0.05)	(1.00)	(0.06)	(0.06)	(0.06)	(0.99)
	[0.00]***	[0.00]***	[0.14]	2000	[0.69]	[0.90]	[0.56]	2054	[0.01]**	[0.92]	[0.01]***	1899
Aspirations and expectations gap aggregate index	0.16***	0.06	0.10**	0.00	0.03	-0.01	0.04	-0.02	0.15**	-0.03	0.17***	0.01
	(0.05)	(0.05)	(0.05)	(1.00)	(0.05)	(0.05)	(0.05)	(0.99)	(0.06)	(0.06)	(0.06)	(0.98)
	[0.00]***	[0.23]	[0.05]**	2003	[0.69]	[0.90]	[0.68]	2059	[0.02]**	[0.92]	[0.01]***	1903
Aspirations minus current level (at baseline)												
Aspired income gap (USD) PPP	1633.47	-2947.78	4581.25*	22564.40	536.30	2086.97	-1550.67	20395.07	2285.27	-636.50	2921.77*	14399.79
	(2869.83)	(2443.90)	(2592.40)	(56840.75)	(2354.50)	(2352.40)	(2489.08)	(44754.25)	(1765.05)	(1600.78)	(1697.30)	(27868.79)
	[0.85]	[0.34]	[0.12]	1951	[0.82]	[0.38]	[0.80]	2003	[0.29]	[0.69]	[0.13]	1848
Aspired wealth gap (USD) PPP	113.91	-3828.69**	3942.60***	11728.91	-1272.92	-1502.58	229.65	12607.96	1093.34	781.82	311.53	10627.57
	(1824.53)	(1666.66)	(1476.21)	(37173.77)	(1599.14)	(1511.07)	(1511.44)	(30017.37)	(1284.52)	(1331.10)	(1377.82)	(21451.27)
	[0.95]	[0.06]*	[0.02]**	1957	[0.64]	[0.38]	[0.88]	2011	[0.39]	[0.69]	[0.82]	1850
Aspired education gap (years)	0.16	0.07	0.08	12.85	0.27*	0.13	0.15	12.84	0.29*	-0.10	0.39**	13.00
	(0.12)	(0.12)	(0.12)	(2.90)	(0.14)	(0.14)	(0.14)	(3.06)	(0.16)	(0.16)	(0.16)	(3.04)
	[0.59]	[0.54]	[0.48]	1943	[0.18]	[0.38]	[0.80]	1936	[0.19]	[0.69]	[0.05]**	1769
Expectations minus current level (at baseline)												
Expected income gap (USD) PPP	1215.91***	611.15**	604.76*	3912.53	239.97	129.13	110.84	4046.39	203.74	49.29	154.45	2337.89
	(333.87)	(304.66)	(336.68)	(5598.73)	(440.52)	(417.23)	(429.14)	(8651.84)	(187.27)	(168.90)	(185.86)	(2765.03)
	[0.00]***	[0.07]*	[0.11]	1931	[0.75]	[0.76]	[0.80]	1986	[0.28]	[0.77]	[0.41]	1843
Expected wealth gap (USD) PPP	949.06***	309.63	639.43*	3196.99	97.35	-330.60	427.95	3285.77	462.89*	220.36	242.53	2611.47
	(309.25)	(295.88)	(326.19)	(5065.44)	(308.26)	(279.20)	(297.98)	(5372.90)	(255.21)	(251.48)	(257.96)	(3947.68)
	[0.00]***	[0.29]	[0.11]	1950	[0.75]	[0.65]	[0.36]	1981	[0.10]	[0.57]	[0.41]	1840
Expected education gap (years)	0.58***	0.62***	-0.04	12.06	0.33**	0.14	0.20	12.29	0.58**	-0.32	0.90***	11.05
	(0.17)	(0.17)	(0.15)	(3.92)	(0.16)	(0.17)	(0.17)	(3.51)	(0.26)	(0.26)	(0.27)	(4.24)
	[0.00]***	[0.00]***	[0.78]	1814	[0.13]	[0.65]	[0.36]	1791	[0.08]*	[0.57]	[0.00]***	1706

Notes: OLS estimates of within-village treatment and placebo effects right after the video screenings (columns 1-2), after six months (columns 5-6), and after five years (columns 9-10). Columns 3, 7, and 11 test for differences in parameters obtained in previous two columns. Column 4, 8, and 12 displays the control mean, standard deviation, and number of observations across rounds. The comparison group comprises households from the 64 treated villages that were not invited to any screening. After six months we have more observations than after the screening because we could not complete the post-screening surveys with 22 individuals that missed the screening and 81 individuals that attended them but left before the end of the videos. Heteroskedasticity-robust standard errors are clustered at the household-level in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Variables are defined in Appendix C. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1. The unit of observation is the individual respondent (household head or their spouse). The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous outcomes above the 99^{th} percentile, though the indices aggregate all non-missing outcomes. Aspirations and expectations are defined in the note to Figure II. The aspirations (or expectations) gaps take the measure of aspirations (or expectations) and subtract the current level at baseline elicited for that same dimension. For the current level of education, we use the respondents' own education level. The aspirations and expectations gap indices are indices (inverse-covariance-weighted averages) of the gaps on these three dimensions, constructed following Anderson (2008), standardised relative to the within-village control group. The aspirations and expectations gap aggregate index is made up of six outcomes: the reported gaps in income, wealth and years of education for the respondents' oldest child, for both aspirations and expectations.

Table A.3: Savings and credit

	After six months				After five years			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.
Has any savings	0.03	0.01	0.02	0.39	-0.02	-0.01	-0.01	0.21
	(0.02)	(0.02)	(0.02)	(0.49)	(0.02)	(0.02)	(0.02)	(0.41)
	[0.31]	[0.82]	[0.58]	2064	[0.76]	[0.80]	[0.86]	1909
Has outside savings	0.03	0.03	0.00	0.26	-0.01	-0.01	-0.00	0.20
	(0.02)	(0.02)	(0.02)	(0.44)	(0.02)	(0.02)	(0.02)	(0.40)
	[0.31]	[0.68]	[0.95]	2064	[0.76]	[0.80]	[0.86]	1909
Has any credit	0.03	-0.02	0.05*	0.34	-0.03	0.02	-0.06**	0.33
	(0.03)	(0.03)	(0.03)	(0.47)	(0.03)	(0.03)	(0.03)	(0.47)
	[0.31]	[0.79]	[0.29]	2052	[0.76]	[0.80]	[0.42]	1897
Has any agricultural credit					0.02***	0.01*	0.01	0.00
					(0.01)	(0.01)	(0.01)	(0.07)
					[0.06]*	[0.34]	[0.73]	1908
Total savings (USD) PPP	19.82***	5.10	14.72*	24.37	-0.21	0.69	-0.90	17.87
	(6.98)	(5.37)	(7.68)	(80.68)	(3.82)	(3.67)	(3.72)	(64.07)
	[0.04]**	[0.77]	[0.29]	2002	[0.96]	[0.89]	[0.86]	1865
Total outside savings (USD) PPP	2.19	1.78	0.41	8.88	-1.05	2.19	-3.24	16.50
	(1.46)	(1.47)	(1.59)	(25.07)	(3.61)	(3.65)	(3.74)	(59.48)
	[0.31]	[0.68]	[0.90]	2002	[0.96]	[0.80]	[0.73]	1863
Total credit (USD) PPP	3.98*	0.56	3.42	19.65	-8.93*	-0.69	-8.24	39.65
	(2.29)	(2.27)	(2.45)	(40.09)	(5.03)	(5.15)	(5.04)	(87.98)
	[0.31]	[0.84]	[0.49]	2044	[0.38]	[0.89]	[0.47]	1897
Hypothetical loan (1 year USD) PPP	76.54	312.51	-235.97	2051.78	-91.33	-277.41**	186.07	1579.91
	(222.17)	(233.55)	(235.01)	(3135.83)	(138.69)	(138.08)	(133.83)	(1974.66)
	[0.82]	[0.68]	[0.58]	1101	[0.76]	[0.34]	[0.47]	1056
Hypothetical loan (5 years USD) PPP	282.76	-83.75	366.51	4406.63	-24.04	-329.73	305.69	3037.61
	(416.15)	(414.02)	(423.73)	(6112.10)	(250.30)	(239.31)	(232.71)	(3518.50)
	[0.64]	[0.84]	[0.58]	1086	[0.96]	[0.56]	[0.47]	1041
Hypothetical loan (10 years USD) PPP	93.08	655.33	-562.24	7552.52	-414.45	-644.33	229.88	5663.60
	(873.91)	(912.53)	(987.41)	(11830.81)	(633.47)	(597.53)	(613.18)	(7780.67)
	[0.92]	[0.79]	[0.73]	1104	[0.76]	[0.70]	[0.86]	1002

Notes: OLS estimates of within-village treatment and placebo effects after six months (columns 1-2) and after five years (columns 5-6) of the intervention. Columns 3 and 7 test for differences in parameters obtained in previous two columns. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 and 8 display the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and characteristics of the respondent (or household head for household-level outcomes): age, years of education, an indicator for being single, and an indicator for being male. Regressions control for the baseline value of the outcome. Heteroskedasticity-robust standard errors are clustered at the household-level in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Variables are defined in Appendix C. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1. Regressions control for baseline outcomes, except for agricultural credit that was not collected at baseline. The unit of observation is the individual respondent (household head or their spouse), except for hypothetical loans (which are at the household-level). The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous outcomes above the 99^{th} percentile. Has savings is an indicator set to one if the respondent has any savings, while Has outside savings is one if savings are held outside the home. Has credit indicates if the respondent borrowed from any formal or informal source in the past six months. Agricultural credit indicates if crop inputs or equipment were received on credit in 2015. Total savings is the total amount saved both in and outside the home. Total credit records the total amount of loans taken. Hypothetical loan repayable in 1, 5, or 10 years measures the amount the respondent would borrow, interest-free, repayable in those time-frames.

Table A.4: Information index components

	After five years (1) Treatment	(2) Placebo	(3) Treat. vs. placebo	(4) Control mean (SD) Total obs.
Information index	-0.02	-0.07	0.04	0.00
	(0.07)	(0.07)	(0.07)	(1.00)
	[0.97]	[0.60]	[0.75]	1104
Income from trading	0.01	-0.03	0.04	0.23
	(0.03)	(0.03)	(0.03)	(0.42)
	[0.97]	[0.60]	[0.48]	1104
Attended community meeting to discuss ag issues	0.02	-0.03	0.04	0.68
	(0.03)	(0.03)	(0.03)	(0.47)
	[0.97]	[0.60]	[0.48]	1089
Requested or visited by ag expert	0.05	0.01	0.04	0.43
	(0.03)	(0.03)	(0.04)	(0.50)
	[0.97]	[0.85]	[0.48]	1089
Uses irrigation	-0.02	-0.05*	0.03	0.24
	(0.03)	(0.03)	(0.03)	(0.43)
	[0.97]	[0.60]	[0.52]	1089
Sought advice on fertilisers	0.00	0.01	-0.01	0.42
	(0.03)	(0.03)	(0.03)	(0.49)
	[1.00]	[0.75]	[0.79]	1023
Sought advice on land preparation	0.02	0.03	-0.01	0.36
	(0.03)	(0.03)	(0.03)	(0.48)
	[0.97]	[0.60]	[0.80]	1079
Sought advice on seeding	0.01	0.02	-0.01	0.42
	(0.03)	(0.03)	(0.03)	(0.49)
	[0.97]	[0.75]	[0.86]	1078
Number of visits by ag extension agent	-0.03	0.11	-0.14*	0.79
	(0.08)	(0.08)	(0.08)	(1.26)
	[0.97]	[0.60]	[0.48]	1089
Visited demonstration plot in last year	0.02	0.00	0.02	0.15
	(0.02)	(0.02)	(0.03)	(0.36)
	[0.97]	[0.85]	[0.75]	1089
Produces cash crops	-0.02	0.04	-0.06	0.61
	(0.03)	(0.03)	(0.04)	(0.49)
	[0.97]	[0.60]	[0.48]	1089
Uses pump	0.00	0.01	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.10)
	[1.00]	[0.60]	[0.48]	1088
Uses stone bands	-0.03	-0.02	-0.02	0.69
	(0.03)	(0.03)	(0.03)	(0.46)
	[0.97]	[0.75]	[0.76]	1104
Applies water harvesting	0.00	-0.02	0.02	0.07
	(0.02)	(0.02)	(0.02)	(0.26)
	[1.00]	[0.60]	[0.52]	1104
Uses crop rotation	-0.03	-0.05	0.02	0.53
	(0.03)	(0.03)	(0.03)	(0.50)
	[0.97]	[0.60]	[0.76]	1089
Uses cattle	0.01	-0.04	0.05	0.70
	(0.03)	(0.03)	(0.03)	(0.46)
	[0.97]	[0.60]	[0.48]	1088

Notes: OLS estimates of within-village treatment effects after five years (columns 1-2). Column 3 tests for differences in parameters obtained in first two columns. All columns control for village fixed effects and characteristics of the household head: age, years of education, an indicator for being single, and an indicator for being male. Regressions control for the baseline value of the outcome. Outcome variables are listed on the left, and described in detail in the Appendix C.4.4. The unit of observation is the household. Regressions do not control for baseline outcomes as these outcomes were only collected at the five-year follow-up. Heteroskedasticity-robust standard errors are in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Column 4 displays the control mean, standard deviation, and total number of observations. The number of observations varies slightly across rows because some respondents do not answer all questions, though the indices aggregate all non-missing outcomes. The information index is an inverse-covariance-weighted average of the components, constructed following Anderson (2008), standardised relative to the within-village control group and also reported in Table 6. The components of the index are indicators equal to 1 if the household does the activity or action mentioned, except the number of visits received by an agricultural extension worker (the only non-binary component of the index).

Table A.5: Revenue

After five years	(1) Treatment	(2) Placebo	(3) Treat. vs. placebo	(4) Control mean (SD) Total obs.
Gross revenue (USD PPP)	105.27	-54.60	159.87*	1468.82
	(95.89)	(94.66)	(94.89)	(1273.97)
	[0.65]	[0.62]	[0.55]	1061
Revenue from crop production (USD PPP)	22.06	21.39	0.67	383.70
	(22.45)	(22.54)	(23.97)	(300.60)
	[0.65]	[0.62]	[0.98]	1077
Revenue from livestock rearing and produce (USD PPP)	-3.25	-98.73	95.48	740.53
	(73.82)	(71.96)	(71.73)	(1002.83)
	[0.96]	[0.62]	[0.55]	1087
Revenue from wage labour (USD PPP)	-3.17	6.01	-9.18	25.86
	(8.04)	(9.16)	(9.07)	(111.32)
	[0.83]	[0.62]	[0.62]	1080
Revenue from non-farm enterprises (USD PPP)	21.24	14.99	6.25	159.94
	(28.81)	(29.84)	(31.79)	(353.37)
	[0.69]	[0.62]	[0.98]	1076
Transfers and remittances (USD PPP)	18.57	7.20	11.37	114.03
	(16.86)	(14.76)	(18.06)	(180.99)
	[0.65]	[0.62]	[0.79]	1079

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences between Columns 1 and 2. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and characteristics of the household head: age, years of education, an indicator for being single, and an indicator for being male. Heteroskedasticity-robust standard errors are in parentheses. The unit of observation is the household. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1. Gross revenue includes revenues received from wage labour, livestock and poultry, crops, non-farm activities and transfers and remittances. Revenue from crops is computed as price times quantity sold in the last agricultural season. We capture production in both the short and long rains seasons. We use Central Statistical Agency data from the Oromiya region for December 2015 and January 2016 to compute the price of crops. Revenue from livestock rearing and produce includes all revenues from sale of livestock and poultry and the production of livestock goods in the last 12 months. We use the self-reported sale prices to construct the livestock revenue variable. Livestock revenue includes also own-consumption of animals, valued at sales prices. Revenue from wage labour is the sum of agricultural and non-agricultural wages received by the household over the past 12 months. Non-farm revenue is the number of months an activity was operated for times the monthly revenue from this activity, for a list of common activities. Transfer revenues are revenues from all sources of transfers: pension, remittances, public works and other assistance programs, and other transfers e.g. burial associations, in the last 12 months.

Table A.6: Economic changes after six months

After six months	(1) Treatment	(2) Placebo	(3) Treat. vs. placebo	(4) Control mean (SD) Total obs.
Household head and spouse's labour effort:				
Daily minutes on family farm	7.16	-6.04	13.20*	299.07
	(7.14)	(7.40)	(7.43)	(216.87)
	[0.56]	[0.83]	[0.15]	1880
Daily minutes in leisure	6.27	0.45	5.83	796.25
	(10.90)	(11.14)	(10.92)	(185.84)
	[0.56]	[0.97]	[0.59]	1861
Savings and credit:				
Total savings (USD) PPP	19.82***	5.10	14.72*	24.37
	(6.98)	(5.37)	(7.68)	(80.68)
	[0.02]**	[0.63]	[0.22]	2002
Total credit (USD) PPP	3.98*	0.56	3.42	19.65
	(2.29)	(2.27)	(2.45)	(40.09)
	[0.16]	[0.80]	[0.32]	2044
Hypothetical loan (1 year USD) PPP	76.54	312.51	-235.97	2051.78
	(222.17)	(233.55)	(235.01)	(3135.83)
	[0.92]	[0.63]	[0.42]	1101
Hypothetical loan (10 years USD) PPP	93.08	655.33	-562.24	7552.52
	(873.91)	(912.53)	(987.41)	(11830.81)
	[0.92]	[0.63]	[0.57]	1104
Cohort 1: Children of post-primary school-going age at the five-year follow-up (aged 11–15 at the time of the intervention)				
Children aged 11-15 in school	0.07	0.03	0.04	0.56
	(0.05)	(0.04)	(0.05)	(0.73)
	[0.20]	[0.75]	[0.56]	1126
Daily minutes in school for children aged 11-15	15.66	6.33	9.33	188.71
	(15.85)	(15.29)	(15.94)	(248.36)
	[0.32]	[0.75]	[0.56]	1109
Daily minutes studying for children aged 11-15	9.88*	1.83	8.05	58.11
	(5.71)	(5.76)	(6.14)	(86.58)
	[0.20]	[0.75]	[0.56]	1109
Cohort $2^{(a)}$: Children of primary school-going age at the five-year follow-up (aged 7–10 at the time of the intervention)				
Children aged 7-10 in school	0.11**	0.05	0.06	0.60
	(0.05)	(0.05)	(0.05)	(0.73)
	[0.07]*	[0.53]	[0.48]	1126
Daily minutes in school for children aged 7-10	23.53	11.57	11.96	198.10
	(16.61)	(16.63)	(16.79)	(250.25)
	[0.23]	[0.53]	[0.48]	1112
Daily minutes studying for children aged 7-10	1.51	-2.93	4.44	45.08
	(4.83)	(4.69)	(4.72)	(70.78)
	[0.75]	[0.53]	[0.48]	1108
For all children				
Schooling expenditure (USD) PPP	6.47*	4.37	2.09	37.75
	(3.52)	(3.62)	(3.96)	(51.39)
	[0.07]*	[0.23]	[0.60]	1107

Notes: OLS estimates of within-village treatment and placebo effects six months after the intervention (columns 1-2). Column 3 tests for differences in parameters obtained in first two columns. The comparison group is households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and characteristics of the household head (or of the individual-respondent for the *effort* outcomes): age, years of education, an indicator for being single, and an indicator for being male. All regressions control for the baseline value of the outcome. Heteroskedasticity-robust standard errors are in parentheses (or clustered at the household-level for the *effort outcomes*). Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Variables are defined in the note to Figure I and Appendix C. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1. The unit of observation is the individual household head or spouse for effort, total savings and total credit and the household for other variables. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous outcomes above the 99^{th} percentile. We examine all households in the sample, including 71 households who have no children in this age group in any of the rounds, to ensure the sample is comparable with other results. Cohort $2^{(a)}$ is not directly comparable to cohort 2 in Table 4, because some of the children in cohort 2 were not of primary school-going age at the time of six-months follow-up and as they would have been between 2 and 6 years old; we did not collect data for children in this age range.

Table A.7: Risk aversion for each payout

	(1)	(2)	(3)	(4)	(5)	(6)
	Payouts					
Choice	Heads	Tails	Exp. value	Std. dev.	ΔE/ ΔSD	Risk aversion
1	2.5	2.5	2.5	0		Severe
2	2	4	3.0	1	0.5	Intermediate
3	1.5	5.5	3.5	2	0.5	Moderate
4	1	7	4.0	3	0.5	Slight-to-neutral
5	0	10	5.0	5	0.5	Neutral-to-preferred

Notes: Column 1 gives the choice number. Columns 2 and 3 give the payout options of the hypothetical lotteries. Columns 4 and 5 give the mean and variance of each lottery. The successive lotteries offered increase in both mean and variance, with payouts ordered from most to least risk averse.

Table A.8: Sample and compliance

	All groups	Treatment	Placebo	Within-village control	Pure control
Number of villages	74		64		10
Individuals:					
Observations					
In sample	2434	690	717	705	322
Given tickets	2112	690	717	705	0
Compliers	2070	673	698	699	0
Non-compliers	42	17	19	6	0
of which					
At wrong screening	20	3	11	6	0
Missed screening	22	14	8	0	0
Proportion of non-compliers	.017	.025	.026	.009	0
Households:					
Observations					
In sample	1331	384	383	384	180
Given tickets	1151	384	383	384	0
Compliers	1124	372	372	380	0
Non-compliers	27	12	11	4	0
of which					
At wrong screening	12	2	6	4	0
Missed screening	15	10	5	0	0
Proportion of non-compliers	.02	.031	.029	.01	0

Notes: Observations for individuals and households by treatment and compliance to treatment.

Balance

Table A.9: Balance tests – individual-level variables

	Baseline Sample Means				Endline Sample Means			
Variable	Control	Placebo	Treatment	p-value	Control	Placebo	Treatment	p-value
Male	0.491	0.484	0.493	0.49	0.495	0.477	0.490	0.14
Age	36.6	37.1	37.0	0.86	36.7	37.0	37.0	0.89
Years of education	1.23	1.28	1.49	0.13	1.25	1.28	1.50	0.15
Single	0.074	0.068	0.093	0.24	0.070	0.065	0.095	0.13
Watches television at least once a week	0.110	0.094	0.104	0.63	0.108	0.093	0.113	0.47
Listens to radio at least once a week	0.627	0.588	0.620	0.37	0.623	0.581	0.627	0.25
Travels outside the district at least once a week	0.142	0.121	0.148	0.38	0.144	0.127	0.159	0.34
Ever lived outside of current district six months	0.104	0.078	0.110	0.12	0.106	0.080	0.116	0.11
Aspiration index	-0.000	-0.018	0.044	0.55	-0.001	-0.018	0.070	0.31
Expectations index	0.000	-0.072	0.054	0.07	-0.006	-0.057	0.065	0.09
Daily minutes working	350	351	355	0.83	354	351	356	0.88
Daily minutes in leisure	739	729	735	0.63	739	727	734	0.53
Internal locus of control	12.7	12.5	12.6	0.61	12.7	12.6	12.6	0.75
Individual causes of poverty	8.91	8.83	8.93	0.76	8.88	8.86	8.96	0.74
Chance locus of control	12.3	12.4	12.5	0.61	12.3	12.4	12.5	0.63
Fate causes of poverty	6.71	6.81	6.84	0.60	6.69	6.79	6.83	0.56
Risk aversion: most to least risk averse (1 to 5)	3.23	3.15	3.24	0.56	3.22	3.16	3.25	0.60
% that is patient	0.333	0.308	0.341	0.45	0.336	0.309	0.341	0.46
% that is somewhat impatient	0.119	0.146	0.120	0.31	0.114	0.146	0.120	0.23
% that is most impatient	0.548	0.546	0.539	0.96	0.550	0.545	0.538	0.93
% that is present biased	0.323	0.362	0.358	0.32	0.324	0.355	0.360	0.41
% that is patient now and impatient later	0.229	0.213	0.230	0.73	0.231	0.220	0.235	0.83
Best life	4.27	4.10	4.31	0.16	4.27	4.10	4.34	0.13
Happiest life	6.64	6.51	6.61	0.68	6.68	6.50	6.65	0.48
Omnibus F-test	0.61	0.22	0.72	0.50	0.69	0.21	0.58	0.43

Table shows means of selected baseline variables for each treatment group in the baselined sample (columns 1-3) and endlined sample (columns 5-7). Column 4 shows p-values for equal means in the baselined sample, evaluating balanced treatment assignments. Column 8 shows p-values for equal means in the endlined sample, evaluating balanced attrition. Hypothesis tests are based on heteroskedasticity-robust standard errors clustered at the household-level. The bottom row reports p-values from an omnibus F-test regressing all covariates on a treatment group indicator. In the fourth and eight column, we report randomization-inference p-values from a chi-squared statistics test from a multinomial logit regression using treatment categories as the dependent variable, following Kerwin, Rostom, and Sterck (2024).

Table A.10: Balance tests – household-level variables

	Baseline Sample Means				Endline Sample Means			
Variable	Control	Placebo	Treatment	p-value	Control	Placebo	Treatment	p-value
Number of individuals aged 0-6	1.42	1.44	1.33	0.32	1.42	1.45	1.32	0.28
Number of individuals aged 11-15	0.610	0.692	0.774	0.03	0.618	0.710	0.782	0.03
Number of individuals aged 7-10	0.908	0.921	1.071	0.02	0.901	0.948	1.075	0.03
Household size	5.53	5.58	5.73	0.43	5.52	5.66	5.73	0.43
Children aged 11-15 in school	0.455	0.526	0.592	0.04	0.460	0.539	0.599	0.04
Daily minutes in school for children aged 11-15	148	180	193	0.03	150	184	196	0.03
Daily minutes studying for children aged 11-15	53.0	64.6	69.0	0.05	53.3	66.5	69.4	0.04
Children aged 7-10 in school	0.471	0.397	0.516	0.05	0.465	0.403	0.514	0.08
Daily minutes in school for children aged 7-10	156	131	167	0.06	154	132	167	0.11
Daily minutes studying for children aged 7-10	38.3	29.7	41.6	0.02	37.3	29.5	41.5	0.03
Schooling expenditure (USD) PPP	38.7	44.2	50.8	0.03	38.3	44.6	51.2	0.02
Value of livestock (USD) PPP	1341	1220	1402	0.09	1346	1226	1432	0.06
Value of tools (USD) PPP	70.8	72.7	76.2	0.65	71.3	71.5	77.3	0.55
Food security index: z-score	0.044	-0.061	0.017	0.31	0.051	-0.051	0.036	0.32
Months of food insecurity	5.59	5.73	5.66	0.80	5.61	5.77	5.67	0.74
Value of durable assets excluding tools (USD) PPP	52.5	53.7	62.6	0.48	53.6	55.1	62.1	0.62
Value of house (USD) PPP	1186	1077	1215	0.21	1185	1093	1218	0.31
Non-organic roof	0.485	0.486	0.570	0.03	0.487	0.503	0.566	0.09
Own toilet facility	0.760	0.765	0.741	0.73	0.765	0.764	0.740	0.69
Omnibus F-test	0.274	0.450	0.290	0.220	0.266	0.384	0.204	0.168
Omnibus F-test (across both Table A.9 and A.10)	0.442	0.314	0.560	0.356	0.388	0.094	0.450	0.200

Table shows means of selected baseline variables for each treatment group in the baselined sample (columns 1-3) and endlined sample (columns 5-7). Column 4 shows p-values for equal means in the baselined sample, evaluating balanced treatment assignments. Column 8 shows p-values for equal means in the endlined sample, evaluating balanced attrition. Hypothesis tests are based on heteroskedasticity-robust standard errors clustered at the household-level. The second last row show p-values from an F-test of all covariates in this table regressed on the control, placebo, or treatment indicators. In the fourth and eight column, we report a nested test from a multinomial logit regression using treatment categories as the dependent variable. The bottom row of p-values comes from household-level regressions of all covariates on the respective indicators or multinomial logits, spanning all covariates in Table A.9 and A.10, following Kerwin, Rostom, and Sterck (2024).

Attrition

Table A.11: Determinants of household-level attrition

Attrited in...	(1)	(2)	(3)	(4)	(5)	(6)
	Any round	Any round	After six months	After six months	After five years	After five years
Treatment	0.021	0.037	-0.000	0.099	0.026*	0.038
	(0.015)	(0.104)	(0.006)	(0.081)	(0.014)	(0.099)
Placebo	0.018	0.073	-0.003	0.017	0.021	0.058
	(0.015)	(0.112)	(0.006)	(0.035)	(0.014)	(0.107)
% male		-0.017		0.018		-0.033
		(0.029)		(0.022)		(0.021)
Age		-0.000		-0.000		-0.000
		(0.001)		(0.000)		(0.001)
Years of education		0.002		-0.001		0.003
		(0.003)		(0.001)		(0.003)
Single		0.048		0.040		0.006
		(0.035)		(0.029)		(0.023)
% that watches television at least once a week		-0.008		-0.013		0.005
		(0.030)		(0.011)		(0.029)
% that listens to radio at least once a week		-0.025		-0.020		-0.006
		(0.024)		(0.013)		(0.020)
% that travels outside the district at least once a week		-0.003		0.020		-0.023
		(0.024)		(0.020)		(0.015)
% that ever lived outside of current district six months		0.029		0.040		-0.016
		(0.036)		(0.031)		(0.021)
Number of individuals aged 0-6		0.008		0.006		0.001
		(0.009)		(0.006)		(0.007)
Number of individuals aged 11-15		-0.008		0.010		-0.017
		(0.012)		(0.006)		(0.010)
Number of individuals aged 7-10		0.021*		0.004		0.018*
		(0.012)		(0.007)		(0.010)
Total assets (USD) PPP		-0.000		-0.000		-0.000
		(0.000)		(0.000)		(0.000)
% male*treatment		-0.001		-0.064		0.006
		(0.064)		(0.047)		(0.061)
% male*placebo		-0.053		-0.013		-0.042
		(0.080)		(0.026)		(0.076)
Age*treatment		0.000		-0.001		0.000
		(0.002)		(0.001)		(0.002)
Age*placebo		0.000		0.000		0.000
		(0.001)		(0.000)		(0.001)
Years of education*treatment		0.003		0.000		0.003
		(0.007)		(0.002)		(0.007)
Years of education*placebo		-0.001		0.000		-0.001
		(0.005)		(0.002)		(0.005)
Single*treatment		-0.050		-0.058		-0.011
		(0.063)		(0.038)		(0.057)
Single*placebo		0.014		-0.044		0.057
		(0.082)		(0.031)		(0.076)

Continues on next page.

Attrited in...	(1)	(2)	(3)	(4)	(5)	(6)
	Any round	Any round	After six months	After six months	After five years	After five years
Continued from previous page						
% that watches television at least once a week*treatment		-0.009		-0.000		-0.014
		(0.043)		(0.013)		(0.043)
% that watches television at least once a week*placebo		0.029		0.014		0.015
		(0.056)		(0.014)		(0.055)
% that listens to radio at least once a week*treatment		0.025		0.010		0.003
		(0.035)		(0.015)		(0.032)
% that listens to radio at least once a week*placebo		0.048		0.013		0.034
		(0.034)		(0.015)		(0.030)
% that travels outside the district at least once a week*treatment		-0.043		-0.024		-0.027
		(0.030)		(0.020)		(0.025)
% that travels outside the district at least once a week*placebo		-0.038		-0.025		-0.013
		(0.035)		(0.021)		(0.029)
% that ever lived outside of current district six months*treatment		-0.068		-0.031		-0.020
		(0.045)		(0.035)		(0.034)
% that ever lived outside of current district six months*placebo		-0.019		-0.045		0.028
		(0.055)		(0.030)		(0.047)
Number of individuals aged 0-6*treatment		0.000		-0.006		0.003
		(0.015)		(0.008)		(0.013)
Number of individuals aged 0-6*placebo		-0.007		-0.007		0.000
		(0.014)		(0.007)		(0.012)
Number of individuals aged 11-15*treatment		0.001		-0.007		0.014
		(0.022)		(0.009)		(0.020)
Number of individuals aged 11-15*placebo		-0.009		-0.010		-0.000
		(0.020)		(0.007)		(0.019)
Number of individuals aged 7-10*treatment		-0.020		-0.003		-0.017
		(0.017)		(0.008)		(0.015)
Number of individuals aged 7-10*placebo		-0.043**		-0.004		-0.039**
		(0.018)		(0.008)		(0.016)
Total assets (USD) PPP*treatment		-0.000		-0.000		-0.000
		(0.000)		(0.000)		(0.000)
Total assets (USD) PPP*placebo		0.000		-0.000		0.000
		(0.000)		(0.000)		(0.000)
Control mean	.03	.03	.01	.01	.03	.03
F-test p-value	.28	.32	.87	1	.11	.34

Notes: Each column reports coefficients from regressions where the outcome is an indicator for individual attrition across different survey rounds. Columns labeled "Any round" indicate attrition at any point during the study period; "After six months" and "After five years" indicate attrition by those specific time-points. If a baseline covariate is missing, we replace the missing values with the sample mean and include a missing data indicator. All regressions include village fixed effects. Standard errors are robust to heteroskedasticity and are reported in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. At the bottom we report the mean attrition rate in the control group and a p-value from an F-test testing that all coefficients on the covariates reported in the column are equal to zero. The number of observations is 1,152 households interviewed at baseline.

Table A.12: Pre-specified groups 1,2,3,4

	(1)	(2)	(3)	(4)
	After five years			
	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.
PAP group 1				
Aspirations index	0.07*	-0.01	0.08**	0.19
Focal outcome	(0.04)	(0.04)	(0.04)	(0.67)
Reported with different index construction	[0.18]	[0.85]	[0.11]	1909
Income (USD) PPP	2436.44	-689.07	3125.51*	15446.71
Reported in Table A.1	(1745.50)	(1604.00)	(1697.89)	(27746.43)
	[0.27]	[0.83]	[0.11]	1894
Wealth (USD) PPP	1550.68	836.95	713.73	11978.72
Reported in Table A.1	(1298.04)	(1338.75)	(1392.27)	(21518.47)
	[0.29]	[0.83]	[0.61]	1889
Social Status (% of individuals)	-1.21	0.62	-1.83	80.13
Not reported elsewhere	(1.41)	(1.34)	(1.38)	(22.92)
	[0.39]	[0.83]	[0.23]	1907
Education (years)	0.31*	-0.13	0.43***	14.26
Reported in Table A.1	(0.16)	(0.17)	(0.16)	(2.60)
	[0.18]	[0.83]	[0.04]**	1803
PAP group 2				
Expectations index	0.13***	0.01	0.11***	0.06
Focal outcome	(0.04)	(0.04)	(0.04)	(0.67)
Reported with different index construction	[0.01]***	[0.76]	[0.01]**	1909
Income (USD) PPP	306.52	107.23	199.29	3409.97
Reported in Table A.1	(188.01)	(177.52)	(192.24)	(2820.92)
	[0.13]	[0.68]	[0.30]	1894
Wealth (USD) PPP	612.21**	256.15	356.06	4009.69
Reported in Table A.1	(249.03)	(247.36)	(250.75)	(3997.53)
	[0.02]**	[0.50]	[0.19]	1888
Social Status (% of individuals)	-0.73	1.96	-2.69*	65.16
Not reported elsewhere	(1.62)	(1.62)	(1.61)	(25.83)
	[0.65]	[0.50]	[0.16]	1907
Education (years)	0.67***	-0.35	1.02***	12.31
Reported in Table A.1	(0.26)	(0.27)	(0.27)	(3.88)
	[0.02]**	[0.50]	[0.00]***	1803
PAP group 3				
Individual agency index	0.02	0.03	-0.01	0.00
Focal outcome	(0.06)	(0.06)	(0.06)	(1.00)
Not reported elsewhere	[0.84]	[0.89]	[0.90]	1909
Internal locus of control	0.02	0.08	-0.05	12.26
Reported in Table 6	(0.11)	(0.11)	(0.11)	(1.90)
	[0.84]	[0.89]	[0.90]	1909
Individual causes of poverty	0.04	0.02	0.02	9.14
Reported in Table 6	(0.13)	(0.13)	(0.13)	(2.03)
	[0.84]	[0.89]	[0.90]	1909
PAP group 4				
Belief in chance index	-0.00	0.01	-0.01	-0.01
Focal outcome	(0.06)	(0.06)	(0.06)	(1.00)
Not reported elsewhere	[1.00]	[0.98]	[0.92]	1909
Chance locus of control	-0.01	-0.07	0.06	12.67
Reported in Table 6	(0.15)	(0.15)	(0.15)	(2.35)
	[1.00]	[0.98]	[0.92]	1909
Fate causes of poverty	-0.03	-0.00	-0.03	6.70
Reported in Table 6	(0.11)	(0.10)	(0.10)	(1.79)
	[1.00]	[0.98]	[0.92]	1909

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences between parameters reported in the first two columns. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and individual: age, years of education, an indicator for being single, and an indicator for being male. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each group (panel) of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in Appendix D.1, or in the main tables where they are reported. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. Heteroskedasticity-robust standard errors are in parentheses, clustered at the household-level. The unit of observation is the individual respondent (household head or their spouse). The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile, though the indices aggregate all non-missing outcomes. Aspirations index and Expectations index calculated as a standardised weighted average, with each dimensions weighted according to respondents' subjective weight for that dimensions.

Table A.13: Pre-specified group 5,6

	(1)	(2)	(3)	(4)
	After five years			
	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.
PAP group 5 (household-level)				
Children aged 6-15 in school	0.11	-0.02	0.13	1.25
Focal outcome	(0.09)	(0.08)	(0.08)	(1.21)
	[0.21]	[0.83]	[0.13]	1086
Schooling expenditure (USD) PPP	9.35***	1.83	7.52**	19.17
Reported in Table 4	(2.94)	(2.54)	(3.11)	(32.73)
	[0.00]***	[0.83]	[0.03]**	1081
PAP group 5: secondary outcomes (household-level)				
Children aged 16-20 in school	0.07**	0.01	0.07*	0.17
Reported in Table 4	(0.03)	(0.03)	(0.04)	(0.41)
	[0.05]*	[0.87]	[0.10]*	1086
Daily minutes in school for children aged 6-15	57.38*	-15.52	72.90**	550.51
Reported in Table 4 for children 7-15	(32.86)	(32.25)	(32.65)	(452.69)
	[0.08]*	[0.87]	[0.06]*	1077
Daily minutes in school for children aged 16-20	36.62***	3.83	32.79**	58.64
Reported in Table 4	(13.44)	(11.45)	(13.72)	(149.88)
	[0.03]**	[0.87]	[0.06]*	1085
Daily minutes studying for children aged 6-15	22.13**	8.73	13.39	93.12
Reported in Table 4 for children 7-15	(9.09)	(8.95)	(9.21)	(117.61)
	[0.04]**	[0.87]	[0.15]	1076
Daily minutes studying for children aged 16-20	9.60**	1.63	7.96	17.82
Reported in Table 4	(4.67)	(4.24)	(4.99)	(52.12)
	[0.05]*	[0.87]	[0.14]	1078
PAP group 6 (household head or spouse)				
Daily minutes working	28.30***	8.35	19.95***	326.16
Focal outcome	(7.67)	(7.77)	(7.69)	(200.01)
Reported in Table 3	[0.00]***	[0.28]	[0.02]**	1887
Daily minutes in leisure	-51.94***	-31.72***	-20.22**	830.59
Focal outcome	(9.61)	(10.05)	(9.91)	(187.38)
Reported in Table 3	[0.00]***	[0.00]***	[0.04]**	1899

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences between Columns 1 and 2. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and characteristics of the household head: age, years of education, an indicator for being single, and an indicator for being male. The controls for family 2.2 correspond to the individual respondent (household head or spouse). Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each group (panel) of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in detail in Appendix D.1, or in the main tables where they are reported. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. Heteroskedasticity-robust standard errors are in parentheses. The unit of observation is the household for group 5 and the individual respondent (household head or their spouse) for group 6. The number of observations varies slightly across rows because some respondents do not answer all questions. Ages of children correspond to those recorded at endline. Children aged 6–15 would have been 1-10 at the time of the intervention. Children aged 16–20 would have been 11–15 at the time of the intervention.

Table A.14: Pre-specified groups 7,8

	(1)	(2)	(3)	(4)
	After five years			
	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.
PAP group 7				
Total savings (USD) PPP	0.20	1.70	-1.51	17.87
Focal outcome	(3.86)	(3.75)	(3.86)	(64.07)
Reported in Table A.3	[0.96]	[0.75]	[0.89]	1890
Has any savings	-0.01	-0.01	-0.00	0.21
Reported in Table A.3	(0.02)	(0.02)	(0.02)	(0.41)
	[0.93]	[0.75]	[0.95]	1909
Has outside savings	-0.01	-0.01	0.00	0.20
Reported in Table A.3	(0.02)	(0.02)	(0.02)	(0.40)
	[0.93]	[0.75]	[0.95]	1909
Has any credit	-0.03	0.03	-0.06**	0.33
Reported in Table A.3	(0.03)	(0.03)	(0.03)	(0.47)
	[0.90]	[0.71]	[0.33]	1909
Has any agricultural credit	0.02***	0.01*	0.01	0.00
Reported in Table A.3	(0.01)	(0.01)	(0.01)	(0.07)
	[0.05]*	[0.30]	[0.79]	1908
Credit amount (USD) PPP	-8.39*	-0.33	-8.06	39.65
Reported in Table A.3	(5.03)	(5.16)	(5.05)	(87.98)
	[0.43]	[0.95]	[0.33]	1897
Hypothetical loan (1 year USD) PPP	-82.78	-297.62**	214.84	1579.91
Reported in Table A.3	(137.97)	(135.57)	(134.58)	(1974.66)
	[0.93]	[0.25]	[0.33]	1082
Hypothetical loan (5 years USD) PPP	-20.54	-353.65	333.12	3037.61
Reported in Table A.3	(245.14)	(234.41)	(230.61)	(3518.50)
	[0.96]	[0.40]	[0.34]	1075
Hypothetical loan (10 years USD) PPP	-230.38	-495.37	264.99	5663.60
Reported in Table A.3	(632.33)	(583.88)	(608.57)	(7780.67)
	[0.93]	[0.71]	[0.89]	1022
PAP group 8				
Aggregate investment in livestock and agriculture (USD) PPP	15.72	-18.17	33.90	214.72
Focal outcome	(30.46)	(29.60)	(28.78)	(412.21)
Not reported elsewhere	[0.85]	[0.94]	[0.39]	1061
Total expenditure on inputs for livestock and poultry activities (USD) PPP	0.68	-27.18	27.86	189.80
Reported in Table 3	(30.10)	(29.04)	(28.00)	(411.72)
but without including animal purchases	[0.98]	[0.86]	[0.39]	1072
Spending on seed or fertiliser	7.33**	3.80	3.53	33.49
Reported in Table 3	(3.07)	(3.32)	(3.31)	(43.54)
	[0.12]	[0.86]	[0.39]	1078
Value of family crop labour (USD) PPP	33.33*	1.27	32.06	387.81
Reported in Table 3	(19.73)	(19.39)	(20.08)	(258.03)
	[0.32]	[0.95]	[0.39]	1079
Spending on hired crop labour (USD) PPP	-1.30	-4.97	3.67	54.16
Reported in Table 3	(5.45)	(5.51)	(5.42)	(93.01)
	[0.95]	[0.86]	[0.50]	1078
Area cultivated (hectares)	0.01	-0.01	0.02	0.55
Reported in Table 3	(0.02)	(0.02)	(0.02)	(0.30)
	[0.85]	[0.95]	[0.39]	1071
Area rented in or sharecropped (hectares)	0.01	-0.00	0.01	0.02
Not reported elsewhere	(0.01)	(0.01)	(0.01)	(0.07)
	[0.34]	[0.95]	[0.39]	1068

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences in Columns 1 and 2. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and characteristics of the household head: age, years of education, an indicator for being single, and an indicator for being male. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each group (panel) of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in detail in Appendix D.1, or in the main tables where they are reported. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. All columns control for village fixed effects and characteristics of the household head (or the spouse): age, years of education, an indicator for being single, and an indicator for being male. The unit of observation is the individual for PAP group 7, except for the hypothetical loans (that are for the household). The unit of observation is the household (and controls refer to the household head only) for group 8. The number of observations varies slightly across rows because some respondents do not answer all questions.

Table A.15: Pre-specified group 9

	(1)	(2)	(3)	(4)
	After five years			
	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.
PAP group 9 (risk preferences)				
1 if extremely risk averse (coin)	-0.04	-0.01	-0.03	0.38
Not reported elsewhere	(0.03)	(0.03)	(0.03)	(0.49)
	[0.44]	[0.86]	[0.44]	1909
1 if extremely risk averse (market)	-0.02	0.01	-0.02	0.37
Raw scores reported in Table 6	(0.03)	(0.03)	(0.03)	(0.48)
	[0.56]	[0.86]	[0.44]	1909
PAP group 9 (time preferences)				
Near term discount factor	-0.03	-0.02	-0.01	0.42
Reported with different definition in Table 6	(0.02)	(0.02)	(0.02)	(0.29)
	[0.11]	[0.21]	[0.70]	1909
PAP group 9 (information)				
Income from trading	0.01	-0.03	0.04	0.23
Reported in Table A.4	(0.03)	(0.03)	(0.03)	(0.42)
	[0.88]	[0.55]	[0.33]	1104
Income from grain milling	-0.00	0.00	-0.01	0.00
Dropped because of low variation	(0.00)	(0.00)	(0.00)	(0.05)
	[0.77]	[0.74]	[0.29]	1088
Information sub-index pre-specified	0.13*	0.02	0.11	0.00
Components reported separately in Table A.4	(0.07)	(0.06)	(0.07)	(1.00)
	[0.52]	[0.80]	[0.29]	1089
Produces cash crops	-0.02	0.04	-0.06	0.61
Reported in Table A.4	(0.03)	(0.03)	(0.04)	(0.49)
	[0.88]	[0.55]	[0.29]	1089
Uses irrigation	-0.02	-0.05*	0.03	0.24
Reported in Table A.4	(0.03)	(0.03)	(0.03)	(0.43)
	[0.79]	[0.55]	[0.36]	1089
Uses stone bands	-0.03	-0.02	-0.02	0.69
Reported in Table A.4	(0.03)	(0.03)	(0.03)	(0.46)
	[0.77]	[0.78]	[0.62]	1104
Applies water harvesting	0.00	-0.02	0.02	0.07
Reported in Table A.4	(0.02)	(0.02)	(0.02)	(0.26)
	[0.99]	[0.55]	[0.36]	1104
Uses water storage	0.01*	0.00	0.01*	0.00
Dropped because of low variation	(0.01)	(0.00)	(0.01)	(0.05)
	[0.52]	[0.98]	[0.29]	1089
Uses crop rotation	-0.03	-0.05	0.02	0.53
Reported in Table A.4	(0.03)	(0.03)	(0.03)	(0.50)
	[0.77]	[0.55]	[0.62]	1089
Uses pump	0.00	0.01	-0.01	0.01
Reported in Table A.4	(0.01)	(0.01)	(0.01)	(0.10)
	[1.00]	[0.55]	[0.29]	1088
Uses cattle	0.01	-0.04	0.05	0.70
Reported in Table A.4	(0.03)	(0.03)	(0.03)	(0.46)
	[0.88]	[0.55]	[0.29]	1088
Information index pre-specified	-0.08	-0.12	0.04	0.00
	(0.07)	(0.08)	(0.08)	(1.00)
	[0.26]	[0.12]	[0.63]	1152

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences between parameters reported in the first two columns. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and individual: age, years of education, an indicator for being single, and an indicator for being male. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each group (panel) of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in detail in Appendix D.1, or in the main tables where they are reported. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. Heteroskedasticity-robust standard errors are in parentheses. The unit of observation is the household. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile. Table 6 reports the raw answers (coded from 1-5, where 5 is extremely risk averse) for the hypothetical maize sale (market), whereas the pre-specified outcome was an indicator equal to one if the respondent was coded to have answered 5 (in either coin-flip scenario or the hypothetical maize sale). The variables in the information panel are measured at the household-level and use the household head characteristics as controls. Two variables that we pre-specified to be part of the index did not have sufficient variation. Only 3 households reported earning income from grain milling and 7 reported using water storage (level basin irrigation).

Table A.16: Pre-specified groups 10,11,12,13,14

	(1)	(2)	(3)	(4)
	After five years			
	Treatment	Placebo	Treat. vs. placebo	Control mean (SD) Total obs.
PAP group 10				
Consumption aggregate (12m recall USD) PPP	97.87	53.33	44.54	2764.68
Focal outcome	(111.93)	(107.64)	(119.16)	(1530.00)
Not reported elsewhere	[0.38]	[0.94]	[0.85]	1088
Food consumption (USD) per ad. equiv. monthly PPP	-1.98	-2.29	0.32	53.91
Reported in Table 5	(2.05)	(1.92)	(2.07)	(29.98)
	[0.38]	[0.70]	[0.88]	1076
Frequent non-food (1m recall USD) per ad. equiv. PPP	0.44	0.04	0.40	4.08
Reported in Table 5	(0.28)	(0.28)	(0.30)	(3.69)
	[0.23]	[0.94]	[0.30]	1076
Nonfood consumption (12m recall USD) per ad. equiv. monthly PPP	0.70	-0.54	1.24**	7.47
Reported in Table 5	(0.51)	(0.43)	(0.48)	(6.35)
	[0.26]	[0.70]	[0.06]*	1079
Consumption of cigarettes and tobacco (USD) PPP	0.20	0.04	0.17	0.80
Reported in Table 5	(0.13)	(0.12)	(0.13)	(1.66)
	[0.23]	[0.94]	[0.30]	1078
General economic position (scale 1 to 4)	0.09*	0.00	0.09*	2.10
Reported in Table 5	(0.05)	(0.05)	(0.05)	(0.73)
	[0.23]	[0.94]	[0.25]	1088
PAP group 11				
Food security index: z-score	-0.06	-0.10	0.04	0.48
Focal outcome	(0.06)	(0.06)	(0.06)	(0.92)
Reported in Table 5	[0.31]	[0.20]	[0.54]	1084
Months of food insecurity	-0.32**	0.03	-0.35**	2.71
Reported in Table 5	(0.14)	(0.15)	(0.14)	(2.13)
	[0.05]*	[0.85]	[0.03]**	1088
PAP group 12				
Asset aggregate (USD) PPP	272.83*	-88.00	360.82**	2190.42
Focal outcome	(144.82)	(138.95)	(144.27)	(1974.69)
Not reported elsewhere	[0.08]*	[0.70]	[0.04]**	1078
Value of livestock (USD) PPP	187.38	-122.78	310.16**	2018.22
Reported in Table 3	(135.89)	(130.84)	(130.45)	(1921.09)
	[0.17]	[0.70]	[0.04]**	1080
Value of tools (USD) PPP	27.55**	12.17	15.37	106.02
Reported in Table 3	(11.61)	(12.35)	(13.66)	(126.90)
	[0.07]*	[0.70]	[0.26]	1077
Value of durable assets excluding tools (USD) PPP	21.95**	-2.93	24.88**	70.55
Reported in Table 5	(10.75)	(9.22)	(11.18)	(127.39)
	[0.08]*	[0.75]	[0.04]**	1077
PAP group 13				
Subjective well-being index (Anderson, 2008)	0.10	0.05	0.05	0.01
Focal outcome	(0.06)	(0.06)	(0.06)	(0.99)
Not reported elsewhere	[0.16]	[0.61]	[0.62]	1909
Best life	0.23**	0.06	0.17	4.83
Reported in Table 3	(0.11)	(0.11)	(0.12)	(1.80)
	[0.13]	[0.61]	[0.43]	1909
Happiest life	0.11	0.12	-0.01	6.05
Reported in Table 3	(0.14)	(0.14)	(0.14)	(2.19)
	[0.42]	[0.61]	[0.95]	1909
PAP group 14				
Total revenue (USD) PPP	66.25	-43.94	110.19	1345.87
Focal outcome	(92.41)	(92.30)	(93.35)	(1236.93)
Reported in Table A.5 with minor change	[0.79]	[0.63]	[0.52]	1076
Crop revenue (USD) PPP	22.06	21.39	0.67	383.70
Reported in Table A.5	(22.45)	(22.54)	(23.97)	(300.60)
	[0.79]	[0.63]	[0.98]	1077
Livestock revenue (USD) (using self-reported prices) PPP	4.45	-98.49	102.94	726.36
Reported in Table A.5 including own-consumption	(73.05)	(71.25)	(70.72)	(994.60)
	[0.95]	[0.63]	[0.52]	1078
Wage labour revenue (USD) PPP	-3.17	6.01	-9.18	25.86
Reported in Table A.5	(8.04)	(9.16)	(9.07)	(111.32)
	[0.87]	[0.63]	[0.52]	1080
Non-farm revenue (USD) PPP	21.24	14.99	6.25	159.94
Reported in Table A.5	(28.81)	(29.84)	(31.79)	(353.37)
	[0.79]	[0.63]	[0.98]	1076

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences between Columns 1 and 2. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and characteristics of the household head: age, years of education, an indicator for being single, and an indicator for being male. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each group (panel) of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in detail in Appendix D.1, or in the main tables where they are reported. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP.Heteroskedasticity-robust standard errors are in parentheses. The unit of observation is the household. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile. The variables in the group 13 are measured at the individual respondent-level (household head or spouse) and use the respondents' characteristics as controls.

Table A.17: Summary indices following PAP variable definitions and groupings

	After five years (1) Treatment	(2) Placebo	(3) Treat. vs. placebo	(4) Control mean (SD) Total obs.
Agricultural investment (PAP) index	0.12*	-0.03	0.16**	0.00
	(0.07)	(0.07)	(0.07)	(1.00)
	[0.08]*	[0.93]	[0.02]**	1088
Educational investment (PAP) index	0.18**	0.01	0.17**	0.00
	(0.07)	(0.07)	(0.07)	(1.00)
	[0.02]**	[0.93]	[0.02]**	1089
Welfare (PAP) index	0.15**	-0.04	0.19**	0.00
	(0.08)	(0.07)	(0.08)	(1.00)
	[0.06]*	[0.93]	[0.02]**	1087
Aspirations (PAP) index	0.07*	-0.01	0.08**	0.19
	(0.04)	(0.04)	(0.04)	(0.67)
	[0.08]*	[0.93]	[0.05]**	1909
Expectations (PAP) index	0.13***	0.01	0.11***	0.06
	(0.04)	(0.04)	(0.04)	(0.67)
	[0.01]***	[0.93]	[0.02]**	1909
Aspirations and expectations aggregate (PAP) index	0.15**	-0.01	0.15**	0.01
	(0.06)	(0.06)	(0.06)	(1.00)
	[0.02]**	[0.93]	[0.02]**	1909
Omnibus (PAP) index	0.23***	-0.03	0.26***	0.00
	(0.07)	(0.07)	(0.07)	(1.00)
	[0.01]***	[0.93]	[0.00]***	1091

Notes: OLS estimates of within-village treatment and placebo effects five years after the intervention (columns 1-2). Column 3 tests for differences between Columns 1 and 2. The comparison group comprises households from the 64 treated villages that were not invited to any screening. Column 4 displays the control mean, standard deviation, and total number of observations. All columns control for village fixed effects and characteristics of the household head (and spouse for individual-level outcomes): age, years of education, an indicator for being single, and an indicator for being male. For the Aspirations, Expectations, and the Aggregate Index, the unit of observation is the individual and standard errors in parentheses are clustered at the household-level. For the other indices the unit of observation is the household and standard errors in parentheses are robust to heteroscedasticity. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile. The Agricultural investment PAP index comprises the outcomes listed in the 'Investment-oriented behaviour' table (PAP, p. 28): specifically: total expenditure on agricultural inputs, livestock inputs, family and hired labour, total land area under cultivation, and land rented or sharecropped. The Educational investment PAP index includes outcomes from the PAP (p. 26), specifically number of children enrolled (ages 6-15) and expenditure on their schooling. The Welfare PAP index comprises the outcomes in the listed under 'household welfare': food consumption, frequent and infrequent expenditures, cigarette and tobacco consumption (positive), and a general well-being index (PAP, p. 28). The Aspirations and Expectations indices are standardised weighted averages of respondents' aspired (expected) income, wealth, social status, and children's education, weighted by subjective weights across these dimensions, with social status the additional category as listed in the PAP. The Aspirations and Expectations Aggregate PAP index is constructed following Anderson (2008), like in Table 7, but also includes the social status component. The Omnibus index combines the Agricultural investment PAP, Educational investment PAP, Welfare PAP, and Aspirations and Expectations Aggregate PAP indices into a single omnibus index, following Kling, Liebman, and Katz (2007).

Table A.18: Robustness tests for summary indices

After five years	ANCOVA		Pre-specified		PDS Lasso		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Control mean (SD) Total obs.
Agricultural investment index	0.21***	0.18***	0.23***	0.21***	0.17***	0.12**	-0.00
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.01]***	[0.00]***	[0.00]***	[0.00]***	[0.04]**	1082
Educational investment index	0.25***	0.21***	0.21***	0.23***	0.19***	0.19***	0.00
	(0.07)	(0.08)	(0.07)	(0.07)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.01]***	[0.00]***	[0.00]***	[0.00]***	[0.00]***	1082
Welfare index	0.15**	0.14**	0.15**	0.17**	0.11*	0.10	-0.00
	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)	(1.00)
	[0.03]**	[0.04]**	[0.04]**	[0.01]**	[0.08]*	[0.12]	1090
Aspiration index	0.12**	0.15**	0.13**	0.16***	0.09	0.12**	0.02
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.04]**	[0.01]**	[0.03]**	[0.01]***	[0.10]	[0.04]**	1904
Expectations index	0.21***	0.22***	0.22***	0.23***	0.18***	0.18***	0.01
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.00]***	[0.00]***	[0.00]***	[0.00]***	[0.00]***	1900
Aspirations and expectations aggregate index	0.19***	0.22***	0.20***	0.23***	0.16***	0.19***	0.01
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.00]***	[0.00]***	[0.00]***	[0.01]***	[0.00]***	1904
Omnibus index	0.26***	0.24***	0.30***	0.32***	0.20***	0.21***	-0.00
	(0.06)	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.00]***	[0.00]***	[0.00]***	[0.00]***	[0.00]***	1091

Notes: Estimates of within-village treatment effects five years after the intervention (columns 1-6). The comparison group comprises households from the 64 treated villages that were not invited to any screening. Columns 1-4 control for village fixed effects and characteristics of the household head (or the spouse for individual-level outcomes): age, years of education, an indicator for being single, and an indicator for being male. Columns 1-2 replicate the results in the main tables of the paper. The pre-specified specification (columns 3-4) uses the same controls as the previous two columns but does not control for the baseline value of the outcome. For the educational investment index, the specifications in columns 3-4 additional control for the number of children aged 0-15 at baseline to account for the baseline imbalance in the number of children. The PDS Lasso specification (columns 5-6) estimates treatment effects following the post-double selection Lasso method (Belloni, Chernozhukov, and Hansen, 2014)—selecting controls from those shown in Appendix Tables A.9 and A.10 and partialling out from the selection the village fixed effects and the baseline value of the outcome, when this is available, following Cilliers, Elashmawy, and McKenzie (2024). Column 7 display the control mean; standard deviation; and total number of observations. Heteroskedasticity-robust standard errors (or clustered at the household-level for individual-level outcomes) are in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in detail in Table 7. The unit of observation is the household, except for the aspirations and expectations indices (which are observed for both household head and their spouse). The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile.

Table A.19: Robustness tests for agricultural investments

After five years	ANCOVA		Pre-specified		PDS Lasso		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Control mean (SD) Total obs.
Household head and spouse's labour effort:							
Daily minutes working	28.01***	19.14**	28.30***	19.95***	26.97***	18.24**	326.16
	(7.80)	(7.95)	(7.67)	(7.69)	(7.42)	(7.51)	(200.01)
	[0.00]***	[0.03]**	[0.00]***	[0.02]**	[0.00]***	[0.03]**	1754
Daily minutes in leisure	-48.62***	-18.55*	-51.94***	-20.22**	-51.16***	-20.02**	830.59
	(9.98)	(10.32)	(9.61)	(9.91)	(9.39)	(9.63)	(187.38)
	[0.00]***	[0.07]*	[0.00]***	[0.04]**	[0.00]***	[0.04]**	1773
Agricultural investment:							
% with any spending on modern crop inputs			0.10***	0.06*	0.09***	0.04	0.58
			(0.03)	(0.03)	(0.03)	(0.03)	(0.49)
			[0.01]**	[0.29]	[0.03]**	[0.57]	1899
Spending on seed or fertiliser (USD) PPP			7.33**	3.53	5.92**	1.75	33.49
			(3.07)	(3.31)	(2.90)	(3.15)	(43.54)
			[0.05]**	[0.38]	[0.08]*	[0.66]	1089
% with any spending on feed or vet supplies			0.10***	0.14***	0.09***	0.12***	0.45
			(0.03)	(0.03)	(0.03)	(0.03)	(0.50)
			[0.01]**	[0.00]***	[0.03]**	[0.00]***	1078
Spending on feed or vet supplies (USD) PPP			2.68	4.52	2.09	2.36	29.30
			(4.81)	(4.63)	(4.51)	(4.26)	(70.92)
			[0.66]	[0.38]	[0.86]	[0.66]	1089
% with any spending on hired crop labour			-0.05**	-0.03	-0.05**	-0.03	0.36
			(0.02)	(0.02)	(0.02)	(0.02)	(0.48)
			[0.05]**	[0.38]	[0.05]**	[0.57]	1081
Spending on hired crop labour (USD) PPP			-1.30	3.67	-0.86	3.59	54.16
			(5.45)	(5.42)	(5.27)	(5.26)	(93.01)
			[0.81]	[0.50]	[0.90]	[0.66]	1089
Value of family crop labour (USD) PPP			33.33*	32.06	30.36	19.94	387.81
			(19.73)	(20.08)	(18.73)	(19.13)	(258.03)
			[0.15]	[0.29]	[0.17]	[0.59]	1078
Area cultivated (hectares)			0.01	0.02	-0.00	0.00	0.55
			(0.02)	(0.02)	(0.02)	(0.02)	(0.30)
			[0.65]	[0.38]	[0.90]	[0.82]	1079
Assets:							
Value of livestock (USD) PPP	112.75	89.51	187.38	310.16**	40.53	71.70	2018.22
	(122.97)	(114.28)	(135.89)	(130.45)	(116.16)	(106.57)	(1921.09)
	[0.36]	[0.51]	[0.17]	[0.04]**	[0.73]	[0.59]	1053
Value of tools (USD) PPP	21.27**	8.50	27.55**	15.37	23.73**	6.91	106.02
	(10.77)	(12.87)	(11.61)	(13.66)	(11.11)	(12.89)	(126.90)
	[0.10]*	[0.51]	[0.04]**	[0.26]	[0.06]*	[0.59]	1049

Notes: Estimates of within-village treatment effects five years after the intervention (columns 1-6). The comparison group comprises households from the 64 treated villages that were not invited to any screening. Columns 1-4 control for village fixed-effects and characteristics of the household head (or the spouse for individual-level outcomes): age, years of education, an indicator for being single, and an indicator for being male. Columns 1-2 replicate the results in the main tables of the paper. The pre-specified specification (columns 3-4) uses the same controls as the previous two columns but does not control for the baseline value of the outcome. The PDS Lasso specification (columns 5-6) estimates treatment effects following the post-double selection Lasso method (Belloni, Chernozhukov, and Hansen, 2014)—selecting controls from those shown in Appendix Tables A.9 and A.10 and partialling out from the selection the village fixed effects and the baseline value of the outcome, when this is available, following Cilliers, Elashmawy, and McKenzie (2024). Column 7 display the control mean; standard deviation; and total number of observations. The agricultural investment panel is reported only in columns 3-4 because the baseline value of the outcome was not collected. Heteroskedasticity-robust standard errors (or clustered at the household-level for individual-level outcomes) are in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in detail in Table 3. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1. The unit of observation is the household, except for the daily minutes working or in leisure (which are are observed for both household head and their spouse). The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile.

Table A.20: Robustness tests for educational investments

After five years	ANCOVA		Pre-specified		PDS Lasso		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Control mean (SD) Total obs.
Cohort 1: Children of post-primary school-going age at endline (aged 11–15 at the time of the intervention)							
Children aged 16-20 in school	0.06*	0.06	0.06*	0.06*	0.03	0.03	0.17
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.41)
	[0.08]*	[0.13]	[0.08]*	[0.11]	[0.39]	[0.33]	1078
Daily minutes in school for children aged 16-20	33.08**	30.69**	30.50**	30.00**	19.63*	22.10*	58.64
	(13.48)	(13.82)	(12.92)	(13.27)	(11.72)	(12.32)	(149.88)
	[0.03]**	[0.05]*	[0.04]**	[0.05]**	[0.18]	[0.14]	1070
Daily minutes studying for children aged 16-20	9.25**	7.56	7.86*	7.27	6.26	5.09	17.82
	(4.68)	(5.05)	(4.52)	(4.90)	(4.22)	(4.73)	(52.12)
	[0.06]*	[0.13]	[0.08]*	[0.14]	[0.18]	[0.33]	1063
Children aged 16-20 that attained 8th grade	0.09***	0.07**	0.08***	0.07**	0.07***	0.07***	0.07
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.26)
	[0.00]***	[0.04]**	[0.01]***	[0.05]**	[0.01]***	[0.03]**	1078
Cohort 2: Children of primary school-going age at endline (aged 2–10 at the time of the intervention)							
Children aged 7-15 in school	0.03	0.05	0.01	0.08	0.01	0.06	1.22
	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)	(0.06)	(1.18)
	[0.67]	[0.52]	[0.86]	[0.26]	[0.83]	[0.37]	1078
Daily minutes in school for children aged 7-15	23.47	37.11	11.47	45.84*	22.73	45.67*	527.12
	(30.16)	(30.22)	(25.84)	(25.31)	(24.98)	(23.91)	(437.21)
	[0.65]	[0.52]	[0.86]	[0.21]	[0.54]	[0.17]	1060
Daily minutes studying for children aged 7-15	17.34*	7.56	15.13*	9.59	16.53**	7.29	91.29
	(8.91)	(9.14)	(8.36)	(8.57)	(7.90)	(8.14)	(115.61)
	[0.16]	[0.52]	[0.21]	[0.26]	[0.11]	[0.37]	1061
For all children							
Schooling expenditure (USD) PPP	6.97**	5.81**	8.20***	6.88**	7.70***	6.23**	19.17
	(2.84)	(2.95)	(2.86)	(3.06)	(2.80)	(2.90)	(32.73)
	[0.01]**	[0.05]**	[0.00]***	[0.02]**	[0.00]***	[0.03]**	1065

Notes: Estimates of within-village treatment effects five years after the intervention (columns 1-6). The comparison group comprises households from the 64 treated villages that were not invited to any screening. Columns 1-4 control for village fixed-effects and characteristics of the household head: age, years of education, an indicator for being single, and an indicator for being male. Columns 1-2 replicate the results in the main tables of the paper. The pre-specified specification (columns 3-4) uses the same controls as the previous two columns but does not control for the baseline value of the outcome. Specifications in columns 3-4 additional control for the number of children aged 0-15 at baseline to account for the baseline imbalance in the number of children. The PDS Lasso specification (columns 5-6) estimates treatment effects following the post-double selection Lasso method (Belloni, Chernozhukov, and Hansen, 2014)—selecting controls from those shown in Appendix Tables A.9 and A.10 and partialling out from the selection the village fixed effects and the baseline value of the outcome, when this is available, following Cilliers, Elashmawy, and McKenzie (2024). Column 7 display the control mean; standard deviation; and total number of observations. Heteroskedasticity-robust standard errors are in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in detail in Table 4. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1. The unit of observation is the household. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile.

Table A.21: Robustness tests for welfare outcomes

After five years	ANCOVA		Pre-specified		PDS Lasso		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Control mean (SD) Total obs.
Food security:							
Food security index: z-score	-0.07	0.03	-0.06	0.04	-0.05	0.03	0.48
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.92)
	[0.28]	[0.61]	[0.31]	[0.54]	[0.37]	[0.62]	1076
Months of food insecurity	-0.33**	-0.38***	-0.32**	-0.35**	-0.25*	-0.28**	2.71
	(0.15)	(0.14)	(0.14)	(0.14)	(0.14)	(0.13)	(2.13)
	[0.05]*	[0.02]**	[0.05]*	[0.03]**	[0.14]	[0.08]*	1038
Consumption:							
Food consumption (USD) per ad. equiv. monthly PPP			-1.98	0.32	-1.90	0.30	53.91
			(2.05)	(2.07)	(1.96)	(1.98)	(29.98)
			[0.33]	[0.88]	[0.33]	[0.88]	1088
Frequent non-food (1m recall USD) per ad. equiv. PPP			0.44	0.40	0.35	0.32	4.08
			(0.28)	(0.30)	(0.26)	(0.28)	(3.69)
			[0.19]	[0.25]	[0.23]	[0.46]	1076
Nonfood consumption (12m recall USD) per ad. equiv. monthly PPP			0.70	1.24**	0.65	0.98**	7.47
			(0.51)	(0.48)	(0.48)	(0.45)	(6.35)
			[0.21]	[0.05]*	[0.23]	[0.14]	1076
Consumption of sin goods (USD) PPP			0.20	0.17	0.17	0.14	0.80
			(0.13)	(0.13)	(0.12)	(0.13)	(1.66)
			[0.19]	[0.25]	[0.23]	[0.46]	1079
General economic position (scale 1 to 4)			0.09*	0.09*	0.07	0.04	2.10
			(0.05)	(0.05)	(0.05)	(0.05)	(0.73)
			[0.19]	[0.21]	[0.23]	[0.46]	1078
Non-productive durables and housing:							
Value of durable assets excluding tools (USD) PPP	20.71**	22.36**	21.95**	24.88**	17.81*	15.17	70.55
	(9.97)	(10.17)	(10.75)	(11.18)	(9.42)	(9.59)	(127.39)
	[0.08]*	[0.06]*	[0.05]*	[0.05]*	[0.08]*	[0.23]	1049
Value of house (USD) PPP	384.94***	301.22***	415.56***	352.73***	312.29***	201.79**	1384.27
	(90.70)	(89.10)	(93.66)	(93.44)	(78.81)	(80.55)	(1235.57)
	[0.00]***	[0.00]***	[0.00]***	[0.00]***	[0.00]***	[0.05]**	1020
Non-organic roof	0.01	-0.01	0.06**	0.02	0.02	-0.02	0.68
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.47)
	[0.59]	[0.64]	[0.05]*	[0.49]	[0.32]	[0.48]	1036
Own toilet facility	0.07*	0.02	0.07*	0.02	0.07**	0.01	0.38
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.49)
	[0.08]*	[0.64]	[0.05]*	[0.49]	[0.08]*	[0.66]	1039
Wellbeing:							
Best life	0.22**	0.12	0.23**	0.17	0.12	0.05	4.83
	(0.11)	(0.11)	(0.11)	(0.12)	(0.10)	(0.11)	(1.80)
	[0.09]*	[0.58]	[0.09]*	[0.28]	[0.47]	[0.65]	1901
Happiest life	0.12	0.00	0.11	-0.01	0.02	-0.14	6.05
	(0.14)	(0.14)	(0.14)	(0.14)	(0.13)	(0.13)	(2.19)
	[0.38]	[0.98]	[0.42]	[0.95]	[0.85]	[0.56]	1885

Notes: Estimates of within-village treatment effects five years after the intervention (columns 1-6). The comparison group comprises households from the 64 treated villages that were not invited to any screening. Columns 1-4 control for village fixed-effects and characteristics of the household head (or the spouse for individual-level outcomes): age, years of education, an indicator for being single, and an indicator for being male. Columns 1-2 replicate the results in the main tables of the paper. The pre-specified specification (columns 3-4) uses the same controls as the previous two columns but does not control for the baseline value of the outcome. The PDS Lasso specification (columns 5-6) estimates treatment effects following the post-double selection Lasso method (Belloni, Chernozhukov, and Hansen, 2014)—selecting controls from those shown in Appendix Tables A.9 and A.10 and partialling out from the selection the village fixed effects and the baseline value of the outcome, when this is available, following Cilliers, Elashmawy, and McKenzie (2024). Column 7 display the control mean; standard deviation; and total number of observations. The consumption panel is reported only in columns 3-4 because the baseline value of the outcome was not collected. Heteroskedasticity-robust standard errors (or clustered at the household-level for individual-level outcomes) are in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in detail in Table 5. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1. The unit of observation is the household, except for the variables in the wellbeing panel (which are observed for both household head and their spouse). The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile.

Table A.22: Robustness tests for aspirations and expectations

After five years	ANCOVA		Pre-specified		PDS Lasso		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Treatment	Treat. vs. placebo	Control mean (SD) Total obs.
Aspirations: What would you like to achieve?							
Aspiration index	0.12**	0.15**	0.13**	0.16***	0.09	0.12**	0.02
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.04]**	[0.01]**	[0.03]**	[0.01]***	[0.10]	[0.03]**	1904
Aspired income (USD) PPP	2505.32	3148.93*	2436.44	3125.51*	2282.09	2953.73*	15446.71
	(1773.81)	(1701.71)	(1745.50)	(1697.89)	(1734.89)	(1666.89)	(27746.43)
	[0.22]	[0.10]*	[0.23]	[0.10]*	[0.19]	[0.11]	1864
Aspired wealth (USD) PPP	1584.05	664.91	1550.68	713.73	1745.25	837.37	11978.72
	(1301.17)	(1395.59)	(1298.04)	(1392.27)	(1277.01)	(1365.20)	(21518.47)
	[0.22]	[0.63]	[0.23]	[0.61]	[0.19]	[0.54]	1867
Aspired education (years)	0.29*	0.39**	0.31*	0.43***	0.27*	0.37**	14.26
	(0.16)	(0.16)	(0.16)	(0.16)	(0.15)	(0.16)	(2.60)
	[0.19]	[0.05]**	[0.16]	[0.02]**	[0.19]	[0.06]*	1769
Expectations: What do you expect in ten years?							
Expectations index	0.21***	0.22***	0.22***	0.23***	0.18***	0.18***	0.01
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.00]***	[0.00]***	[0.00]***	[0.01]***	[0.00]***	1900
Expected income (USD) PPP	309.11	238.22	306.52	199.29	140.94	41.78	3409.97
	(190.28)	(189.95)	(188.01)	(192.24)	(178.04)	(179.70)	(2820.92)
	[0.10]	[0.21]	[0.10]	[0.30]	[0.43]	[0.82]	1861
Expected wealth (USD) PPP	596.06**	369.61	612.21**	356.06	396.13*	108.87	4009.69
	(250.19)	(247.91)	(249.03)	(250.75)	(234.50)	(236.43)	(3997.53)
	[0.04]**	[0.20]	[0.02]**	[0.23]	[0.14]	[0.82]	1853
Expected education (years)	0.58**	0.90***	0.67***	1.02***	0.59**	0.91***	12.31
	(0.26)	(0.27)	(0.26)	(0.27)	(0.26)	(0.26)	(3.88)
	[0.04]**	[0.00]***	[0.02]**	[0.00]***	[0.07]*	[0.00]***	1706
Aspirations and expectations aggregate index	0.19***	0.22***	0.20***	0.23***	0.16***	0.19***	0.01
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(1.00)
	[0.00]***	[0.00]***	[0.00]***	[0.00]***	[0.01]***	[0.00]***	1904

Notes: Estimates of within-village treatment effects five years after the intervention (columns 1-6). The comparison group comprises households from the 64 treated villages that were not invited to any screening. Columns 1-4 control for village fixed-effects and characteristics of the household head or spouse: age, years of education, an indicator for being single, and an indicator for being male. Columns 1-2 replicate the results in the main tables of the paper. The pre-specified specification (columns 3-4) uses the same controls as the previous two columns but does not control for the baseline value of the outcome. The PDS Lasso specification (columns 5-6) estimates treatment effects following the post-double selection Lasso method (Belloni, Chernozhukov, and Hansen, 2014)—selecting controls from those shown in Appendix Tables A.9 and A.10 and partialling out from the selection the village fixed effects and the baseline value of the outcome, when this is available, following Cilliers, Elashmawy, and McKenzie (2024). Column 7 display the control mean; standard deviation; and total number of observations. Heteroskedasticity-robust standard errors are clustered at the household-level in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Outcome variables are listed on the left, and described in detail in Appendix Table A.1. All monetary values are in PPP-adjusted USD, set at 2016 (endline) prices and deflated using national non-food CPI. In 2016, USD 1 = 8.67 ETB (Ethiopian birr) PPP. The conversion is described in Appendix C.1. The unit of observation is the individual. The number of observations varies slightly across rows because some respondents do not answer all questions and because we trim continuous variables above the 99^{th} percentile.

Table A.23: Educational investments by proportion of female children

	After five years			
	(1)	(2)	(3)	(4)
	Treatment	Treatment*Share of girls	Share of girls	Control Mean (SD) Total Obs.
Cohort 1: Children of post-primary school-going age at endline (aged 11-15 at the time of the intervention)				
Children aged 16-20 in school	0.03	0.11	-0.31***	0.17
	(0.03)	(0.10)	(0.07)	(0.41)
				1031
Daily minutes in school for children aged 16-20	18.58*	68.00	-149.02***	58.64
	(10.25)	(42.01)	(24.34)	(149.88)
				1024
Daily minutes studying for children aged 16-20	5.96	24.48	-37.43***	17.82
	(4.02)	(17.30)	(9.78)	(52.12)
				1018
Children aged 16-20 that attained 8th grade	0.07***	0.08	-0.23***	0.07
	(0.02)	(0.08)	(0.05)	(0.26)
				1031
Cohort 2: Children of primary school-going age at endline (aged 2-10 at the time of the intervention)				
Children aged 7-15 in school	0.07	-0.08	-0.08	1.22
	(0.09)	(0.16)	(0.10)	(1.18)
				1031
Daily minutes in school for children aged 7-15	13.01	11.07	-50.15	527.12
	(31.90)	(58.49)	(36.84)	(437.21)
				1013
Daily minutes studying for children aged 7-15	26.95**	-24.67	-4.25	91.29
	(10.61)	(18.29)	(11.95)	(115.61)
				1016
For all children (aged 7-20)				
Schooling expenditure (USD) PPP	9.86**	-6.87	-5.45	19.17
	(4.39)	(6.73)	(3.55)	(32.73)
				1015

Notes: This table reports within-village heterogeneous treatment effects five years after the intervention. Column 1 reports the treatment effect relative to control group households with no girls. Column 2 shows how the treatment effect varies with the share of girls in the household within specific age ranges (interaction of treatment with the share of girls aged 16-20 in the top panel, 7-15 in the middle, and 7-20 in the bottom). The share of girls is defined as the number of girls in the relevant age group divided by the total number of children in that group. Column 3 reports the difference in the control group of households between households with only girls and no boys (where the share of girls within the specific age ranges is equal to one) and households with only boys and no girls. For households with no children in the age group, the share is coded as zero; we include an indicator for having no children in that age group as a control. The unit of observation is the household. All regressions control for household head characteristics (age, sex, marital status, education), village fixed effects, a placebo indicator, and baseline values of the outcome. Robust standard errors are in parentheses. Minimum q-values correcting across outcomes are in brackets. * denotes significance at the 10% level, ** at 5%, and *** at 1%. Column 4 reports the control mean, standard deviation, and number of observations.

Table A.24: Summary indices in spillover analysis

After five years	(1) Treatment	(2) Placebo	(3) Control	(4) Treat. vs. placebo	(5) Treat. vs. control	(6) Placebo. vs. control	(7) Pure Control mean (SD) Total obs.
Agricultural investment index	0.25***	0.03	0.01	0.22***	0.24***	0.02	0.00
	(0.09)	(0.09)	(0.09)	(0.06)	(0.06)	(0.05)	(1.00)
	[0.01]***	[0.84]	[1.00]	[0.00]***	[0.00]***	[0.84]	1223
Educational investment index	0.20***	-0.02	-0.00	0.22***	0.20***	-0.02	0.00
	(0.07)	(0.07)	(0.06)	(0.07)	(0.06)	(0.06)	(1.00)
	[0.01]***	[0.84]	[1.00]	[0.00]***	[0.00]***	[0.84]	1219
Welfare index	0.42***	0.26***	0.25***	0.16**	0.17**	0.01	-0.00
	(0.09)	(0.10)	(0.09)	(0.08)	(0.08)	(0.08)	(1.00)
	[0.00]***	[0.06]*	[0.05]*	[0.05]**	[0.04]**	[0.91]	1224
Aspiration index	0.22***	0.06	0.09	0.17***	0.13**	-0.04	0.00
	(0.08)	(0.08)	(0.07)	(0.05)	(0.06)	(0.06)	(1.00)
	[0.01]***	[0.84]	[0.51]	[0.00]***	[0.04]**	[0.84]	2231
Expectations index	0.27***	0.03	0.06	0.24***	0.21***	-0.03	-0.00
	(0.09)	(0.08)	(0.08)	(0.06)	(0.07)	(0.06)	(1.00)
	[0.00]***	[0.84]	[0.63]	[0.00]***	[0.01]***	[0.84]	2230
Reference-point index	0.25***	0.02	0.07	0.23***	0.18***	-0.05	-0.00
	(0.08)	(0.08)	(0.08)	(0.05)	(0.06)	(0.06)	(1.00)
	[0.01]***	[0.84]	[0.63]	[0.00]***	[0.01]***	[0.84]	2231
Omnibus index	0.34***	0.05	0.08	0.29***	0.25***	-0.03	-0.00
	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(1.00)
	[0.00]***	[0.84]	[0.51]	[0.00]***	[0.00]***	[0.84]	1219

Notes: OLS estimates of between-village effects five years after the intervention (columns 1, 2 and 3). Column 4 tests for differences in parameters obtained in first two columns. Column 5 tests for differences in parameters obtained in first and third columns. Column 6 tests for differences in parameters obtained in second and third columns. The comparison group comprises households from the ten pure-control villages that were first surveyed five years after the intervention. Column 7 displays the mean, standard deviation for the pure-control group, and total number of observations. All regressions control for screening-site fixed effects, individual characteristics of the respondent (age, years of education, an indicator for being single, and an indicator for being male) and village-level controls (the number of inhabitants, hectares covered by forest, an indicator for whether sorghum is the main crop, costs of trip to nearest market, an indicator for whether the village has a first cycle school, percentage of households with radio, distance to the next market place, distance to the school, distance to the next farmers training centre, distance to the next health centre, distance to the next river). Regressions on the educational investment index and omnibus index additionally control for the number of children aged 0-20 currently in the household to account for the baseline imbalance in the number of children. Heteroskedasticity-robust standard errors are clustered at the village-level and are in parentheses. Stars on the coefficient estimates reflect unadjusted p-values. Minimum q-values are in square brackets and are calculated over each panel of variables. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The unit of observation is the household, except for the aspirations and expectations indices (which are are observed for both household head and their spouse). The number of observations varies slightly across rows because some respondents do not answer all questions, though the indices aggregate all non-missing outcomes. The outcomes are inverse-covariance-weighted averages standardised relative to the pure-control group, following Anderson (2008). The agricultural investment index includes all outcomes reported in Table 3, with daily minutes in leisure being recoded to be negative. The educational investment index includes all outcomes reported in Table 4. The welfare index includes all outcomes reported in Table 5, with months of food insecurity in the last year recoded to be negative. The welfare index averages over the household head's subjective well-being outcomes. The aspirations and expectations aggregate index is made of the reported income, wealth and years of education for children, for aspirations and expectations. The omnibus index aggregates the agricultural investment, educational investment, welfare, and aspirations and expectations aggregate standardised indices into a single index, following Kling, Liebman, and Katz (2007). As the omnibus index is for the whole household, we use the household head's aspirations and expectations aggregate index.

Table A.25: Village-level balance (64 treated vs. 10 control villages)

	Sample Means		
Variable	Treatment	Control	p-value
Number of inhabitants	465	374	0.04
Number of households	79.3	66.4	0.09
Majority is Oromo	0.938	0.800	0.30
Percentage share of inhabitants belonging to the main ethnicity	94.0	94.7	0.82
Percentage share of inhabitants that are Muslim	86.6	85.7	0.91
Percentage share of inhabitants that are Orthodox	13.4	14.2	0.92
Agricultural land (hectares)	38.5	32.5	0.23
Cultivated land (hectares)	31.0	30.1	0.84
Irrigated land (hectares)	3.14	0.10	0.00
Grazing land (hectares)	0.531	0.400	0.72
Forest (hectares)	6.23	2.10	0.01
Most important crop is sorghum	0.922	1.000	0.02
Most important livestock is oxen	0.516	0.300	0.18
Inhabitants' main source of income is subsistence farming	0.641	0.700	0.71
Inhabitants' main source of income is cash crops	0.219	0.100	0.28
Percentage share of workers that are farmers	95.1	97.0	0.34
Percentage share of workers that are farm laborers	2.32	5.64	0.38
Percentage share of workers that are non-agricultural business owners	2.12	0.06	0.00
Percentage share of workers that are non-agricultural business workers	1.24	1.67	0.72
Cost of trip to nearest market (USD) PPP	4.32	4.04	0.63
Walking time to the nearest market (in minutes)	101	92	0.61
Village has first cycle school	0.250	0.200	0.72
Village has second cycle school	0.172	0.200	0.84
Village has TV transmission	0.234	0.200	0.81
Village has mobile network	0.937	0.700	0.12
Percentage share of inhabitants with a radio	36.5	31.6	0.54
Percentage share of inhabitants with a mobile phone	0.321	0.245	0.11
Main source of drinking water is pond/river/stream	0.031	0.100	0.49
Most important transportation mode is mules/donkey/horse	0.813	0.800	0.93
Distance to the nearest market (in kilometers)	10.0	8.9	0.50
Distance to next school (in kilometers)	1.27	0.93	0.09
Distance to next farmer training centre (in kilometers)	3.54	3.74	0.77
Distance to next health centre (in kilometers)	10.2	9.7	0.76
Distance to next river (in kilometers)	2.44	3.76	0.00
Omnibus F-test			0.12

Notes: Table shows means of pre-specified village characteristics collected from a village survey conducted five years after the experiment for each village group (columns 1-2). All variables are from village-level questionnaires collected five years after the experiment, except for the bottom three distance variables, which come from administrative data collected prior to the intervention (before baseline). Market centers were recorded by the IFPRI/FAO Environment and Natural Resources Service (SDRN) in 2004. Health centers and rivers information was gathered by the FAO Environment and Natural Resources Service (SDRN) in 2007. Column 3 shows p-values for equal means across groups, evaluating balance between treated and control villages. Hypothesis tests are based on heteroskedasticity-robust standard errors. Omnibus test randomised inference p-values on the bottom row, following Kerwin, Rostom, and Sterck (2024).

Table A.26: Cost-effectiveness Ratios at Different Scales

	(1) Experiment Scale (N=960)	(2) Reasonable Scale (N=57,750)
Costs (per household, USD PPP)		
Fixed cost	43.64	0.73
Variable cost	18.73	9.47
Total cost	**62.37**	**10.20**
Benefit-Cost Ratios		
Durable assets only	0.33	2.03
Durable + Livestock + Productive assets	2.48	15.18
All assets incl. housing	8.65	52.93

Notes: All costs are presented in 2016 USD PPP per household. Fixed costs at the experiment's scale include video production. Variable costs primarily consisted of transport, staffing, and projection costs. Data on costs at reasonable scale are from Bernard et al. (2019). Benefit-cost ratios are calculated using as benefits the five-year intention-to-treat effects relative to the control group, conservatively excluding any potential flow benefits in future from ownership of the asset. The first row of benefit cost ratios consider only benefits in terms of durable assets. The second row considers benefits in terms of durable assets, livestock assets and productive assets. The third considers in terms of durable assets, livestock assets, productive assets and housing.

Table A.27: Details on variable standardization in Appendix Figure A.5

Outcome	Paper Name	Variable definition	Expression of value	Timeline
Total assets	Bernard et al. (2025)	Value of tools, livestock, and non-productive durable assets	Control mean %	52 months
	Banerjee et al. (2015)	Total assets value	Control mean %	36 months
	Orkin et al. (2023)	Total assets value	Control mean %	17 months
Productive assets	Bernard et al. (2025)	Value of tools and livestock	Control mean %	52 months
	Orkin et al. (2023)	Value of tools and livestock	Control mean %	17 months
	Banerjee et al. (2015)	Sum of productive assets, livestock and agricultural tools	Control mean %	36 months
Durable assets	Bernard et al. (2025)	Durable assets, excluding tools	Control mean %	52 months
	Orkin et al. (2023)	Durable household assets	Control mean %	17 months
	Banerjee et al. (2015)	Household assets value	Control mean %	36 months
	Blattman, Jamison, and Sheridan (2017)	Durable consumption assets	Control mean %	12 months
Work time	Bernard et al. (2025)	Respondents' daily minutes working	Control mean %	52 months
	Banerjee et al. (2015)	Total minutes spent doing productive activities in last day	Control mean %	36 months
	Blattman, Jamison, and Sheridan (2017)	Hours per week of work in past month	Control mean %	12 months
	Blattman et al. (2023)	Hours per week in past 7 days	Control mean %	10 years
	Lubega et al. (2021)	Time spent working in own enterprise	Control mean %	6 months
	Batista and Seither (2019)	Workdays per week	Control mean %	6 months
	John and Orkin (2022)	Total hours of work in past 7 days	Control mean %	30-36 months
	Orkin et al. (2023)	Respondents' days of labour supplied across various activities in a year	Control mean %	17 months
Aspirations	Bernard et al. (2025)	Aspirations index	Control SD %	52 months
	Beaman et al. (2012)	Wishes child to graduate or get higher education	Control mean %	36-48 months
	Batista and Seither (2019)	Having a goal	Control mean %	6 months
	Orkin et al. (2023)	Aspirations index	Control SD %	17 months
	Lubega et al. (2021)	Ambition defined as a dummy	Control mean %	6 months
	Macours and Vakis (2014)	Strong positive expectations about the future	Control mean %	9 months
	Cecchi et al. (2022)	Aspirations index	Control SD %	3 months
	Rojas Valdes, Wydick, and Lybbert (2021)	Aspirations index	Control SD %	12 months
Expected education	Bernard et al. (2025)	Expected years of education of oldest child	Control mean %	52 months
	Orkin et al. (2023)	Expected education for eldest child	Control mean %	17 months
	Baranov et al. (2020)	Expected grade attainment	Control mean %	7 years
Educational investment	Bernard et al. (2025)	Educational expenditure	Control mean %	52 months
	John and Orkin (2022)	Total educational expenditure	Control SD %	30-36 months
	Baranov et al. (2020)	Monthly expenditure on education (ln)	Logarithmic value	7 years
	Orkin et al. (2023)	Total educational expenditure for children aged between 6 and 20 years old	Control mean %	17 months

Notes: The coefficients reported in Figure A.5 correspond to the "psychological" treatment arms of the different studies cited, except for Banerjee et al. (2015), where we report the three-year effects of a graduation program in Ethiopia. All monetary values are converted to 2016 USD PPP.

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