

THE FUTURE IN MIND: ASPIRATIONS AND FORWARD-LOOKING BEHAVIOUR IN RURAL ETHIOPIA*

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TANGUY BERNARD
STEFAN DERCON
KATE ORKIN
ALEMAYEHU SEYOUM TAFFESSE

Poor people often do not make investments, even when returns are high. One possible explanation is that they have low aspirations and form mental models of their future opportunities which ignore some options for investment. This paper reports on a field experiment to test this hypothesis in rural Ethiopia. Individuals were randomly invited to watch documentaries about people from similar communities who had succeeded in agriculture or business, without help from government or NGOs. A placebo group watched an Ethiopian entertainment programme and a control group were simply surveyed. In addition, the number of people invited was varied by village to assess the importance of peer effects in formation of aspirations. Six months after screening, aspirations had improved among treated individuals and did not change in the placebo or control groups. Treatment effects were larger for those with higher pre-treatment aspirations. We also find treatment effects on savings, use of credit, children's school enrolment and spending on children's schooling, suggesting that changes in aspirations can translate into changes in a range of forward-looking behaviours. There are also treatment effects on measures from psychology and sociology, including locus of control, which theory predicts should behave in similar ways to aspirations. Most effects are robust to corrections for multiple testing. Peer effects result in further impact on educational spending and induce more work and less leisure. The result that a one-hour documentary shown six months earlier induces actual behavioural change suggests a challenging, promising avenue for further research and poverty-related interventions. JEL codes: D03, I31.

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I INTRODUCTION

Governments and non-governmental organisations have long offered solutions to help poor people escape poverty that try to build up poor people’s assets via improved health, skills, education, or micro-finance, or fix market failures particularly costly for the poor. Such interventions are based on the conviction that poverty is driven by constraints faced by poor people. Just like any other human beings, poor people are assumed to grab opportunities and make decisions that optimise returns given the constraints they face (Schultz, 1964). Poverty reduction need only increase the opportunity sets faced by the poor.

There is extensive empirical evidence that market failures lead to underinvestment by poor people, such as linked to risk and credit in agriculture (Karlan et al., 2014), seasonal migration (Bryan et al., 2012) or health, despite large impacts on children’s health and school participation (Miguel and Kremer, 2004). Social and local political structures also help to explain why people often fail to invest even where there are opportunities with high returns. Gender norms and local power structures lead to underinvestment in maintaining land fertility (Goldstein and Udry, 2008). Norms about caste behaviour prevent parents enrolling children into newer forms of education despite large returns (Munshi and Rosenzweig, 2006).

However, people often underinvest even in the absence of market failures or constraining social structures. Advances in behavioural science have encouraged economists to reassess the simple “rational” view of human decision-making. Acquiring and processing information consumes energy and time. People thus use mental short-cuts – heuristics or rules of thumb – to filter, categorise and interpret information and make decisions almost automatically (Kahneman, 2002). Some short-cuts are innate and result from long-term evolution. Others are learned from our experiences, from parents or others in our communities, or from collective beliefs or social conventions developed over generations (Bisin and Verdier, 2001). There is thus heterogeneity between individuals and communities in the overarching “mental models” (Craik, 1943) that structure our perception and understanding of the world, the opportunities and possibilities that are available, the constraints we face, and what we and others are capable of.

Mental models help people in interpreting information and making decisions (Jones et al., 2011). However, they may also lead to cognitive biases, neglect of relevant information and underinvestment (Hoff and Stiglitz, 2010; Gilovich et al., 2002; Bénabou, 2013; Hanna et al., 2012). All decision-makers, rich and poor, exhibit such bounded rationality. But poor people also suffer the psychological stresses of poverty and scarcity, which have been shown to decrease cognitive capacity, exacerbate cognitive biases and lead to decisions that contribute to poverty persistence (Mani et al., 2013).

In this paper, we ask about the role played by poor people’s understanding of the opportunities they face by actively trying to change their mental models of their possible lives using an experimental design. We are not trying to use insights about particular cognitive biases to nudge people into specific behaviours (Thaler and Sunstein, 2008). Failures and biases in people’s mental processes no doubt matter: research demonstrates that impatience, discounting of long-term implications of choices, or struggles to commit lead to sub-optimal choices, costly for the poor (Mullainathan and Shafir, 2009; Duflo et al., 2008; Ashraf et al., 2006). We aim to look deeper, by affecting poor people’s perceptions of the possibilities for their own lives: their mental models about their opportunities and whether and how they can achieve them.

We do this in a poor, remote part of rural Ethiopia. We showed short documentaries in which people from similar backgrounds to the audience tell stories about their lives. They describe how they improved their socio-economic position from being poor or of average socio-economic position in their communities to being relatively successful. They achieved this through setting goals, careful choices, perseverance and hard work, and not based on help from government or NGOs. We find that this intervention changed aspirations and related psychological measures such as the locus of control, as well as future-oriented behaviour, namely saving, use of credit and investment in education, six months after the screening.

Aspirations are defined as forward-looking goals or targets (Locke and Latham, 2002). In economic terms, we might think of aspirations as bounds among individuals’ preferences, the elements of the choice sets which they consider as relevant for them and motivate their actions. While forming aspirations, we dismiss some options, and fail to even imagine other options – we ignore part of a possible choice set. Once formed, our aspirations can function like other mental models in limiting the possible futures we consider by focusing our attention on some future options and filtering out others.

Appadurai (2004) and Ray (2006) argue that individuals largely form aspirations by observing the outcomes of other individuals whose behaviours they can observe and with whom they can identify. Social psychologists also argue that aspirations and broader beliefs about self-efficacy are largely modelled on the experience of others in the immediate environment (Bandura, 1977). Beaman et al. (2012) find that, when Indian villages were randomly assigned to reserve a seat on the village council for a woman and girls had female role models in leadership positions, the gender gap in occupational aspirations declined among girls themselves and among parents. This also altered behaviour: the gender gap in adolescent educational attainment disappeared and girls spent less time on household chores. Female leadership may have affected these outcomes through public good provision or other policy effects that changed opportunities and constraints for girls, although the authors offer suggestive evidence that the role model effect was important and that labour market opportunities remained unchanged.

By using short documentaries recounting life stories, and without any further interventions, we can offer a clear link between exposure to potential role models and subsequent outcomes. In this sense, we are closer to Chong et al. (2012) and Chong and La Ferrara. (2009), who show that exposure to TV shows with strong female role models and smaller families in Brazil reduces fertility and increases divorce, or Jensen and Oster (2009), who show that exposure to soap operas depicting urban women reduces fertility and domestic violence and alters beliefs about women’s autonomy in rural India. However, by using an experimental design, we can offer cleaner identification of a link between exposure to the documentary and changes in aspirations and behaviour. Most importantly, by introducing a placebo screening in our experiment, in the form of a popular Ethiopian entertainment show of short comedy sketches, we overcome the potential problem that the impact is just based on exposure to TV in a remote area, rather than on the actual content of the documentary. Finally, by introducing variable exposure to the documentaries and placebos to individuals’ peer groups within the village, our design can also assess the indirect role of exposure through friends and village networks.

We find evidence of direct treatment effects on aspirations and related psychological measures such as the locus of control, as well as effects on behaviours such as savings, use of credit, spending on children’s education and enrolment of the children of treated individuals. We also find suggestive evidence that those whose peers saw documentaries improved their aspirations and spending on children’s education, even if they did not see the documentaries themselves.

Other experimental studies show that providing concrete information about untapped opportunities can boost investment. Jensen (2012) finds that, when Indian villages randomly received recruitment visits which publicised job opportunities in call centres for women, young women in these villages increased their labour market participation and enrolment in relevant courses, delayed their marriage and births of children, and reported higher career aspirations. Parents enrolled girls in school more and fed their daughters better. Jensen (2010) finds that returns to education in the Dominican Republic were underestimated and that providing information on returns reduced school drop-out, at least for less poor students. Hanna et al. (2012) improved the efficiency of input allocation on seaweed farms in Indonesia by pointing out specific inputs that they could gather easily on their farms but were not using.

Unlike these studies, we are not trying to change behaviour via specific relevant information on untapped opportunities. In our case, the life stories narrated by the subjects in the documentaries are not specifically about particular actions that ought to be followed and do not draw general conclusions about opportunities for others based on the experience of the documentary subject. As in Chong et al. (2012), Chong and La Ferrara. (2009), Berg and Zia (2013) and Jensen and Oster (2009), any changes in our study are linked to a “vicarious experience” (Bandura, 1977), where watching the documentary provides audience members with a resonant, salient experience of what a different life might be like. As a result of this experience, they may re-evaluate their perceptions of their own lives and opportunities. Our finding that aspirations, other psychosocial measures and future-oriented behaviour are all affected is strongly suggestive that the experience of watching the documentary enabled a shift in people’s mental models, in small but perceptible ways.

The next section offers a discussion of the concept of aspirations in the economics and social psychology literature. Section III describes the experimental design and the data, including the measures of aspirations. Section IV describes the direct impact of the treatment on aspiration and future-oriented behaviour. Section V explores the robustness of the results and our interpretation, by examining the effect of the intervention on locus of control, risk aversion, time discounting, and other psychosocial measures. Section VI presents the analysis of peer effects.

II ASPIRATIONS – CONCEPT AND MEASUREMENT

The word “aspiration” means “a desire or ambition to achieve something” (Oxford English Dictionary, 1989). The word signifies some goal or target and a desire to attain it, but also suggests the intention to exert effort towards realising the goal. The conceptualisations of aspirations in the academic literature largely share this everyday understanding of the concept.

Pioneers in sociology and social psychology identified aspirations with goals (for a review in psychology, see Fishbach and Ferguson (2007)). Haller and Miller (1963) writes that “(a)t perhaps the most fundamental level, the term indicates that one or more persons are oriented toward a goal.” More recent work adopts similar definitions (Sherwood, 1989; Quaglia and Cobb, 1962; Ryan et al., 1999). In economics, aspirations appear in Herbert Simon’s “satisficing” approach. Simon (1955, 1979) argues that full rationality is beyond the reach of economic agents because of the complex environment in which they function, their limited cognitive and information-processing capabilities, and the costs of processing information. Instead, he characterises decision-making as a search for alternatives that meet or exceed specified criteria or aspiration levels:

... one could postulate that the decision maker had formed some aspiration as to how good an alternative he should find. As soon as he discovered an alternative for choice meeting his level of aspiration, he would terminate the search and choose that alternative. I called this mode of selection “satisficing” (Simon, 1979).

The literatures in sociology, social psychology and economics on the nature, formation, and significance of individual aspirations come to similar conclusions on a number of issues. First, aspirations express goals or goal-orientations (or desired future end-states) that are relevant to well-being, broadly defined. Second, aspirations evolve over time in response to life experience and circumstances (Haller and Miller, 1963; Appadurai, 2004; Ray, 2006; Quaglia and Cobb, 1962; Simon, 1979). Authors have used different terms for the experiences that shape aspirations: “intra-personal and extra-personal environment” (Haller, 1968), “vicarious experience” (Bandura, 1977), and “aspiration window” (Ray, 2006) to cite a few. In particular, social comparisons and learning from relevant others are important determinants of aspirations.

Third, as goals, aspirations are an important influence on behaviour (or actions) and thus attainment or outcomes. Aspirations motivate behaviour: “... [they] serve to mobilise and direct energy into action with respect to their objects, thus providing motive power for action” (Haller and Miller, 1963, 11). The link between aspirations and behaviour has mostly been studied in relation to occupational choice (Haller and Miller, 1963; Haller et al., 1974; Cook et al., 1996) and educational attainment (Quaglia and Cobb, 1962; Page et al., 2007; Beaman et al., 2012). These studies find significant impact of aspirations on choice and/or attainment.

Conceptually, aspirations are boundary-states which are sought after in a relevant domain of choice. In other words, an aspiration expresses a preference for a “state of the world” where the relevant goal is achieved, instead of other states.¹ Although most work examines educational and occupational aspirations, individuals may hold aspirations in many domains. Aspirations are different from beliefs, which are stances of individuals about the nature and configurations of the present state of the world and other potential states, the link between actions and outcomes, and the possible behaviour of others (Denzau and North, 1994). Aspirations are also not simply expectations of what the future will be like, as individuals may aspire to outcomes that would only be possible if constraints were lifted or if they changed their behaviour. Nevertheless, aspirations, beliefs and expectations are not unrelated: the beliefs held by individuals about their environment and themselves, including expectations, will influence their aspirations.

We use survey data to construct measures of aspirations in four specific dimensions: income, wealth, social status and children’s educational attainment. For each of these dimensions, respondents were asked two questions: what level on this dimension they would like to achieve (which we refer to as “aspirations”) and what level they thought they would reach in ten years (which we refer to as “expectations”). The survey instrument’s validity and reliability was tested in 2009 in 16 villages in central Ethiopia (Bernard and Taffesse, 2014).² Income, measured in Ethiopian

1. As goals that provide rationale for corresponding behaviour, aspirations are akin to preferences. Recall that Bowles characterised preferences explicitly as “reasons for behaviour” (Bowles, 2004). Furthermore, aspirations are context-dependent and changing. This is consistent with the recent emphasis on the endogeneity of preferences (Bowles, 1998; Fehr and Hoff, 2011; Hausman, 2012).

2. The validity and reliability tests were performed on the aspiration indicator only and rested on a slightly different wording, namely “what is the level that (they) would like to achieve *in their life*”. The phrase “in your life” was removed so respondents would report the highest achievement they sought rather than the level at the end of their life. Results from (Bernard and Taffesse, 2014) suggest the

birr (ETB) includes cash income from all activities. Wealth focuses on durable wealth (including housing, vehicles, furniture and other valuable durables). Education was measured in the years of schooling the respondent wanted their eldest child to complete. We include codes for different types of post-school education available in Ethiopia, so completing a three-year university degree is 15 years of education, while a one-year diploma is 13 years. Social status was measured as the percentage of community members who would ask for the respondent’s advice at times of important decisions.

We asked respondents to weight the four dimensions according to their own assessment of each dimension’s significance for them, which accounts for heterogeneity in valued attributes of life. We used these weights to aggregate the standardised responses to each of the four dimensions into an aspirations index. In particular, let a_i^k be individual i ’s aspiration for dimension k . w_i^k is the weight that individual i assigned to this dimension. μ_i^k and σ_i^k measure the sample mean and standard deviation at baseline on dimension k . The aspiration index is thus $A_i = \sum_k \left(\frac{a_i^k - \mu_k}{\sigma_k} \right) w_i^k$. Overall, our measurement approach is similar to Beaman et al. (2012), except that each aspiration constituent is numerical (as opposed to categorical) and weights are specific to the individual.

III EXPERIMENTAL DESIGN AND DATA

People may not aspire to a different life because they do not believe change in their circumstances is possible. We assess whether individuals revise their aspirations after a “vicarious experience” (Bandura, 1977), where they are exposed to the lives of potential role models from a similar background to theirs who have improved their socio-economic position. There is a substantial literature on exposure to role models in laboratory experiments. For example, Stout et al. (2011) find improvements in self-efficacy, career aspirations and effort in science subjects among female calculus students after they are exposed to photographs and videos of female role models in science. Greene et al. (1982) find ninth graders are more likely to rate jobs not traditionally filled by women as suitable for both men and women after hearing interviews with women working in these occupations. We bring such experiments into the field in deprived settings, and re-survey individuals both immediately after screening and after six months to examine the persistence of any changes in aspirations and related behaviour.

III.A *The study site and the experimental design*

The field experiment was conducted during 2010 and 2011 in Doba, an administrative district in Oromia region, 380 km east of Addis Ababa (Ethiopia’s capital city). The documentaries all featured rural inhabitants in grain-growing areas, so the study site was selected to be similar. Doba is poor and food-insecure: it was one of the first districts selected in 2005 for the Productive Safety Net Program (PSNP), which is targeted at the most chronically food-insecure districts in Ethiopia. According to the 2007 Census, only 1.5 per cent of Doba’s population live in urban areas and 99 per cent are subsistence farmers growing sorghum and maize (Central Statistical Agency, 2007).

We used the Central Statistical Agency’s list of rural villages for the district to create a list of villages with 50-100 households in them and randomly selected 64 villages for the intervention. One team of 30 enumerators moved from village to village to ensure homogeneity in how the screenings were conducted. The baseline survey and screening took place between September and November 2010 and the follow-up survey occurred six months after the baseline.

The district is remote: the majority of villages surveyed were only accessible by 4x4 vehicle and some required camel transportation. There is limited exposure to television: at baseline only 10 per cent of respondents watched TV once a week or more, 29 per cent watched at least once a month and 61 per cent watched about once a year or never. The remoteness of the district and the relative lack of exposure of the audience to television mean that the screenings were a highly unusual event, so the screening might have an effect on aspirations and behaviour independently of its content.

To capture the effect of being invited to a screening, we implemented a placebo design. Other studies which have examined the effect of providing information (Card et al., 2010, 2012) or watching soap operas (Berg and Zia, 2013) have also used a placebo arm. In each village, the enumerators compiled a list of all households, randomly selected 18 households and randomly assigned 6 households to each of the treatment, placebo and control groups. Thus, one

measure had high reliability and validity, provided experiences enumerators are used. The enumerators in this study were all experienced. Two days of the two weeks of survey training were dedicated to the administration of the aspiration-related questions.

third of the households we surveyed received an invitation to watch a screening of the four 15 minutes documentaries. Another third received an invitation to watch four fifteen-minute segments of an Ethiopian comedy TV show about rural life. The remaining third were surveyed but did not receive any invitations. Randomisation was at household level, so household heads and their spouses had the same treatment status. A baseline survey was conducted with all household heads and their spouses at their home (n=2,063, see first column of Table I).

At the end of the baseline interview, treatment and placebo households received two tickets, one for the household head and one for their spouse (if they had a spouse), for a screening session in a few days time. To avoid priming effects, respondents were simply told that the screening was an entertainment show. Households were told that tickets were non-transferable, that each respondent would receive a bag of sugar after the screening, and that they could only attend the screening at the time and place written on the ticket.

Ticketing was tightly controlled at the screening to ensure compliance with treatment assignment. Tickets were colour-coded by treatment and had the name and survey identifier of each respondent on them. Tickets were checked against the sampling frame at the door to the screening. Straight after the screening, in the screening venue, enumerators interviewed both household members again to measure aspirations and expectations.

On the same day as the screening, some enumerators interviewed control households at their homes to capture follow-up measures of aspirations and expectations from control households and also prevent control households from attending the screening. Control households also received a bag of sugar at this follow-up interview.

III.B Documentary and placebo content

In 2009, we asked development agents and NGO staff in rural areas to submit descriptions of the life stories of ordinary individuals who had improved their socio-economic well-being significantly despite adverse initial conditions. Ten individuals were selected to have short documentaries made about their lives by Next Studios, an Ethiopian production company, in which they narrate their life story. Of the ten documentaries, four were selected for the intervention, two about men and two about women. Each documentary is 15 minutes long and in Oromiffa, the local language in the study site.³ Three documentary subjects were from other districts in the Oromia region, of which Doba is a part, and one was from Amhara, a neighbouring region. It was thus almost impossible that respondents would know anyone in the videos and we have no evidence that this happened.

The documentaries have common themes. Firstly, no individuals featured were rich or powerful to begin with or became rich or powerful in the documentaries: all were ordinary rural residents who were either poorer than or of similar socio-economic status to those around them. They all took slightly different courses of action to those around them: starting a small business, diversifying their source of income, improving their farming practices, or acting outside cultural norms by adopting non-traditional divisions of household responsibility between spouses. Secondly, spouses and mentors highlight the personal qualities of the subjects, such as perseverance, determination and reliability. The subjects also emphasise the importance of setting goals and working towards them. Thirdly, individuals succeed largely through their own efforts and, in some cases, by drawing on assistance from community members and available resources, not through receiving interventions from government or NGOs.

The literature on aspirations highlights that people may form unrealistically high aspirations if they lack role models or “reference points” who come from similar backgrounds to them (Genicot and Ray, 2014; Ray, 2006). We avoid this by focusing on individuals to whom the audience can directly relate. When those who saw the documentary were asked at endline about the story they found the most relevant to them, 52 per cent of audience members thought the documentary subjects had initially been worse off than they currently were (11 per cent said the subject was initially the same as them, while 36 per cent said the subject was better off than them). The other concern is that the intervention will not raise aspirations enough, if documentaries are about very poor individuals who face much harder challenges than those faced by the audience. However, 73 per cent of the audience said that by the end of the documentary the documentary subjects were better off than they were currently (21 per cent said the subject was worse off and 5 per cent said the subject was the same as them). The subjects of the documentaries start from a similar position to the audience but provide an example of a better life to which the audience could reasonably aspire.

3. The documentaries, with English subtitles, and one of four placebo segments are at <https://www.youtube.com/channel/UCqfoNjCzt8YPjTRWQaMQfAg>. Appendix C summarises two documentaries and one placebo segment.

III.C Peer-level treatment

An individual’s peer networks are likely to affect her aspirations. Individuals largely model their aspirations on the experience of others in the immediate environment whom they can observe and with whom they can identify (e.g. Macours and Vakis (2014); Ray (2006); Beaman et al. (2012)). Ray (2006) calls this group of people an individual’s “aspiration window”. To explore the role social interactions play in how aspirations and behaviour change in response to the treatment, we generated exogenous variation in the extent to which an individual’s network was exposed to the treatment. Villages are about 30 minutes walk apart and individuals’ networks of close friends were mostly in the same village (see Section VI.A). We thus focus on spillovers between individuals in the same village.

The 64 villages were grouped into 16 screening sites with four neighbouring villages in each site. In each screening site, the survey team identified one central location, usually a school or farmers’ training centre, for the screening. Enumerators conducted the baseline in all four villages and then conducted the treatment and placebo screenings on one day at different times. The screenings used a projector and speakers connected to a power generator.

In each screening site of four villages, we randomly assigned two villages to be “intense treatment” villages and the other two to be “intense placebo” villages. In the “intense treatment” villages, we randomly selected 18 additional households (roughly 36 individuals, or 1,152 individuals in total) to be invited to the documentary but did survey these individuals. In the “intense placebo” villages, we similarly randomly selected 18 additional households to be invited to the placebo session. We discuss the results of the peer-level treatment in Section VI

III.D Compliance and experimental integrity

Compliance is reported in Table II. If all the sampled households had a head and a spouse, there would have been 768 individuals in each of the treatment, placebo and control groups. However, 95 individuals were single, widowed or divorced, so the household was only given one ticket. A further 95 individuals were not interviewed in the baseline or given tickets because they were away, ill or had just given birth. The remaining individuals were interviewed and given tickets. Compliance among those allocated tickets is high, despite an average 29 minutes travel time to the screening venue: 96 per cent of surveyed individuals and 92 per cent of non-surveyed individuals invited to screenings were recorded arriving at the correct screening.⁴ There are no significant differences in compliance between groups.

Attrition between baseline and follow-up is very low because households were visited repeatedly in the second round if someone was absent. Only 48 individuals (2.3 per cent of the sample) were not found for the second round. There are no significant differences in attrition rates between groups. In the rest of the paper, we examine the 2,063 individuals who appeared in both rounds.⁵

Table III reports tests of balance for both treatment and placebo experiments on individual and household-level variables. There are no significant differences across groups in gender or age of the individuals, nor in the frequency with which they watch TV, listen to the radio, travel outside the district or have lived outside the district, variables which proxy for their level of exposure to the opportunities of individuals outside their village. There is a small imbalance between the treatment and control groups in the proportion of people who are single, widowed or divorced, but only a small proportion (6 per cent) of the sample are single. There is also a small imbalance in the highest grade attained between the treatment (1.49 years) and control (1.26 years) groups.

The household head answered questions on household assets and food security. To measure assets, we asked household heads which of a list of durable goods – tools, furniture, electrical goods and carts or bicycles – they owned and their estimated resale value. We did not include land and houses. Instead of a consumption module, we use a version of the United States Department of Agriculture’s food insecurity questionnaire (Bickel et al., 2000; Carlson et al., 1999) adapted for Ethiopia (Hadley et al., 2008). This standardised set of questions captures the frequency of actual recent experiences of hunger, asking if adults or children had cut portion sizes, skipped meals or gone for a

4. We consider an individual as compliant if we recorded the time that they arrived at the correct screening. For 153 individuals, the time of arrival at the screening venue was not recorded, but the individuals were also not marked as absent. If these individuals are included, compliance rates rise to 98 and 97 per cent for surveyed and non-surveyed households. Only 20 respondents attended the incorrect screening.

5. We trim the sample for all continuous outcome variables used in the paper. Individuals who report values on the outcome variable which are four standard deviations or more above or below the sample mean have that outcome variable replaced as missing. This applies to 26 observations on the expectations measure (1 per cent of the sample of individuals) and 61 observations on the measure of aspirations (3 per cent of the sample). We use the same procedure on the other outcome variables considered, and we lose similar percentages of observations in this way. Trimming does not affect the magnitude or significance of results.

whole day without food, as well as perceived levels of food insecurity, with items such as "We worried whether our food would run out before we got money to buy more". Households are classified as food-secure, food-insecure without hunger and food-insecure with hunger. Across groups, 59 per cent of households at baseline are food-insecure with hunger, reflecting high levels of food insecurity in the district. There is no difference between groups in the total value of household assets or in the distribution of households over categories on the food security measure using a chi-squared test.

III.E Aspirations and outcomes at baseline

We test the impact of the experiment on aspirations and other outcome indicators that capture future-oriented behaviour. As discussed in Section II, we use an aspirations index based on a standardised weighted average of aspirations measured over four dimensions: income, wealth, education and status. Table IV reports expectations and aspirations at baseline, using non-standardised variables.

Mean expectations and aspirations are relatively high. Current household income levels at baseline are 6,243 ETB (about 367 USD at exchange rates at the time of the survey, 1 ETB=17 USD). Households report durable wealth in the form of assets of around 7,420 ETB (436 USD). At baseline, respondents expect to do rather well in ten years time, expecting on average 5.3 times their current income and 3.8 times their current wealth. The mean for income aspirations is roughly 23 times the mean for current income (although, given the poverty level they start from, and with a household size on average of 5.81, this is still only in the order of per capita annual income of about 1,480 USD). Similarly, the mean for wealth aspirations is 21 times the mean for current wealth.

However, the mean is easily affected by outliers.⁶ When one examines the median, income and wealth expectations and aspirations for the bulk of respondents are relatively low and the gaps between expectations and aspirations are much smaller. The median respondent has income and wealth of 4,000 ETB (237 USD) at baseline. They aspire to income and wealth of 20,000 ETB (1,176 USD, per capita roughly 202 USD for an average sized household) and expect income of 12,000 ETB (706 USD) and wealth of 14,000 ETB (824 USD respectively) in ten years.

Gaps between aspirations and expectations are smaller for education and social status. Mean expectations are full secondary education (12.43 years), and aspirations are somewhat higher (12.91 years), including some college education for many. Currently, about 44 per cent of individuals said they were consulted for important decisions in the village – our measure of social status. At baseline, they aspire to reach 75 per cent and expect to reach 71 per cent. There are no significant differences between the treatment, placebo and control groups at baseline.

Table IV also reports means of the weights which individuals assign to each dimension at baseline. Each respondent was given twenty beans and a piece of paper divided into quadrants, one representing each dimension. Enumerators explained which dimension each quadrant represented and asked respondents to allocate the beans according to the relative importance they gave to each dimension. Average weights are relatively close to five beans for each dimension, although simple t-tests across dimensions show differences are statistically significant. In addition, differences in the weights given to each dimension can be quite large within each individual.

Across all four dimensions, aspirations are mainly higher than expectations. Appendix Figure A.I shows that, except for a very small percentage of the sample at low values of the status variable, measures of aspirations stochastically dominate measures of expectations, as would be expected. The percentage of respondents aspiring to a particular level of each dimension is higher than the percentage of respondents expecting to reach that level. In addition, there is a larger tail of low expectations in education than in aspirations.

Nonetheless, we are cautious about reading too much in the difference between our measures of expectations and aspirations. Although expectations are conceptually different from aspirations, as discussed in Section II, it is difficult to measure or treat aspirations and expectations entirely separately. The issue is not one of translation: we translated and back-translated measures to ensure precision. Rather, the formation of aspirations and expectations is related: individuals' mental models of their future opportunities are likely to shape both aspirations and expectations. An intervention which alters aspirations is also likely to affect expectations. Furthermore, the survey first asked about aspirations and then expectations, so framing effects may well influence the response to the expectations question.

6. For example, there are three very large outliers on wealth aspirations, so even when we trim observations four standard deviations or more above or below the sample mean, the sample standard deviation is relatively large and the final sample includes high levels of wealth aspirations in placebo and control groups at baseline.

Appendix Table A.II shows that aspirations and expectations at baseline are strongly correlated (the correlation coefficient is 0.33).

Appendix Table A.I reports correlations between the aspirations index and aspirations in each dimension of life with baseline characteristics. Relationships are largely in the expected direction. Individuals with higher levels of education have higher wealth aspirations, higher educational aspirations for their children and higher status aspirations for themselves. Wealthier individuals (measured by their household assets) have higher aspirations for their own income – an increase of 1,000 ETB in household assets is associated with an increase of 5,089 ETB in income aspirations. Women have significantly lower aspirations than men on all dimensions except wealth, where their aspirations are lower but not significantly so. Older individuals have higher status aspirations, but not higher wealth or income aspirations, than younger individuals.

We also investigate whether our experiment affected actual behaviour. We measure behaviours that might suggest individuals are planning more for the future or making investments that might improve their future economic position. We could not investigate whether individuals altered their productive activities, such as by diversifying into non-agricultural activities, farming new crops or using different farming methods, as the endline survey took place only six months after the experiment and individuals would have required at least a full agricultural year to make such changes. Instead, we focus on time allocation, savings and credit behaviour and children’s education. Table IV reports descriptive statistics for these outcome variables at baseline.

To capture time allocation to work and leisure, the household head reported the amount of time each member of the household spent on various types of tasks on a typical day during March.⁷ We focus on time spent working on the farm or in business and compare this to time spent in leisure (including eating, bathing and sleeping).⁸ At baseline, individuals spent an average of 5.8 hours in farm work, and 12.3 hours for leisure on a typical day of March.

We define an individual’s savings as the total savings each respondent has in all possible savings places. We ask about respondents’ cash savings in banks, co-operatives, voluntary savings and loan groups, traditional *iqqub* (a version of a rotating credit and savings association), with a friend or relative, or at home. Baseline levels of savings are low: at baseline, 78 per cent of respondents had no cash savings and the average stock of savings per individual amounted to 83 ETB – roughly 5 USD – including those with no savings.

We also ask about all loans larger than 15 ETB (just under 1 USD), including from co-operatives, banks or micro-finance institutions, money-lenders, savings and loan groups, *iqqub* and friends or family. Total credit is the total value of the loans larger than 15 ETB the respondent has taken out in the last six months, considering only the principal (the value of the loan when it was first taken out). At baseline, 43 per cent of individuals had taken out a loan of more than 15 ETB in the past six months. On average, individuals had taken out a total of 176 ETB (10 USD) in the preceding six months, including those who had not taken out any credit.

We also examine hypothetical demand for credit by asking household heads how much they would borrow if they had an opportunity to borrow without paying interest. This is intended to capture the demand for credit in a simple way. This may not be captured by the actual loans people take, as there are likely to be credit market failures in any rural setting and in Ethiopia government imposes strict limitations on lending. Respondents were asked: “Someone from a micro-finance institution came to you and offered to lend you any amount of money you ask without charging interest or service charge. (1) How much would you ask for if the loan is payable in 1 year?”. The question was repeated for loans with a 5 and 10 year maturity. The amounts individuals would borrow increase with the length of the repayment period, from 5,867 ETB if the loan is payable in one year, to 12,448 ETB if the loan is payable in five years, to 22,210 ETB if the loan is payable in ten years. This is consistent with other findings that loan size is responsive to changes in loan maturity (Karlan and Zinman, 2005).

Finally, we measure the number of children in the household between the ages of 6 and 15 who were enrolled in school at the beginning of the relevant school year. At baseline, the average household has 1.02 children aged 6 to 15 enrolled in school.⁹ 24 per cent of households with children in this age group had no children enrolled in school. We

7. The baseline and endline took place at different times of year. The time allocation questions in the baseline and endline referred to March 2010 and 2011, respectively, to avoid seasonal differences in time allocation between baseline and endline. These would in any case be unrelated to treatment.

8. We focus on time spent on farm and business work, as the head may be more likely to report accurately on these measure, and not time spent working in the home, which is difficult to measure because it is extremely fragmented, often entails multiple tasks performed at the same time, and is more likely to be poorly estimated by heads for all the members.

9. These variables are at the household level. At baseline, we asked about the number of children in the household who were enrolled in school at the beginning of the 2009/10 school year in September 2009. We included all children resident in the house, including those who

also examine annual spending on schooling for children in the household in the previous school year, a total of the amount spent on uniforms, stationery and books, textbooks, and donations to the school – at baseline 186 ETB (11 USD) per year (including households with no spending on schooling). This is consistent with estimates in other rural parts of Ethiopia (?).

There are no significant differences between treatment, placebo and control groups on time allocation, savings, credit or hypothetical credit variables. There are differences between the treatment and control groups on education spending and enrolment. We examine 48 variables at baseline across three groups, resulting in 96 comparisons between treatment and control groups and placebo and control groups in total (we report on a further set of outcomes – risk aversion, discount rates and a variety of psychological measures – in Section V). We find imbalances in six comparisons at the 90 per cent level, two at the 95 per cent level, and one at the 99 per cent level, roughly as many as would arise by chance, suggesting that the randomisation was correctly performed. Further, and as discussed later on, all estimates are tested for robustness to the inclusion of baseline values of these variables, and results are largely robust.

IV EMPIRICAL STRATEGY AND RESULTS

IV.A Empirical strategy: direct effects

We first examine direct effects on individuals from being directly treated in the experiment by being invited to attend a documentary screening.¹⁰ We use the same specifications to examine whether exposure to the documentary causes changes in aspirations and changes in forward-looking behaviour. We do not argue that changes in aspirations cause changes in future-oriented behaviour: we merely examine the effects of an intervention intended to affect aspirations on both sets of outcomes.

Equation (1) is the basic equation used, in which y_{i2} measures the outcome variable in the endline survey, six months after the screening, T_i is an individual-level dummy variable equal to one if the individual was invited to a documentary screening, so δ_1 is the direct effect of being invited to a documentary. P_i is an individual-level dummy variable equal to one if she was invited to a placebo screening, so ρ_1 is the effect of being invited to the placebo. η_i is an individual-level error term. We account for village fixed effects using village-level dummies, μ_v , which absorb village-level shocks, village characteristics and unobserved differences between screening sessions. Within villages, random allocation to different treatment statuses was at household level, but many outcomes are measured at individual level and observations for each spouse within households are likely not independent. For outcomes measured at individual level, we correct standard errors for clustering at household level. These estimates provide the Intention-to-Treat (ITT) effect of the intervention. Given the high rates of compliance, these effects are unlikely to differ substantially from the Average Treatment Effects on the Treated (ATT).¹¹ We use the entire sample of respondents who were given tickets, including those non-compliers who missed the screening or attended the incorrect screening.¹²

$$(1) \quad y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \mu_v + \eta_i$$

In Equation (2), we include y_{i1} , the baseline value of the dependent variable, to control for imbalances in the outcome variable at baseline, without imposing any structure on the relationship between the outcome at baseline and endline. This ANCOVA regression model is more efficient than either difference-in-difference estimators or simple post-treatment estimation when the outcome variable is measured with noise, as is likely for most of our measures (McKenzie, 2012).

were not the children of the household heads or their spouse. At endline, we asked about the number enrolled at the start of the 2010/11 school year. The age range was chosen because children are supposed to enrol in Grade 1 when they have turned 7 and it is compulsory to stay until Grade 8, when they would be about 14 or 15. We examine all households in the sample, including 107 households who have no children in this age group in both rounds, to ensure the sample is comparable with other results.

10. In Section VI, we examine the additional effects of the number of an individual’s peers who were treated, which is independent of their own treatment status. We find no evidence of interactions between the direct treatment and the number of an individual’s peers who are treated and find that estimates of direct treatment effects are the same whether or not our specification accounts for the peer treatment.

11. We do not compute the ATT as there are too few non-compliers to estimate parameters correctly. In addition, estimating the ATT assumes that treating the compliers has no effect on non-compliers. We find suggestive evidence that those in the placebo or control group whose peers are treated are affected by the intervention, so it is likely that similar spillovers will affect non-compliers.

12. We only drop the 52 individuals for whom we do not have two rounds of data.

$$(2) \quad y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \gamma y_{i1} + \mu_v + \eta_i$$

In Equation (3), we include a vector of additional controls X_{i1} , measured at baseline. We use variables that we have theoretical reason to believe might influence aspirations and other outcomes to increase precision. We include age, gender, the highest grade the respondent completed at school, whether the respondent is single (unmarried, divorced or widowed) and household wealth (captured by the total value of the household’s assets excluding their land and house), using the indicators described in Section III.D. All controls are captured at baseline. In addition, this acts as a robustness check to ensure that any baseline imbalances in correlates of aspirations do not alter results.

$$(3) \quad y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \gamma y_{i1} + X'_{i1} \pi + \mu_v + \eta_i$$

Our primary concern is estimation of δ_1 , the difference in the outcome variable between treatment and control groups. δ_1 is the effect of the intervention in total, including both the fact that a screening occurred and the content of the screening. This is the most policy-relevant parameter. Importantly, we *underestimate* δ_1 because there are no villages which are “pure-control”: the control group all live in villages where at least some people were treated, so there may be some spillovers to this group, potentially reducing the difference between treatment and control groups if spillovers are positive. We also examine $\delta_1 - \rho_1$, the difference between the treatment and placebo groups, which identifies the effect of the content of the screening. ρ_1 , the difference in the outcome between placebo and control groups, captures potential effects arising simply from the event of a screening and exposure to television in a remote area.

IV.B Treatment effect on aspirations

In Table V, we report the treatment effect of the intervention on our measures of aspirations and expectations. These variables were collected from respondents straight after the screenings (reported in panel 1) and six months after the intervention (panel 2). In all regression tables, Column 1 is the specification in Equation (1), including only the treatment and placebo dummies and village fixed effects. Column 2 is the specification in Equation (2), which adds the lagged value of the dependent variable. Column 3, the specification in Equation (3), includes the lag and vector of controls X_{i1} . There is little difference between these three specifications for most outcome variables.

The first panel of Table V reports on aspirations directly after the screening.¹³ Being invited to a documentary screening has a positive and significant effect (δ_1 in Equation (3)) of 0.12 (aspirations) and 0.11 (expectations) straight after the screening, compared to being assigned to the control group – or about 20 per cent of a standard deviation. The effect is robust in magnitude and significance to the inclusion of the lagged dependent variable and individual controls. In contrast, there is no significant difference in aspirations between those invited to a placebo session and those in the control group, indicating that the effect arises because of the content of the documentary rather than the event of the screening being held in the village (ρ_1 in Equation (3)). Aspirations and expectations among the treated group are also significantly larger than among the placebo group ($\delta_1 - \rho_1$ in Equation (3)).

The second panel of Table V shows that these effects persist after six months, although they decrease in size: aspirations and expectations among treated group are significantly higher than among the control group by 5 per cent of a standard deviation for expectations and 3 per cent of the standard deviation for aspirations.¹⁴ The last two rows of Table V show tests of whether the treatment effects are different just after screening ($t = 1$) and six months later ($t = 2$). We cannot reject that the treatment effect straight after screening is the same as the effect after six months, consistent with the finding that the effect persists over time and is not a temporary change in mood immediately

13. There may be some persistence between the first and second measures of aspirations and expectations, as there was only between four days and a week between the first and second time that participants did the aspirations questionnaire and participants may remember what they answered the first time and answer the same again. However, any persistence would be uncorrelated with treatment.

14. There is no significant difference between the treatment and placebo groups ($\delta_1 - \rho_1$) once control variables are included. As discussed, estimates of $\delta_1 - \rho_1$ after six months may be biased downward if they capture spillovers from the treatment to placebo group. We do not have a “pure” placebo group, as all villagers in the placebo group lived in villages where some people were in the treatment group.

after an inspiring screening.¹⁵ However, the difference in aspirations between treatment and placebo group narrows significantly over time, in line with the regression results in the first two panels. The difference in expectations between treatment and placebo group also narrows, but the decrease is not statistically significant.

It is striking that, six months after the screenings of four fifteen minute documentaries, we find a persistent significant effect for those who were invited to the documentaries, even if the size of this effect declines over time (including through spillover effects). To investigate how credible this is, we asked the treated and placebo individuals how they felt about the screening six months after the intervention, shown in Table VI. A high proportion of respondents like the screenings. A significantly higher proportion liked the documentary (96 per cent) than the placebo (73 per cent). The majority of respondents discussed the screening that they saw a lot with their neighbours in the time since the screening. Those who saw the documentary were more likely to discuss the screening they saw than those who saw the placebo (87 and 71 per cent respectively). Even six months after the screening, 33 per cent of the treatment group and 22 per cent of the placebo group had discussed the documentary with their neighbours in the two weeks preceding the endline survey. The treatment group were also more likely than the placebo to discuss the film they did not see (69 compared to 57 per cent), possibly indicating that the treatment sparked greater interest in the whole intervention in the village than the placebo did. But, nonetheless, 57 per cent of those in the placebo group had discussed the documentary, the film that they did not see, with their neighbours. This suggests spillover effects are possible. We discuss these further in Section VI.

In Table VII, we investigate the direct and indirect effect of treatment on the components of the aspiration index. Each column reports a separate estimate of Equation (3) for income, wealth, education and social status aspirations, respectively. Results show strong and positive direct effects on aspirations for children’s education, and no such effects on other dimensions.

This effect on parents’ aspirations for children education is particularly plausible in the Ethiopian context. In recent years, primary schooling in Ethiopia has become relatively accessible to most families as a means to improve their future opportunities. Access to education has dramatically improved: in 1992, nearly four out of five primary school age children were not in school; by 2009, this was below one in five (Engel, 2010, 7). Even in this remote and relatively hilly district, households in our sample were now on average only 25 minutes walk from the nearest primary school. Since 1995/6, costs of education have decreased considerably: no fees are charged for the first eight years of primary school, and textbooks are often, although not always, provided by the school. This has reduced barriers to enrolment, although parents still need to pay for stationary and uniforms (Orkin, 2012). High educational aspirations have also been found in other surveys in rural Ethiopia. For example, the Young Lives survey found parental aspirations for their children in poor communities across Ethiopia to be even higher than in this sample, with caregivers aspiring to 14.21 years of education for their children for the cohort of children aged 8 in 2009 (Dercon and Singh, 2013).

These results are striking, as none of the four documentaries featured a character with significant formal education and the subjects of the documentaries did not mention literacy or education in explaining their success. The results thus suggest that the audience is not merely responding to specific information in the documentaries about how the featured individuals became successful and taking the same actions taken by these individuals. Rather, the results suggest a deeper change in individuals’ perceptions of their future opportunities.

Lastly, we explore whether treatment effects on aspirations differ by various characteristics: whether the respondent had above median aspirations at baseline, whether the respondent is above median age, the respondent’s gender, the highest grade they have completed and whether their household has above the median asset level. In Equation (4), θ_2 , the coefficient on the interaction of the treatment effect and each characteristic captures whether the treatment effect increases, decreases or is constant with the characteristic.¹⁶

$$(4) \quad y_{i2} = \alpha + \delta_1 T_i + \theta_1 Z_i + \theta_2 T_i * Z_i + \mu_v + X'_{i1} \pi + \eta_i$$

Results are presented in Table VIII; we show only the results for aspirations. We note that the aspirations boost from the experiment is only for those with above median aspirations. For these individuals, the effect on aspirations

15. We also find no significant differences in the means of aspirations and expectations among the control group at baseline, directly after the screening and six months after the screening, suggesting our measures are stable over time.

16. Given that we have found no significant effects of the placebo on aspirations in earlier regressions, we pool the placebo and control group together to increase power.

is large and significant both straight after screening (0.28 standard deviations) and after six months (0.06 standard deviations). The effect depreciates significantly, but remains significant after six months. Those with lower initial aspirations to start with (in an overall deprived setting) are not affected by the screenings.¹⁷ We find no other significant heterogeneous treatment effects by age, gender or level of education in this sample, although we are not powered to detect small differences between groups.

IV.C Treatment effects on future-oriented behaviour

We then investigate how the treatment has affected individuals' behaviour in the following six months. First, the documentaries emphasised that the subjects had worked hard to make progress, so we might expect an increase in hours in work among the treatment group. We have data on work and on leisure, but we find no evidence of an effect of watching the documentary or placebo on time allocation (results are shown in Table IX). Second, Table X reports on the impact on savings and actual credit. We check the effect on whether the individual has any cash savings, the amount of savings, whether they took out a loan of more than 15 ETB in the last 12 months and the total credit in this period. We find some striking impact on these variables from the experiment and not from the placebo. We find some effect on the probability of holding savings, but this is not robust to introducing whether the individual had savings at baseline and the village fixed effects. Among treated individuals, the stock of savings is on average 97 ETB higher than among the control group, controlling for the stock of savings at baseline, a difference which is significant at the 10 per cent level. Treated individuals also have 88 ETB more savings than the placebo group, also significant at the 10 per cent level.

In the second panel of Table X, we examine credit behaviour. We find that the treatment group took out 21 ETB more in credit than the control group (a difference which is significant at the 10 per cent level), and significant differences with the placebo on whether credit is taken (with treatment group 5 percentage points more likely to take out a loan). They were also three percentage points more likely than the control group to take out a loan, but the difference is not statistically significant ($p=0.17$).

We also examine hypothetical demand for credit by asking household heads how much they would borrow if given the opportunity. Results for each hypothetical loan maturity are shown in Table XI. We find positive significant effects of the intervention on the amounts treatment individuals would ask for in ten years compared to both the placebo and the control group. There is no significant effect on individuals who saw the placebo. Overall, results are consistent across the savings and credit variables, pointing to a direct effect of treatment on the use of financial instruments (whether savings or credit) for the treatment group and no effects for the placebo and control groups.

Finally, we examine effects on children's enrolment in school and the amount spent on their schooling. While there was nothing in the documentaries emphasising the role of education in the progress of the people featured, we knew from previous work that education aspirations were high in Ethiopia (Bernard and Taffesse, 2014), so this seemed a plausible avenue in which people might choose to invest.

In the first three columns of Table XII, we investigate whether our intervention affects the number of children in the household between the ages of 6 and 15 who are enrolled in school. We find significant positive effects on enrolment: the number of children between 6 and 15 who are enrolled increases by 0.21, a 21 per cent increase from the baseline average of 1.02 children enrolled in school across all groups and 17 per cent larger than the control group average of 1.23 at the time of the endline survey.¹⁸ There is no significant effect on the placebo group, but the difference between treatment and placebo groups is not significant ($p=0.11$).

In the second three columns of Table XII, we examine spending on schooling for children of all ages still living in the household. Treated households spend 33 ETB more on school expenses than the control group did in the same period (17 per cent more), a difference which is significant at the 10% level. There is no significant effect on the placebo group, but the difference between the treatment and placebo groups is not significant.¹⁹

17. This result is in line with recent theoretical predictions by Dalton et al. (2014).

18. These effects may underestimate the effect of the intervention on enrolment. The school year starts on September 1st, but households are usually able to enrol their children in school until the end of October. Households were treated between August and October 2010, so some households may have received the treatment after the last point when they could have enrolled their children in school, so they couldn't enrol children in response to the treatment even if they had wanted to.

19. Again, there is some potential for measurement error. Households were asked about their expenditure between September and December 2009 at baseline and the same period in 2010 at endline. So some households had limited time to increase school expenditure after the intervention if they wanted to do so.

Overall, despite a relatively soft intervention – a one-hour documentary screening – we find clear evidence of behavioural changes six months after treatment. These results are also in line with our analysis of which components of the aspirations index are affected by treatment: the documentaries have the strongest effects on aspirations for children’s education. Although there is nothing related to education in the screened documentaries, we find that the documentaries have strong effects in relation to children’s education, both through changes in aspirations and changes in investment in education. Our behavioural measures examine investments in the education of all children in the household, while our aspirations measure asks specifically about parents’ aspirations for their eldest child’s education. However, it is likely that an intervention that affects parents’ aspirations for the eldest child will also improve aspirations for the education of other children.

V ROBUSTNESS AND FURTHER INTERPRETATION

The experiment can identify a link between watching the documentary and changes in aspirations, and between watching the documentary and changes in future-oriented behaviour. Through the use of a placebo, we can also demonstrate that the effects arise because of the content of the documentary, separate from the effect of a screening and gathering in a remote rural area. Because of the household-level randomisation, we know that it is unlikely that any other opportunities or constraints changed solely for treated households in the six months between baseline and follow-up. We also know that the change in behaviour did not occur because the treatment group received specific relevant information on untapped opportunities, because the documentaries did not contain such information. However, this does not establish that aspirations are the main or even a relevant mechanism by which changes occur. Other, possibly more important, changes may have occurred in the mindset of participants and led to changes in their behaviour. In this section, we explore the effects of the intervention on traits and psychosocial outcomes that may support or contradict the plausibility of our argument that changes in aspirations are a relevant explanation for changes in behaviour. We also explore statistical robustness; in particular whether statistical corrections for testing for multiple outcomes alters our main conclusions.

V.A *Treatment effect on time and risk preferences*

The documentaries depict individuals who are goal-oriented, plan and focus on the future. Watching the documentaries may have highlighted the value of these behaviours and caused respondents to think more about the future or to be more patient. Similarly, documentary subjects sometimes took actions that the audience might have perceived as risky, such as investing in new technologies or farming methods or diversifying their economic activities, and watching the documentaries may have encouraged respondents to be less risk-averse. We examine whether the intervention had any effects on discount rates and risk preferences. A change in time or risk preferences is not clearly conceptually related to a change in aspirations. If the treatment affects both these preferences and aspirations, we cannot argue that there is one likely psychological mechanism behind the changes in behaviour we observe in response to the treatment.

We use survey-based measures, as in Cole et al. (2013) in India and Hill et al. (2013) in Ethiopia, described in Appendix B. For time preference, the outcome variable is the subjective discount factor $\beta = \frac{1}{1+\delta}$, where δ is the rate of time preference. In other words, the subjective discount factor is the value to a person today of 1 ETB received in future. Appendix Table A.III shows that the mean subjective discount factor is 0.54 and there are no differences between groups at baseline.

To measure risk preferences, we use Binswanger-style lottery choices (Binswanger, 1980) which have been used in Ethiopia before (Hill et al., 2013). Individuals were asked which of five hypothetical payouts they would choose if the payout was determined by a coin toss, and then, separately, about the amount of price risk they would choose when selling surplus grain output. The two questions had the same payout structure, in which the lotteries offered increased in both mean and variance (so payouts were ordered from most to least risk averse), but the payouts in the grain sale measure were multiplied by 100. Appendix Table A.III shows that the mean coefficient of partial risk aversion at baseline is 0.99 on the coin measure and 1.22 on the grain sale measure. Respondents are significantly more risk averse when answering the question about maize prices fetched at market ($p=0.000$), as the payouts are larger on the maize question and higher stakes are often associated with more risk-averse behaviour (Holt and Laury, 2002). There is no systematic difference between groups at baseline, except that the coefficient of partial risk aversion using the

coin measure is higher in the treatment than control group at the 10 per cent level.

In Table XIII, we find no treatment or placebo effects on the respondents' discount rate or on the coefficient of risk aversion. Changes in savings, investment in children's education and other future-oriented behaviours cannot be explained by treated individuals becoming more patient or changing their risk preferences.

V.B Treatment effect on locus of control and perceptions of causes of poverty

There is a long tradition in psychology and sociology of measuring people's perception of their ability to shape their lives and future. From social psychology, we focus on the concept of locus of control. Locus of control is "a generalised expectancy pertaining to the connection between personal characteristics and/or actions and experienced outcomes" (Lefcourt, 1991, 414). We use the Internality, Powerful Others and Chance (IPC) scale (Levenson, 1981), which captures three independent components of the construct of control, building on the measure by Rotter et al. (1972) commonly used in economics, for example by Heckman et al. (2006) and Heckman and Kautz (2012).²⁰ The Internality scale captures if people see outcomes as contingent on individual behaviour; the Chance scale captures whether individuals think chance, luck or fate affects their outcomes; while the Powerful Others scale examines beliefs about whether other people control events in their lives. Appendix B describes the instruments.

Some psychologists suggest that locus of control, together with self-esteem, self-efficacy (the belief that one can act effectively to achieve desired results), and aspects of emotional stability (as captured in the Big Five personality measure), are indicators of a common construct, called "core self-evaluation" (Judge et al., 1998, 2002a). A positive core-self evaluation indicates a proactive and positive perception of oneself and one's ability to shape one's interactions with the world. There is debate about whether these are separate concepts or indicators of a common construct (Johnson et al., 2008), but these concepts are clearly strongly correlated.

There is also extensive evidence that these concepts are strongly correlated with aspirations. Among children, there are strong correlations between children's educational and occupational aspirations (using measures similar to those in this paper), their self-efficacy and locus of control, and subsequent educational and occupational outcomes, in both psychology (Lent and Hackett, 1987; Holland and Gottfredson, 1975; Bandura et al., 2001) and economics (Wysock et al., 2013). Among adults, there is evidence suggesting that adults with higher positive core self-evaluations (including an internal locus of control) are more likely to pursue the attainment of aspirations (Shah and Higgins, 2001) and that self-efficacy promotes higher aspirations (Bandura, 1997, 2001). More broadly, there is extensive evidence of a link between more active setting and pursuit of valued goals and, respectively, an internal locus of control (Strickland, 1965; Levenson, 1974), higher self-efficacy (Locke and Latham, 1990) and higher positive core self-evaluation (Elliot et al., 1997). Parents with higher self-efficacy have higher aspirations for their children (Bandura, 2001). Other work suggests that interventions like ours, which provide role models with whom the person can identify, are effective at improving self-efficacy (Bandura, 1997; White and Locke, 2000), suggesting that our intervention may affect core self-evaluations.

From sociology and political science, we use the Attributions for Poverty scale (Feagin, 1972, 1975) to measure people's perceptions of the causes of poverty among people in general, rather than only in their own lives. We use a version adapted for China (Shek, 2003) (a shorter but less accurate version is included in the World Values Survey (Abramson and Inglehart, 1995)). The scale assesses the extent to which respondents agree with each of three types – individualistic, structural and fatalistic – of explanations for poverty. These echoes the scales used in the IPC scale.

Appendix Table A.II shows correlations between these variables and our measures of aspirations. As expected, there are significant positive correlations between higher aspirations, having an internal locus of control, and agreeing with the idea that individual behaviour results in poverty. Aspirations are also negatively and significantly correlated with attributing poverty to structural factors or to fate, and negatively (but not significantly) correlated with attributing life outcomes to chance or powerful others in the locus of control scale.

Table XIV gives treatment effects on these measures. For locus of control, those who saw the documentary score 0.28 points higher than the control group (a 1.8 per cent increase, significant at the 5 per cent level, from the baseline mean) and 0.36 points higher than the placebo group (a 2.3 per cent increase, significant at the 5 per cent level, from the

20. Early scales differentiated people with an external locus of control, who believe that outcomes are not determined by personal efforts, from those with an internal locus of control who believe that outcomes are contingent upon their own actions (Rotter et al., 1972). However, subsequent empirical research found that beliefs about control are multidimensional and not captured well by this single dimension (Lefcourt, 1991).

baseline mean) on the Internality scale, which captures if people see outcomes as contingent on individual behaviour. There are no significant differences in the Chance or Powerful Others scales, but this is plausible as theory suggests links only between aspirations and the internal locus of control (Levenson, 1974; Strickland, 1965). The documentaries clearly focused on one’s own behaviour playing a key role in outcomes, although they do not necessarily remove the role for some luck or support by others.

On the causes of poverty measure, those who saw the documentary are significantly less likely than the placebo and control group to agree with fatalistic explanations that attribute poverty to luck and fate. They are significantly more likely than the control group to agree with items that offer individualistic explanations and place responsibility for poverty on the behaviour of poor people. However, the placebo also had a positive effect on the Individual scale. The treatment has no significant effect on respondents’ agreement with structural explanations, which attribute poverty to societal and economic forces. This is to be expected as the documentaries focussed on individuals rather than their environments or the social and economic forces affecting them.

These results provide strong support for our findings that the documentary improved aspirations. These scales are entirely separately administered, but we would expect that people’s perceptions of their ability to affect their own lives they would be correlated with aspirations. The fact that the measures behave in the same way indicates that our intervention is altering these underlying related constructs and is not simply an artefact of the measure of aspirations we use.

V.C Treatment effect on life satisfaction

Finally, we consider subjective wellbeing. Subjective well-being measures can capture two related concepts. Life evaluation captures individuals’ perspectives on their lives, while emotional well-being, hedonic well-being or daily affect capture emotions at a point in time (Diener et al., 2003; Kahneman and Deaton, 2010). As our intervention alters locus of control and aspirations, it might be expected to alter life satisfaction. Locus of control is an important predictor of life satisfaction (Judge et al., 2002a,b). Furthermore, individuals with an internal locus of control are more likely to set and pursue goals or aspirations, and evidence suggests doing so makes them happier and more satisfied Elliot et al. (1997).

We measure life satisfaction 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 question was repeated with the top and bottom of the ladder representing the happiest and most miserable possible life.²¹ As predicted by theory, we also find strong positive correlations between aspirations and expectations and wellbeing and between wellbeing and an internal locus of control, shown in Appendix Table A.II.

We report each measure separately in Table XIV. We find no significant effects of the treatment on the measure of well-being asking about the best life but find a significant positive difference of 0.22 between the treatment group and both the placebo and control groups on where participants place themselves on a ladder measuring happiness (the average score on this measure at baseline was 6.59). The measure may capture differences in personality traits, specifically the propensity to be satisfied with life (Schimmack et al., 2002), but we control for the person’s answer at baseline so we only capture changes since the intervention.

V.D Corrections for multiple testing

As a final robustness check we implement a test for multiple outcomes. Although we did not produce a formal pre-analysis plan, this study’s primary outcome, the measures of aspirations, was defined and tested in a pilot study (Bernard and Taffesse, 2014) and prioritised for measurement immediately after the screening (other variables were only collected at baseline and endline). We have also examined two groups of further outcome variables. First, we have tested for changes in behaviour for six basic outcomes: savings, credit, time allocation, spending on children’s education, number of children enrolled in school and hypothetical demand for credit. Second, we have assessed the

21. The Cantril ladder refers only to the best and worst life. We used measures from the European Social Survey, which relate to both happiness and life satisfaction. While life satisfaction has a mean on average 0.4 points higher, the same variables explain happiness and life satisfaction and country rankings are similar (Helliwell et al., 2010).

effect of our intervention on psychosocial measures which are conceptually related to aspirations as a robustness check for our aspirations results.

Testing for multiple outcomes increases the probability of false discoveries. In Table XV, we assess whether results are robust to controlling for the false discovery rate. We rely on the Benjamini et al. (2006) two stage procedure within families of outcomes. Anderson (2008) argues that this procedure is less conservative than familywise error rate control procedures, which assess the probability that at least one false discovery is made.²² We also do not use summary index tests. Summary index tests are statistically more powerful and suitable for assessing the overall impact of an intervention but give little consideration to each particular outcome. We aim to understand the intervention’s impact on separate groups of outcomes, as results for different outcomes have different theoretical implications.

We use the Benjamini et al. (2006) procedure within seven families of outcome variables, namely (i) aspirations and expectations, (ii) time allocation, (iii) financial outcomes, (iv) hypothetical demand for credit, (v) children’s education outcomes, (vi) other psychological measures and (vii) time and risk preferences. For each outcome variable, Table XV reports the naive p-value from running each estimate independently already discussed in earlier sections of the paper and the q-value that accounts for multiple testing within each family. Results based on q-values are qualitatively similar to those from naive p-values. In particular, effects on aspirations and expectations, as well as on whether people believe that poverty is determined by individuals, remain positive and significant after controlling for the false discovery rate. Results related to children’s enrolment and spending on children’s education remain strongly significant. Inference regarding financial outcomes is no longer statistically significant ($p=0.163$), nor is the effect on whether individuals have an internal locus of control ($p=0.13$). Overall, our results are reasonably robust to the possibility of false discoveries.

VI PEER EFFECTS

VI.A *Design and measures*

Finally, we test for the presence of treatment spillovers within village communities. As discussed in Section II, people’s aspirations may be influenced by the aspirations of their peers. In addition to direct effects on treated individuals, our intervention might have second order effects, where individuals’ aspirations and behaviour are affected by changes in the aspirations and behaviour of treated individuals. If peer effects on individuals in the placebo and control groups are positive, the treatment effects identified previously represent lower bounds of the true treatment effects. However, if peer effects also contribute to positive changes within the treatment group, the estimated parameters are the upper bound of the “treatment on the uniquely treated”: the effect of having been exposed to documentary screening absent any peer-related effects (Baird et al., 2012).

It is difficult to identify how an individual’s peer network affects their outcomes, just as it is difficult to identify the causal effect of any behaviour of a group on the outcomes of a group member. As Manski (1993) highlights, the direction of causation between an individual’s outcomes and the peer group they are part of may be blurred by sorting effects (where individuals form groups of similar peers with similar outcomes), correlated or contextual effects (where behaviour is driven by the exogenous characteristics of others in the group, or where all members of the groups are subject to the same shocks), or reflection biases (where one cannot distinguish the effect of group-member interactions from the mere summation of individual behaviour).

Identification of endogenous interaction effects, where individuals are influenced by the actions of their peers, has recently mostly relied either on exogenous variations in group composition (e.g. Yang (2007), Kling et al. (2007) or Sacerdote (2001)), or on partial population interventions that directly affect some peers within a group, but not others (e.g. Duflo and Saez (2003) and Bobonis et al. (2006)). Our approach is akin to the latter. In particular, we rely on village-level variations in treatment intensities described in Section III.C, where an additional 18 households (36 individuals) per village were invited to documentaries in half of the villages of our sample. In the other villages, an additional 18 households (36 individuals) per village were invited to placebo screenings.

Two caveats are in order. First, our experimental design does not include a pure control group of villages where no individuals were exposed to treatment. Within budget constraints, we chose to maximise statistical power on direct

22. More conservative familywise error rate control procedures tend to be used when a false discovery has large consequences, such as for policy. However, this study is more exploratory, so we use the slightly less conservative procedure.

treatment effects, which we expected to be low, at the cost of being able to identify the full range of peer effects parameters described in Baird et al. (2012). Thus, our current design is merely sufficient to test for the likely presence of peer effects, but not to provide an accurate measure of these effects.

Second, we rely on two levels of treatment intensities: six households (12 individuals) were targeted in low-intensity villages, while twenty-four households (48 individuals) were targeted in high-intensity villages. The average village size was roughly 75 households.²³ If one assumes linearity in peer effects, such that each additional treated individual contributes to spillovers in a village, this variation may suffice to identify the presence of peer effects. In the presence of threshold effects, however, is it unclear whether these village-level variations in treatment intensities lie on either side of a given threshold.

We partially account for these limitations by capturing variation in the exposure of a respondent’s own network to treatment due to both the individual and the village-level treatment. We asked each surveyed individual to list their four closest friends at baseline. We matched these lists to the lists of invitees to treatment or placebo screening sessions to capture how many of a respondent’s friends were invited to the documentary and placebo. We only asked about four peers to avoid potential biases related to the size of one’s social network. 99 per cent of respondents cited exactly four peers. Among the respondents who cited four peers, 93 per cent cited four individuals who lived in the same village, as would be expected given the remoteness of these communities. Only 14 per cent of the respondents listed any siblings among the four individuals, suggesting that any peer effect cannot be fully explained by family characteristics.

With imperfect correlation between one’s village and one’s social network, this design generates an almost continuous distribution of network-level intensity of treatment (Baird et al., 2012). Table XVI presents the distribution of individual-level peer treatment in high-intensity and low intensity villages. As expected, more of the respondent’s peers had seen the documentary in intense-treatment villages and more of the respondent’s peers had seen the placebo in intense-placebo villages. However, it is also possible that a person in an intense-placebo village may have four peers who are treated, or that a person in an intense-treatment village may have none. Thus, while partially correlated with village-level treatment intensity, this measure offers further individual-level variation. It is only partial however, in that it only captures four peers. Individuals may talk to many more people directly or indirectly exposed to the documentary, something better accounted for by the village-level treatment intensity variable.

VI.B Specification

We test for the presence of peer effects based on modified versions of Equation (3). Specifically, we capture village-level treatment intensities with a dummy variable I_v , equal to one for villages where an additional 36 individuals were invited to a documentary session and zero otherwise. As treatment is at the village-level, we cannot use village fixed effects as in previous specifications. Instead, we use screening site-level fixed effects μ_s to account for site-specific characteristics.

$$(5) \quad y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \lambda I_v + \gamma y_{i1} + \mu_s + X'_{i1} \pi + \eta_i$$

With a similar specification, we assess the effect of treatment intensities in individuals’ networks of peers. n_i^T measures the number out of four of the respondent’s four closest friends who were invited to a documentary session, while n_i^P measures the number of peers invited to a placebo screening. The probability of having a given number of peers treated is in part determined by village-level treatment intensities, as is clear from Table XVI. Thus we account for non-independence of observations within village by clustering all standard errors at the village-level.²⁴ Our parameters of interest are labelled as δ_2 , the effect of each additional peer being treated on an individual’s outcome, irrespective of his or her own treatment status, and the corresponding parameter for the number of peers invited to a placebo screening, ρ_2 .

23. We only sampled villages with 50-100 households to minimise effects of village size.

24. We do not use the usual Liang and Zeger (1986) clustered standard errors as these can be unreliable if there are fewer than about 100 clusters and we have 64 villages. We therefore base inference on a t distribution with $g - k$ degrees of freedom, where g is the number of groups, rather than on the standard normal distribution (Cameron et al., 2008; Donald and Lang, 2007).

The direct individual treatment and the variation in treatment intensities at village level are independent by construction. However, individuals within the same peer group may simply react homogeneously to treatment without ever discussing the documentary or observing the ways in which their peers' behaviour changes after watching the documentary, and this may be confused for a peer effect. In other words, δ_2 may in part result from "sorting effects" (in the terminology of Manski (1993)). If true, one should observe such effects on individuals that are in the treatment group themselves. Thus, we expand Equation (5) to include interactive terms between one's treatment status and the number of his/her peers in the treatment group, and estimate the corresponding parameter δ_3 .²⁵ If equal to zero, these parameters indicate limited sorting effects, as well as limited additional effect of peers' treatment (placebo) on individuals in the treatment (placebo) group.

$$(6) \quad y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \delta_2 n_i^T + \rho_2 n_i^P + \delta_3 T_i * n_i^T + \rho_3 P_i * n_i^P + \gamma y_{i1} + \mu_s + X'_{i1} \pi + \eta_i$$

VI.C Results

We group all peer-related results into Table XVII and present results from Equation (5) and (6) in Panels A and B respectively. We find no evidence of peer effects on respondents' aspirations. There is consistent evidence that households in high-intensity villages increased the time they dedicated to income-generating activities and decreased leisure time. Although non-statistically significant, point estimates of parameters δ_2 and ρ_2 for these outcome variables in Panel B give further support to the village-level findings. There is no clear evidence of peer effects on financial indicators related to savings and credit.

However, we find increased spending on children's education for those households where a larger share of the household head's peer network was invited to screening. No such effect is found in response to increased number of peers invited to a placebo screening. In this estimation, attending the documentary increases spending on children's education by 53 ETB. For each additional peer who attends the screening, spending on children's education increases by 32 ETB. This is a large peer effect – more than half the individual treatment effect. There is, however, no evidence that the number of peers treated affects the number of children enrolled in school. The last line of the table, which reports parameter δ_3 , shows we find no evidence of sorting effects to which these results may be attributed. Overall, while partially identified, the results provide support to peers' influence on behavioural outcomes related to children's education.

These last results resonate with other experimental evidence in developing county settings. Children whose peers are more likely to be attending school are also more likely to attend school. In the PROGRESA programme in Mexico for instance, poor families in randomly selected villages were eligible to receive a cash grant for each child who attended school more than 85 per cent of school days. Attendance increased among eligible children at primary school by 5.8 percentage points from a base of 78 per cent, but also increased among children from families above the poverty cutoffs for program eligibility, by 2.1 percentage points from a base of 78 per cent (Lalive and Cattaneo, 2009). Using a slightly different identification strategy, Bobonis and Finan (2009) find secondary school attendance increased by 5 percentage points from a base of 68 per cent.

Neither paper explores peer effects in the networks of parents, which may be different from the networks of their children. Bobonis and Finan (2009) examine differences in attendance between eligible and non-eligible children in treated communities, so peer effects could arise through children's or parents' networks. Lalive and Cattaneo (2009) focus more specifically on children's networks by comparing eligible and non-eligible children at the same grade level who are usually in the same classroom. But it is likely that peer effects would also occur among parents: parents may benefit from information externalities about the benefits of spending on children's education which they learn from their peers, or they may be influenced by a desire to conform to the behaviour of other parents (Bernheim, 1994; Bikhchandani et al., 1992). Our results support this hypothesis.

25. Similarly, ρ_2 may result from sorting effects, so we estimate the interaction of an individual's placebo status and the number of peers in the placebo group, ρ_3 .

VII CONCLUSION

We randomly exposed individuals to a one-hour documentary in which four people from similar backgrounds to the audience tell their life story of getting out of deep poverty. We find significant improvements in the aspirations of treated individuals, measured both straight after the screening and six months later. Treatment effects on average are driven by effects among individuals with above-median baseline aspirations, but we find no heterogeneity by gender, age, baseline wealth or level of education. Although the success of the documentary subjects was not related to education, effects on aspirations are largely driven by individuals' revision of their aspirations for their children's education. Results are robust to corrections for multiple testing.

We also assess the reduced-form effect of our intervention on individuals' behaviour. We find consistent evidence that being invited to a documentary screening directly affected individuals' use of financial tools: treated individuals increase their total savings, total amount taken in credit and whether they used credit. We also find a positive treatment effect on a hypothetical demand for credit question. However, results are not robust to correction for multiple testing. We find no direct effects on individuals' time allocation to work or leisure.

Finally, we find strong effects on the number of children in the household aged 6-15 enrolled in school and in total spending on children's education. Results are robust to multiple testing corrections. Exposure to the documentary has changed both parents' educational aspirations and their immediate investment in children's education. Results highlight that improvements in aspirations and changes in behaviour that may be related to changes in aspirations can have inter-generational effects. Many of parents' future-oriented decisions are about investments in their children's human capital. So changes in individuals' perceptions of their future opportunities could be as, if not more, likely to affect the next generation, rather than those whose aspirations shift.

We can identify a link between watching the documentary and changes in aspirations, and between watching the documentary and changes in future-oriented behaviour. The randomised allocation to treatment means it is unlikely that any other opportunities or constraints changed solely for treated households in the six months between baseline and follow-up. Results are robust to a symmetrical placebo experiment, so effects arise because of the content of the documentary, separate from the effect of a screening and gathering in a remote rural area. The documentaries also did not contain specific information on untapped opportunities, so effects are not driven by access to new information.

We cannot conclusively establish that aspirations are the main psychological mechanism behind changes in behaviour, although we find strong treatment effects on aspirations. However, we show it is unlikely that the mechanism behind the effect on behaviour is simply that individuals focus more on the future or change their risk preferences, because we find no effects on time discounting or risk aversion.

We also find treatment effects on psychological measures of people's perception of whether they are able to shape their lives and future, namely locus of control from psychology and perceptions of the causes of poverty from sociology. Our intervention increases the extent to which individuals think their outcomes are contingent on their own actions and decreases the extent to which they think that poverty is caused by fate or chance. Psychologists have found extensive evidence of correlation between aspirations and an internal locus of control, so we would expect that our intervention, which alters aspirations, would affect locus of control. The fact that we find results in the expected direction supports our conclusion that the intervention is altering these underlying related constructs and that the changes in aspirations that we see are not simply an artefact of the measure of aspirations we use. The results, although small in magnitude, are also encouraging in themselves, as an internal locus of control is associated with a range of positive outcomes.

We find some evidence that effects on aspirations are in part mediated through the number of peers also exposed to treatment. These results give support to the hypothesis set forth by Appadurai (2004) and Ray (2006) that aspirations, although an individual attribute, respond to collective influence. As a side contribution, our results confirm that video-based interventions can be effective in altering perceptions and behaviours (see Berg and Zia (2013) on financial education and financial behaviour in South Africa; Jensen and Oster (2009) on female autonomy in India; Paluck (2009) on a radio program towards inter-group tolerance in Rwanda).

Are we giving false hope? We cannot judge this. But 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 their peers from similar backgrounds. The extent and nature of their response has surprised us. Our research suggests further research on the psychological effects of poverty and on the role interventions to reduce these effects may play in reducing poverty.

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Table I: SAMPLE OF INDIVIDUALS SURVEYED AND GIVEN TICKETS

	All villages	Intense treatment villages	Intense placebo villages
# individuals surveyed	64 2,063	32 1,034	32 1,029
of which:			
Treatment individuals	675	337	338
Placebo individuals	702	354	348
Control individuals	686	343	343

This table reports on the sample of individuals (n=2,063) used in all subsequent analysis, which excludes 52 individuals who were not surveyed in Round 2 or were missing a questionnaire. In each of the 64 villages, 18 households were randomly selected and randomly allocated to one of the treatment, placebo or control groups. The 64 villages were grouped into groups of four to make 16 screening sites. In each screening site of four villages, we randomly assigned two villages to be "intense treatment" villages and the other two to be "intense placebo" villages. In "intense treatment" villages, 18 additional households (roughly 36 individuals) were randomly invited to watch documentaries; in "intense placebo" villages, the 18 additional households were invited to the placebo screening. The additional households received invitations but were not surveyed. Their compliance with treatment is reported in Table II.

Table II: COMPLIANCE AND ATTRITION OF SAMPLED INDIVIDUALS

	Surveyed				Not surveyed ¹		
	Treated	Placebo	Control	Total	Treated	Placebo	Total
Total sample	769	768	767	2,304	1,154	1,151	2,305
Household head's spouse dead/divorced	38	27	30	95	56	62	118
Not given ticket (sick or away)	41	24	30	95	8	3	11
Given tickets	691	717	707	2,115	1,090	1,086	2,176
Non-compliers Missed screening	15	9	0	24	43	28	71
Non-compliers Attended incorrect screening	3	11	6	20	0	0	0
Non-compliers Not recorded at correct screening ²	11	10	25	46	45	62	107
Compliers Recorded at correct screening ³	662	688	673	2,023	1,002	996	1,998
Compliance rate ⁴	0.958	0.960	0.952	0.957	0.919	0.917	0.918
Missed Round 2	15	15	19	48	N/A	N/A	
Missing q'naire	1	0	3	4	N/A	N/A	0
Attrition rate ⁵	0.022	0.021	0.027	0.023			
Sample ⁶	675	702	685	2,063			

¹ These columns describe the additional households who received invitations to screenings but were not surveyed. We recorded which invitation they received and whether they arrived at the designated screening session.

² Some individuals were not recorded as arriving at the correct screening, but were also not recorded as absent or attending the wrong screening.

³ For treatment and placebo groups, these are individuals who were recorded arriving at the correct screening. For control groups, these are individuals who completed a follow-up interview at their home during the screenings for their village.

⁴ The compliance rate is the proportion of those given tickets who were recorded arriving at the correct screening.

⁵ The attrition rate is the proportion of those surveyed in Round 1 and given tickets who were not found in Round 2.

⁶ The sample in this paper consists of individuals given tickets minus those who could not be found in Round 2.

Table III: EXPERIMENTAL INTEGRITY: BALANCE TESTS ON VARIABLES AT BASELINE

	Mean	Mean for controls	T-C difference	P-C difference	N
Individual-level variables					
Age	36.82	36.76	0.36 (0.82)	0.37 (0.88)	2,063
Male	0.49	0.49	0.00 (0.01)	-0.01 (0.01)	2,063
Highest grade attained	1.34	1.26	0.25* (0.14)	0.05 (0.13)	2,063
Single	0.06	0.05	0.03** (0.01)	0.00 (0.01)	2,063
Watches TV > monthly	0.10	0.10	0.00 (0.02)	-0.01 (0.02)	2,059
Listens to radio > monthly	0.61	0.61	-0.01 (0.03)	-0.03 (0.03)	2,059
Travels outside district > monthly	0.14	0.13	0.01 (0.02)	-0.02 (0.02)	2,049
Has lived outside district	0.10	0.09	0.01 (0.02)	-0.02 (0.02)	2,028
Household-level variables					
Food insecure and hunger	0.59	0.60	-0.01 (0.04)	0.03 (0.04)	1,127
Food insecure but no hunger	0.17	0.18	-0.03 (0.03)	-0.04 (0.03)	1,127
Food secure	0.23	0.22	0.04 (0.03)	0.01 (0.03)	1,127
Total value of household assets (ETB)	6,567.63	6,205.69	696.22 (617.9)	-780.08 (617.49)	1,138

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors (clustered at household level for individual outcomes) are in parenthesis. We find no differences between groups in the distribution of households across the three categories of food insecurity using a chi-squared test (p=0.256 for the difference between treatment and control and p=0.308 for the difference between placebo and control).

Table IV: ASPIRATIONS, EXPECTATIONS AND FUTURE-ORIENTED BEHAVIOUR AT BASELINE

	Mean	Mean for controls	T-C difference	P-C difference	N
Aspirations					
Income	146,057.00	141,822.25	25,871.88 (33,262.89)	25,495.87 (33,220.02)	2,047
Wealth	152,577.10	197,835.62	-130,000.00 (118,793.91)	15,587.48 (186,069.31)	2,049
Children's education	12.91	12.88	0.09 (0.11)	0.03 (0.10)	2,015
Social status	75.01	75.64	-1.87 (2.13)	0.14 (2.09)	2,039
Index	0.03	0.03	0.00 (0.03)	0.02 (0.04)	2,058
Expectations					
Income	33,081.82	35,936.55	-869.56 (3,528.42)	15,525.44 (14,864.51)	2,045
Wealth	28,073.07	28,329.12	207.59 (3,897.11)	1,956.23 (4,753.79)	2,049
Children's education	12.43	12.37	0.14 (0.17)	-0.08 (0.17)	1,936
Social status	70.95	71.40	-1.20 (1.98)	0.35 (1.89)	2,040
Index	0.02	0.03	0.00 (0.03)	0.01 (0.04)	2,055
Weights for dimensions of life					
Income	5.51	5.47	0.06 (0.10)	-0.12 (0.09)	2,063
Wealth	4.83	4.85	0.01 (0.08)	0.14 (0.08)	2,063
Children's education	4.38	4.43	-0.12 (0.09)	0.09 (0.08)	2,063
Social status	5.05	5.06	-0.03 (0.11)	0.02 (0.10)	2,063
Variables at individual level					
Average daily time in work (hours)	348.72	347.34	6.41 (8.71)	4.31 (9.08)	1,961
Average daily time in leisure (hours)	740.14	742.33	-14.26 (10.49)	-14.93 (10.96)	1,961
Has any cash savings	0.22	0.21	0.04 (0.02)	0.00 (0.02)	2,063
Total savings (ETB)	82.97	71.91	35.15 (38.23)	2.60 (26.48)	2,057
Has taken out credit in last year	0.43	0.43	0.04 (0.03)	0.05 (0.03)	2,063
Amount of credit (ETB)	176.17	170.03	30.02 (19.05)	22.25 (18.34)	2,063
Variables at household level					
Hypothetical loan repayable in 1 year (ETB)	5,867.38	5,959.42	-680.56 (536.32)	-813.19 (537.03)	1,131
Hypothetical loan repayable in 5 years (ETB)	12,448.34	12,891.35	-1,152.16 (1,278.92)	366.00 (1,277.22)	1,130
Hypothetical loan repayable in 10 years (ETB)	22,210.22	23,731.81	-3,788.19 (2,618.29)	1,660.70 (2,611.2)	1,122
Expenditure on children's schooling (ETB)	184.68	168.20	63.98*** (20.34)	28.96 (20.32)	1,138
Number of children 6-15 in school	1.02	0.96	0.16** (0.08)	-0.01 (0.08)	1,138

*p below 0.1, **p below 0.05, ***p below 0.01. The aspirations measures asked what level respondents would like to achieve. The expectations measures asked what level respondents thought they would reach in 10 years. The individual components of aspirations and expectations reported in this table are unstandardised. To get weights for each dimension, we gave each respondent twenty beans. Enumerators asked respondents to allocate the beans according to the relative importance they gave to each of the four dimensions proposed. Robust standard errors clustered at household level are in parenthesis. The T-C difference and P-C difference are the treatment and placebo dummies run on the baseline data.

Table V: ASPIRATIONS AND EXPECTATIONS INDICES STRAIGHT AFTER SCREENING AND AFTER SIX MONTHS

Panel 1: Straight after screening (t=1)						
	Aspirations			Expectations		
Treated individual	0.13*	0.13*	0.12*	0.12**	0.12**	0.11**
	(0.07)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)
Placebo individual	-0.00	-0.00	-0.00	0.02	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>
Respondents	1959	1957	1957	1959	1954	1954
Control group mean	0.03	0.03	0.03	0.02	0.02	0.02
Treated-Placebo	0.13	0.13	0.13	0.10	0.10	0.08
P: Treated-Placebo	0.04	0.04	0.05	0.03	0.03	0.08
Panel 2: After six months (t=2)						
	Aspirations			Expectations		
Treated individual	0.04*	0.04*	0.03*	0.06***	0.06***	0.05***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Placebo individual	0.03	0.02	0.03	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>
Respondents	2063	2058	2058	2062	2054	2054
Control group mean	0.03	0.03	0.03	-0.01	-0.01	-0.01
Treated-Placebo	0.01	0.01	0.01	0.03	0.03	0.03
P: Treated-Placebo	0.60	0.51	0.69	0.11	0.10	0.14
Difference in post-screening and six-month treatment effects						
P: Treated _(t=1) = Treated _(t=2)	0.165	0.164	0.168	0.238	0.223	0.270
P: (Treated-Placebo) _(t=1) = (Treated-Placebo) _(t=2)	0.064*	0.070*	0.072*	0.149	0.163	0.295

*p below 0.1, **p below 0.05, ***p below 0.01. The indices are in standard deviations. Robust standard errors clustered at household level are in parenthesis. Regressions control for age, gender, highest grade attained, marital status and household assets. Column 1 (equation 1) includes only the treatment and placebo dummies and village fixed effects. Column 2 (equation 2) adds the lagged value of the dependent variable. Column 3 (equation 3) includes the lag and controls. The second last row is the p value of a test of whether the difference between the treatment and control is significantly different when measured straight after screening or after six months ($\delta_1, t=1 = \delta_1, t=2$). The last row is the same test, but for the difference between the treatment and placebo ($\delta_1 - \rho_1, t=1 = \delta_1 - \rho_1, t=2$). We conduct a seemingly unrelated estimation to account for likely correlations in the error term between the two equations testing aspirations for the same individuals at two points in time.

Table VI: ASSESSMENT OF DOCUMENTARIES AND PLACEBO AFTER SIX MONTHS

	Treatment	Placebo	P: Difference
Enjoyed watching what I saw	0.958 (0.201)	0.732 (0.443)	0.000***
Discussed film I saw a lot with my neighbours	0.873 (0.333)	0.713 (0.453)	0.000***
Discussed film others saw a lot with my neighbours	0.693 (0.462)	0.573 (0.495)	0.000***
Discussed film I saw at least once with neighbours in the past two weeks	0.331 (0.471)	0.216 (0.411)	0.000***
What I saw generated a lot of discussion within village	0.932 (0.251)	0.731 (0.444)	0.000***
Number who answered question	638	668	
Number who were given ticket but didn't answer	37	34	

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parentheses. The last column gives the p value of the difference between the treatment and placebo group.

Table VII: TREATMENT EFFECTS ON COMPONENTS OF ASPIRATIONS INDEX

	Aspirations index	Income	Wealth	Education	Social status
Treated individual	0.03* (0.02)	4069.15 (12326.19)	-4076.35 (4388.10)	0.16* (0.09)	0.71 (1.32)
Placebo individual	0.03 (0.02)	11678.66 (12169.93)	-1812.30 (4338.86)	0.09 (0.09)	0.95 (1.30)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Respondents	2058	2036	2007	1935	2036
Control group mean	0.03	110079.21	55265.97	12.89	80.39
Treated-Placebo	0.01	-7609.51	-2264.05	0.08	-0.24
P: Treated-Placebo	0.69	0.53	0.60	0.41	0.85

*p below 0.1, **p below 0.05, ***p below 0.01. This table reports on the aspirations index, which is in standard deviations. Robust standard errors clustered at household level are in parenthesis. Regressions control for age, gender, highest grade attained, marital status and household assets.

Table IX: TIME (IN MINUTES) IN WORK AND LEISURE

	Time in farm work			Time in leisure		
Treated individual	7.16 (7.82)	1.83 (8.68)	5.61 (7.36)	5.82 (10.59)	9.99 (10.82)	5.10 (10.58)
Placebo individual	-1.31 (8.30)	-8.83 (9.38)	-1.94 (7.99)	6.66 (11.23)	14.90 (11.56)	10.95 (11.20)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>
Respondents	2052	1950	1950	2052	1950	1950
Control group mean	310.54	310.54	310.54	798.91	798.91	798.91
Treated-Placebo	8.47	10.66	7.54	-0.85	-4.91	-5.85
P: Treated-Placebo	0.29	0.23	0.32	0.94	0.66	0.59

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parenthesis. Regressions control for age, gender, highest grade attained, marital status, household assets, village fixed effects and the outcome variable at baseline.

Table VIII: HETEROGENEOUS TREATMENT EFFECTS ON ASPIRATIONS

	Baseline aspirations		Age		Male		Highest grade		Household assets	
	t=1	t=2	t=1	t=2	t=1	t=2	t=1	t=2	t=1	t=2
Treated individual	-0.02 (0.03)	-0.01 (0.02)	0.09 (0.07)	0.01 (0.02)	0.17 (0.12)	0.03 (0.02)	0.06** (0.03)	0.03* (0.02)	0.14 (0.11)	0.03 (0.02)
Treated*Above median aspirations	0.28*** (0.11)	0.06** (0.03)								
Treated*Below median age			0.07 (0.18)	0.01 (0.03)						
Treated*Male			-0.10 (0.14)		-0.02 (0.03)					
Treated*Highest grade						0.04 (0.03)	-0.01 (0.01)			
Treated*Above median assets									-0.03 (0.12)	-0.03 (0.03)
Village F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged term	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1957	2058	1957	2058	1957	2058	1957	2058	1957	2058
Control group mean	0.03	0.03								
	Difference in post-screening and six-month treatment effects									
P: Treated _(t=1) =	0.861	0.281	0.248		0.369		0.321			
Treated _(t=2)										
P: Treated*Z _(t=1) =	0.041**	0.776	0.611		0.119		0.965			
Treated*Z _(t=2)										

*p below 0.1, **p below 0.05, ***p below 0.01. This table reports on the aspirations index, which is in standard deviations. Robust standard errors clustered at household level are in parenthesis. Regressions control for age, gender, highest grade attained, marital status and household assets. The table shows the coefficients on the interaction between the treatment dummy and a series of characteristics: whether the respondent had above median aspirations at baseline, whether the respondent is above median age, the respondent's gender, the highest grade they have completed and whether their household has above the median asset level. The second last row is the p value on a test of whether the treatment effect is significantly different between the straight after screening measurement and the measurement after six months ($\delta_1, t=1 = \delta_1, t=2$ in equation 4). The last row is the p value on a test of whether the interaction between the treatment dummy and the baseline characteristic is significantly different between the straight after screening measurement and the measurement after six months ($\theta_2, t=1 = \theta_2, t=2$). We conduct a seemingly unrelated estimation to account for likely correlations in the error term between the two equations testing aspirations for the same individuals at two points in time.

Table X: SAVINGS AND USE OF CREDIT

Panel 1: Savings						
	Has savings			Total savings		
Treated individual	0.05*	0.03	0.03	122.56**	106.19*	97.05*
	(0.03)	(0.02)	(0.02)	(61.64)	(54.42)	(52.12)
Placebo individual	0.01	0.01	0.01	-1.84	-13.85	9.02
	(0.02)	(0.02)	(0.02)	(49.38)	(44.85)	(41.48)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>
Respondents	2063	2063	2063	2053	2051	2051
Control group mean	0.39	0.39	0.39	182.36	182.36	182.36
Treated-Placebo	0.04	0.02	0.02	124.41	120.04	88.02
P: Treated-Placebo	0.12	0.33	0.44	0.04	0.02	0.08
Panel 2: Credit						
	Took out credit			Total credit		
Treated individual	0.03	0.03	0.03	22.35*	19.11*	20.70*
	(0.03)	(0.02)	(0.02)	(11.75)	(11.61)	(11.59)
Placebo individual	-0.01	-0.02	-0.02	5.44	3.07	3.29
	(0.03)	(0.02)	(0.02)	(11.74)	(11.52)	(11.47)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>
Respondents	2063	2063	2063	2044	2044	2044
Control group mean	0.34	0.34	0.34	100.99	100.99	100.99
Treated-Placebo	0.04	0.04	0.05	16.91	16.04	17.41
P: Treated-Placebo	0.12	0.10	0.06	0.18	0.20	0.16

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parenthesis. Regressions control for age, gender, highest grade attained, marital status and household assets.

Table XII: INVESTMENT IN CHILDREN'S EDUCATION

	Children 6-15 enrolled			Education spending		
Treated individual	0.28***	0.18***	0.21***	60.73***	34.81*	32.99*
	(0.09)	(0.07)	(0.06)	(21.02)	(19.37)	(18.76)
Placebo individual	0.08	0.09	0.10	31.72	18.00	20.20
	(0.09)	(0.07)	(0.07)	(21.16)	(19.56)	(19.17)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>
Households	1137	1137	1113	1128	1128	1104
Control group mean	1.23	1.23	1.23	197.42	197.42	197.42
Treated-Placebo	0.20	0.09	0.11	29.01	16.81	12.78
P: Treated-Placebo	0.02	0.21	0.11	0.20	0.41	0.54

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors are in parenthesis. The outcome is at household level. Regressions control for age, gender, highest grade attained and marital status of household head and household assets.

Table XI: HYPOTHETICAL DEMAND FOR CREDIT, IF LOAN IS REPAYABLE

	In one year			In five years			In ten years		
Treated individual	868.15 (1209.99)	1179.20 (1149.54)	506.32 (1158.01)	2127.78 (2289.59)	2419.65 (2129.20)	1296.50 (2145.14)	6211.66* (3266.93)	6636.43** (3109.23)	5538.00* (3173.37)
Placebo individual	1399.06 (1254.64)	1874.20 (1197.90)	1678.74 (1195.34)	-174.15 (2287.53)	-764.47 (2130.29)	-884.49 (2136.45)	-1424.10 (2796.91)	-2415.88 (2593.38)	-2863.60 (2529.75)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>
Households	1131	1125	1101	1126	1119	1095	1114	1100	1076
Control group mean	10525.73	10525.73	10525.73	22604.28	22604.28	22604.28	33955.50	33955.50	33955.50
Treated-Placebo	-530.91	-695.00	-1172.42	2301.93	3184.13	2180.99	7635.77	9052.31	8401.60
P: Treated-Placebo	0.68	0.57	0.35	0.32	0.15	0.32	0.02	0.00	0.01

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors are in parenthesis. Outcome is at household level. Regressions control for age, gender, highest grade attained and marital status of the household head and household assets.

Table XIII: TIME DISCOUNTING AND RISK AVERSION

	Subjective discount factor			Risk aversion					
				Coin			Market		
Treated individual	-0.01 (0.02)	-0.01 (0.02)	-0.01 (0.02)	-0.11* (0.06)	-0.10* (0.06)	-0.09 (0.06)	-0.05 (0.06)	-0.03 (0.06)	-0.03 (0.06)
Placebo individual	-0.03 (0.02)	-0.02 (0.02)	-0.02 (0.02)	0.01 (0.06)	0.03 (0.06)	0.03 (0.06)	0.07 (0.06)	0.09 (0.06)	0.08 (0.06)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>	<i>Yes</i>
Respondents	2061	2037	2037	2061	2035	2035	2061	2024	2024
Control group mean	0.54	0.54	0.54	1.26	1.26	1.26	1.25	1.25	1.25
Treated-Placebo	0.02	0.02	0.02	-0.12	-0.13	-0.12	-0.12	-0.12	-0.12
P: Treated-Placebo	0.32	0.31	0.25	0.05	0.04	0.05	0.04	0.05	0.06

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parenthesis. Regressions control for age, gender, highest grade attained, marital status and household assets.

Table XIV: LOCUS OF CONTROL, PERCEPTIONS OF CAUSES OF POVERTY AND LIFE SATISFACTION

	Locus of control			Causes of poverty			Life satisfaction	
	Chance	Others	Internality	Fate	Structural	Individual	Best life	Happiest life
Treated individual	-0.00 (0.16)	-0.05 (0.18)	0.28* (0.14)	-0.39* (0.20)	0.04 (0.15)	0.41** (0.16)	0.03 (0.11)	0.22* (0.12)
Placebo individual	-0.01 (0.15)	0.02 (0.17)	-0.08 (0.14)	-0.01 (0.20)	0.23 (0.15)	0.34** (0.16)	0.09 (0.11)	-0.00 (0.12)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Lagged outcome	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Controls	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Respondents	2008	2035	2049	2045	2031	1999	2055	2037
Control group mean	13.35	12.62	16.05	9.86	10.47	11.48	4.85	6.86
Treated-Placebo	0.01	-0.07	0.36	-0.38	-0.19	0.08	-0.06	0.22
P: Treated-Placebo	0.97	0.69	0.02	0.06	0.19	0.64	0.55	0.06

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parenthesis. Regressions control for age, gender, highest grade attained, marital status and household assets.

Table XV: CORRECTION FOR MULTIPLE TESTING

Family	Outcome	n	Effect	Naive p-value	FDR q-value
Aspirations	Aspirations index after screening	1957	0.12	0.05	0.053
	Expectations index after screening	1954	0.11	0.03	0.048
	Aspirations index after six months	2058	0.03	0.06	0.064
	Expectations index after six months	2054	0.05	0.01	0.042
Time allocation	Time in farm work	1950	5.61	0.44	1.000
	Time in leisure	1950	5.10	0.63	1.000
Credit and savings	Took out credit	2063	0.03	0.21	0.163
	Total credit	2044	20.70	0.07	0.163
	Has savings	2063	0.03	0.25	0.163
	Total savings	2051	97.05	0.06	0.163
Hypothetical credit	Loan repayable in 1 year	1101	506.32	0.66	0.786
	Loan repayable in 5 years	1095	1296.50	0.54	0.786
	Loan repayable in 10 years	1076	5538.00	0.08	0.316
Children's education	Children 6-15 enrolled	1101	0.22	0.01	0.002
	Education spending	1085	32.25	0.08	0.045
Self-concept	Locus of control: Chance	2008	0.00	0.98	0.961
	Locus of control: Others	2035	-0.05	0.78	0.843
	Locus of control: Internality	2022	0.28	0.05	0.133
	Perceptions of poverty causes: Fate	2045	-0.39	0.05	0.133
	Perceptions of poverty causes: Structural	2031	0.04	0.79	0.843
	Perceptions of poverty causes: Individual	1999	0.41	0.01	0.087
	Well-being: Best life	2055	0.03	0.78	0.843
	Well-being: Happiest life	2037	0.22	0.07	0.140
Time and risk preferences	Discount rate	2037	-0.01	0.75	1.000
	Risk aversion: coin	2035	-0.09	0.12	0.429
	Risk aversion: market	2024	-0.03	0.60	1.000

We use the Benjamini et al. (2006) procedure within seven families of outcome variables. Column 5 gives the naive p-value obtained from running each estimate independently, which is reported in the other tables in the paper. Regressions control for age, gender, highest grade attained, marital status, household assets, village fixed effects and the outcome variable at baseline. Standard errors for individual-level outcomes are clustered at household level. Column 6 gives the q-value that accounts for multiple testing within each family.

Table XVI: NUMBER OF TREATED/PLACEBO AMONG THE RESPONDENT'S FOUR CLOSEST FRIENDS

	N	All villages	Treatment villages	Placebo villages
<i>Distribution of peer-level treatment</i>				
No peer has seen documentary	2,056			
1 peer has seen documentary	941	45.77	25.48	66.21
2 peers have seen documentary	670	32.59	37.69	27.44
3 peers have seen documentary	331	16.10	26.65	5.47
4 peers have seen documentary	97	4.72	8.72	0.68
	17	0.83	1.45	0.20
<i>Distribution of peer-level placebo</i>				
No peer has seen placebo	2,063			
1 peer has seen placebo	984	47.86	69.38	26.17
2 peers have seen placebo	659	32.05	24.81	39.36
3 peers have seen placebo	327	15.90	5.43	26.46
4 peers have seen placebo	71	3.45	0.39	6.54
	15	0.73	0.00	1.46

Respondents were asked to list their four closest friends. These lists were matched to the lists of treated, placebo and control individuals in the village and neighbouring villages.

Table XVII: PEER EFFECTS ON ASPIRATIONS AND BEHAVIOUR

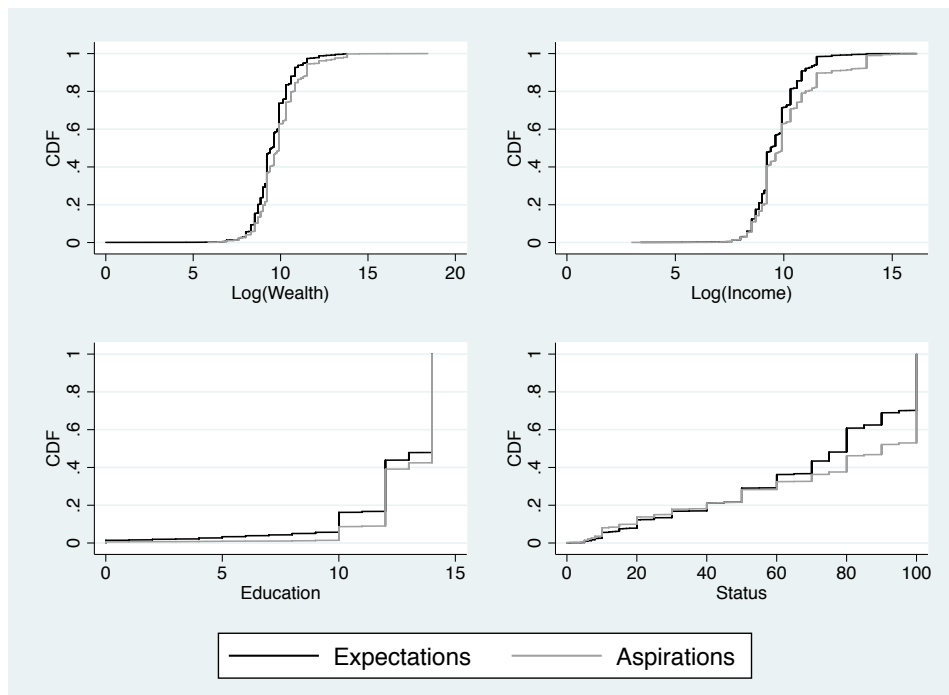
	After 6 months		Time allocation		Savings and credit		Children's education	
	Aspirations	Expectations	Time (work)	Time (leisure)	Total Savings	Total Credit	Children 6-15 enrolled	Ed. spending
<i>Panel A: Village-level treatment</i>								
Individual treatment	0.04*	0.06**	5.88	3.68	95.00*	20.35*	0.21***	30.13
	(0.02)	(0.02)	(8.54)	(12.23)	(56.10)	(11.76)	(0.07)	(19.94)
Village-level treatment intensity	0.00	-0.01	14.42*	-19.98*	13.50	-2.01	0.01	5.01
	(0.04)	(0.06)	(7.78)	(10.95)	(36.81)	(10.07)	(0.06)	(14.98)
<i>Panel B: Peer-level treatment</i>								
Individual treatment	0.04	0.07**	10.89	-5.79	137.64*	11.65	0.22**	53.11**
	(0.02)	(0.03)	(9.79)	(15.06)	(78.60)	(15.09)	(0.09)	(21.59)
Peer treatment	0.00	0.01	6.98	-11.09*	15.88	-7.37	0.05	32.12**
	(0.02)	(0.02)	(4.50)	(5.96)	(24.20)	(7.39)	(0.05)	(12.53)
Individual treatment*	0.00	-0.02	-6.65	3.1	-52.96	10.33	-0.02	-27.85
Peer treatment	(0.03)	(0.02)	(8.79)	(12.51)	(70.81)	(12.54)	(0.07)	(22.96)

*p below 0.1, **p below 0.05, ***p below 0.01. Village-level clustered standard errors in parenthesis. Regressions control for individual placebo treatment, age, gender, highest grade attained, marital status, household assets, screening site-level fixed effects and the outcome variable at baseline. Individual treatment measures whether the individual was allocated an invitation to watch the documentaries. Panel A: Village-level treatment intensity measures whether the individual lives in an intense treatment village, where 48 individuals in total were invited to watch the documentary (in intense placebo villages, only 12 individuals were treated). Panel B: Each surveyed individual was asked to list her four closest friends at baseline. The peer treatment variable is the number of an individual's four closest friends who were treated. The interaction of individual and peer-level treatment tests for the presence of sorting effects, where peers may both exert similar characteristics and react similarly to treatment. If parameters on the interacted term are insignificant, the coefficients on the peer treatment variable are considered causal. All estimates in Panel B include an interaction between peer placebo and individual placebo (not displayed).

SUPPLEMENTARY DATA

APPENDIX A: SUPPLEMENTARY TABLES AND FIGURES

Figure A.I: CUMULATIVE DISTRIBUTION FUNCTIONS FOR DIMENSIONS OF ASPIRATIONS AND EXPECTATIONS AT BASELINE



The expectations measure asked what level respondents thought they would reach in 10 years. The aspirations measure asked what level respondents would like to achieve. The graphs show the cumulative distribution function of each separate dimension of expectations (in black) and aspirations (in grey) at baseline. We display the measures before they are standardised. We log the income and wealth measures for ease of display, but the measures are not logged when used in the index.

Table A.I: BASELINE CORRELATES OF ASPIRATIONS INDEX AND COMPONENTS OF ASPIRATIONS INDEX

	Aspirations index	Income	Wealth	Education	Social status
Age	0.00 (0.00)	-911.83 (1339.91)	3812.52 (2918.19)	0.00 (0.00)	0.13** (0.06)
Male	0.16*** (0.02)	92559.38*** (26102.26)	37820.79 (65944.87)	0.36*** (0.08)	8.14*** (1.15)
Household assets ('000 ETB)	0.00* (0.00)	5089.10** (2479.02)	2881.00 (4830.84)	0.00 (0.00)	-0.11* (0.07)
Highest grade completed	0.02*** (0.01)	486.91 (4306.63)	70974.69* (42387.84)	0.10*** (0.01)	0.89*** (0.28)
Single, widowed or divorced	-0.03 (0.04)	-49276.79* (29179.69)	-24193.14 (62172.06)	-0.14 (0.22)	-1.00 (2.52)
Constant	-0.12* (0.07)	74791.90 (69013.83)	-223810.61 (200904.95)	12.06*** (0.41)	80.64*** (3.63)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	2058	2047	2049	2015	2039

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parenthesis. Results are for aspirations, which asked what level respondents would like to achieve on each dimension. The aspirations index is in standard deviations, while the individual components of the index are unstandardised.

Table A.II: CORRELATIONS AT BASELINE BETWEEN ASPIRATIONS AND PSYCHOSOCIAL MEASURES AT BASELINE

	Aspirations					Expectations				
	Index	Income	Wealth	Education	Status	Index	Income	Wealth	Education	Status
Aspirations: level you'd like to achieve										
Index	1.00									
Income	0.57***	1.00								
Wealth	0.51***	0.02	1.00							
Education	0.48***	-0.00	0.03	1.00						
Status	0.49***	0.12***	0.03	0.08***	1.00					
Expectations: level you think you'll reach in 10 years										
Index	0.33***	0.07***	0.02	0.28***	0.34***	1.00				
Income	0.02	0.06**	0.00	0.01	-0.04*	0.48***	1.00			
Wealth	0.10***	0.03	0.07***	0.09***	0.03	0.61***	0.05**	1.00		
Education	0.25***	-0.02	-0.03	0.49***	0.10***	0.53***	0.03	0.08***	1.00	
Status	0.39***	0.11***	0.01	0.08***	0.79***	0.44***	-0.02	0.08***	0.09***	1.00
Locus of control										
Aspirations										
Aspirations	1.00									
Expectations	0.33***	1.00								
<i>Locus of control</i>										
Chance	-0.02	-0.02	1.00							
Others	0.00	-0.03	0.59***	1.00						
Internality	0.10***	0.15***	-0.22***	0.04	1.00					
<i>Causes of poverty</i>										
Fate	-0.06***	-0.09***	0.49***	0.29***	-0.31***	1.00				
Structural	0.01	-0.06***	0.19***	0.18***	-0.08***	0.42***	1.00			
Individual	0.00	0.07***	-0.09***	-0.02	0.29***	0.06***	0.26***	1.00		
<i>Life satisfaction</i>										
Best life	0.01	0.13***	-0.09***	-0.06***	0.09***	-0.05**	-0.09***	0.10***	1.00	
Happiest life	0.21***	0.20***	-0.05**	-0.01	0.13***	-0.09***	-0.07***	-0.08***	0.31***	1.00

*p below 0.1, **p below 0.05, ***p below 0.01. Indices are in standard deviations. All available observations are used to calculate a pairwise correlation without regard to whether variables outside that pair are missing.

Table A.III: EXPERIMENTAL INTEGRITY: RISK AVERSION, DISCOUNT RATE AND PSYCHOLOGICAL VARIABLES AT BASELINE

	Mean	Mean for controls	T-C difference	P-C difference	N
Subjective discount factor	0.54	0.54	0.00 (0.02)	-0.01 (0.02)	2,039
Risk aversion (coin toss)	0.99	0.95	0.1* (0.06)	-0.01 (0.06)	2,037
Risk aversion (market)	1.22	1.24	-0.06 (0.07)	0.04 (0.07)	2,026
Locus of control (internality)	15.54	15.51	0.04 (0.16)	-0.09 (0.16)	2,052
Locus of control (others)	11.82	11.79	0.15 (0.18)	0.09 (0.19)	2,045
Locus of control (chance)	12.37	12.35	0.14 (0.17)	0.12 (0.18)	2,030
Causes of poverty (fate)	9.28	9.22	0.23 (0.2)	0.13 (0.2)	2,047
Causes of poverty (structural)	10.24	10.31	-0.25* (0.15)	-0.08 (0.16)	2,049
Causes of poverty (individual)	11.21	11.17	0.02 (0.16)	-0.15 (0.17)	2,037
Well-being (best life)	4.24	4.20	0.03 (0.12)	-0.19 (0.12)	2,055
Well-being (happiest life)	6.61	6.59	-0.01 (0.15)	-0.12 (0.15)	2,037

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parenthesis. The T-C difference and P-C difference are the treatment and placebo dummies run on the baseline data. For the locus of control measures, scores on each of the Internality, Chance and Others scales can range from 5 (if respondents strongly disagree with all 5 items in the scale) to 20 (if they strongly agree with all 5 items). For perceptions of the causes of poverty measures, for the Structural and Individual scales, scores can range from 4 (strongly disagree with all four items) to 16. For the Fate scale, scores can range from 3 to 12 as there were only 3 items in this scale.

Table A.IV: STRUCTURE OF PAYOFFS IN RISK AVERSION LOTTERIES

(1) n	(2) Payouts (coin toss)		(3)	(4) Expected value	(5) Std. dev.	(6) $\Delta E/\Delta SD$	(7) Risk aversion	(8) S	(9) Value given
	Heads	Tails							
1	2.5	2.5	2.5	0.00	0.35	Severe	3.26 - ∞	3.260	
2	2	4	3	1.41	0.35	Intermediate	1.2 - 3.26	1.978	
3	1.5	5.5	3.5	2.83	0.35	Moderate	0.68 - 1.2	0.903	
4	1	7	4	4.24	0.35	Slight-to-neutral	0.33 - 0.68	0.474	
5	0	10	5	7.07		Neutral-to-preferred	0 - 0.33	0.165	

Table A.V: EXPERIMENTAL INTEGRITY: RISK AVERSION AT BASELINE

n	Distribution of sample at baseline: Coin toss				Distribution of sample at baseline: Maize sale			
	Total	Treatment	Placebo	Control	Total	Treatment	Placebo	Control
1	0.12	0.12	0.12	0.11	0.19	0.17	0.2	0.19
2	0.13	0.15	0.11	0.12	0.14	0.12	0.15	0.14
3	0.26	0.25	0.27	0.25	0.22	0.24	0.23	0.21
4	0.15	0.16	0.12	0.15	0.18	0.22	0.15	0.18
5	0.36	0.31	0.39	0.36	0.27	0.25	0.28	0.28
N			2,037				2,026	

*p below 0.1, **p below 0.05, ***p below 0.01. Options 1 to 5 correspond to the choices in Table A.4. Using a chi-squared test, there are no significant differences in the distribution over categories between treatment, placebo and control group. For the difference between treatment and control, $p=0.263$ for coin and 0.106 for maize sale. For the difference between placebo and control, $p=0.372$ for coin and 0.593 for maize sale.

Table A.VI: CRONBACH'S ALPHA FOR LOCUS OF CONTROL AND PERCEPTIONS OF CAUSES OF POVERTY SCALES

	Items	Cronbach's alpha	n
Locus of control			
Internality	5	0.753	2,052
When I make plans, I am almost certain/guaranteed/sure to make them work			
I can mostly determine what will happen in my life			
I am usually able to protect my personal interests			
When I get what I want, it is usually because I worked hard for it			
My life is determined by my own actions			
Powerful others	5	0.724	2,045
I feel like what happens in my life is mostly determined by powerful people			
My life is chiefly controlled by other powerful people			
In order to have my plans work, I make sure that they fit in with the desires of people who have power over me			
People like myself have very little chance of protecting our personal interests when they conflict with those of more powerful people			
Getting what I want requires making those people above me (people with higher status) happy with me			
Chance	5	0.676	2,030
When I get what I want, it is usually/mostly because I'm lucky			
Often there is no chance of protecting my personal interests from bad luck happenings			
To a great extent my life is controlled by accidental/chance happenings			
It is not always wise for me to plan too far ahead because many things turn out to be a matter of good or bad fortune			
My experience in my life has been that what is going to happen will happen			
Perceptions of causes of poverty			
Individual	4	0.703	2,047
They lack the ability to manage money or other assets			
They waste their money on inappropriate items (e.g. alcohol, cigarettes, gambling)			
They do not actively seek to improve their lives			
They are not motivated because of food aid (e.g. direct support programme, food parcels)			
Structural	4	0.703	2,049
They are exploited by rich people			
Society fails to help and protect the most vulnerable			
The distribution of land between poor and rich people is uneven/unequal			
They lack opportunities due to the fact that they come from poor families			
Fate	3	0.898	2,037
They lack luck			
They have encountered misfortunes			
They have bad fate/destiny			

APPENDIX B: RISK AND TIME PREFERENCES AND PSYCHOSOCIAL MEASURES

B.I Rate of time preference

We use a survey-based measurement tool used by Cole et al. (2013) in India and Hill et al. (2013) in Ethiopia to construct individual subjective discount factors. The scale and logistics of the study meant that a survey-based tool was chosen over an experimental tool. We find a very similar distribution over categories to Hill et al. (2013) in Ethiopia.

The outcome variable is the subjective discount factor $\beta = \frac{1}{1+\delta}$, 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 factor between 1 and 0.8. We assign 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. Those who have a discount factor lower than 0.667 are asked how much they would need to be given in one month to choose to wait.²⁶

26. 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 actually concave. More complex measures are available to address this (Andersen et al., 2008; Andreoni and Sprenger, 2012), but we do not use these because of the very limited numeracy of our respondents. Results using the log of

We report individual discount rates at baseline in Appendix Table A.III. The relatively high mean subjective discount factor at baseline (0.54) could reflect respondent suspicion about default on the promised future payment or measurement error because of the hypothetical nature of the question (as Cole et al. (2013) hypothesise). Unlike Duflo et al. (2008), we find no significant difference between discount rates at baseline in the rainy, hungry season and endline at planting time ($p=0.213$).

B.II Risk aversion

We use survey-based instruments to calculate risk preferences, following the line of enquiry by Binswanger (1980) and in line with Cole et al. (2013) and Hill et al. (2013). Individuals were presented with two hypothetical decisions. The first asked which of five hypothetical payouts they would choose if the payout was determined by a coin toss (Cole et al., 2013). The second asked about the amount of price risk individuals would choose when selling surplus grain output (Hill, 2009). It had the same structure of payouts as the first question but multiplied by 100. All payouts had the same, constant probability, as in a coin toss, which is simple to explain to respondents.

The payout options are shown in Columns 2 and 3 of Appendix Table A.IV. Columns 4 and 5 give the mean and variance of each lottery. The successive lotteries offered increase in both mean and variance, so effectively payouts were ordered from most to least risk averse.

As in Hill (2009) and Binswanger (1980), we assume a constant partial risk aversion utility function (CPR) of the form $U = (1 - S)M^{1-S}$, where U is utility, S is partial risk aversion (fixed regardless of the level of payoff), and M is the certainty equivalent of a given lottery. Column 8 shows the range for the coefficient of partial risk aversion, calculated from this function, that corresponds to each of the five payouts. For Column 9, we follow how Hill (2009) scales this coefficient to assign risk aversion “numbers” to the discreet classes. For options 2-4, we take the geometric mean of the endpoints as the coefficient, because as the interval length decreases the alternatives get more risky). For option 1, the upper bound for the coefficient is infinity and the lower bound is 3.26. Only 12 per cent of individuals chose this option, so their partial risk aversion is unlikely to exceed 3.26 by very much, so we allocated them a value of 3.26. For option 5, which has an endpoint of 0 (assuming no respondent is risk loving), we use the arithmetic mean. The distribution of individuals across categories is very similar to results from the same measure in the Ethiopian Rural Household Survey (Hill et al., 2013).

Table A.III shows that the mean coefficient of partial risk aversion at baseline is respectively 1.22 (market) and 0.99 (coin toss). We show a further test for balance in risk preferences at baseline in Appendix Table A.V. We examine the variable in its original categorical form and find, using a chi-squared test, that the distribution of individuals across categories is balanced for both measures of risk preference.

B.III Locus of control and attributions for poverty

To measure locus of control, we use the Internality, Powerful Others and Chance (IPC) scale (Levenson, 1981), which captures three independent components of the construct of control, shown in Table We used a selection of items from each of Levenson’s three scales, omitting those which were not appropriate to the rural Ethiopian context. Respondents scored 1 on an item if they "Strongly disagree", 2 if they "Disagree", 3 if they "Agree" and 4 if they "Strongly agree". Scores on each of the Internality, Chance and Others scales can range from 5 (if respondents strongly disagree with 5 items) to 20. For example, higher values on the Internality scale indicate that respondents see outcomes as contingent on individual behaviour.

The Attributions for Poverty scale (Feagin, 1972, 1975) measures people’s perceptions of the causes of poverty among people in general, rather than only in their own lives. We use a version adapted for China (Shek, 2003). The scale assesses the extent to which respondents agree with each of three types – individualistic, structural and fatalistic – of explanations for poverty.²⁷ We used the same four point scale as for locus of control. For the structural and individual scales, which had four items, scores can range from 4 (strongly disagree, scored as 1, with all four items) to 16 (strongly agree, scored as 4, with all four items). For the fate scale, which had three items, scores can range from 3 to 12.

APPENDIX C: SUMMARY OF DOCUMENTARIES AND PLACEBO

The treatment consisted of four documentaries about two men and two women. Two documentaries are described below. Two documentaries are not summarised here, “Immortal Treasure”, about Ayelech Fikre, and “The Exemplary Achievement”, about Waki Feyyera. The four documentaries and an example of the placebo segments are available at <https://www.youtube.com/channel/UCqfoNjCzt8YPjTRWQaMQfAg>.

the discount factor are very similar to those displayed.

27. We dropped the item “They are born with less talent/they are less gifted” from the Fate scale because it was poorly translated and did not cluster closely with the other three items in factor analysis. We altered one item in the original Individual scale, which refers to welfare, to refer to food aid.

C.I Teyiba Abdella, in the video “The Life Transforming Flour Trade”

Teyiba Abdella lives in a district in Eastern Hararghe zone, Oromia Region. Most people in the district are involved in cultivating crops and livestock and in trade. Teyiba is engaged in both trade and farming. She married her husband, Aliya Yousuf, by choice. Her parents refused to bless her marriage, so Teyiba and Aliya 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 wheat 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 villages used to criticise Teyiba for being the major breadwinner, but she rejected their criticisms. People in the village have a high regard for her hard work and commitment. Aliya, Teyiba’s husband, admires her strength and believes she is a great role model for people in their village.

C.II Beshir Malim Yisak, in the video “The Fast Journey”

Beshir Malim Yisak is a farmer living roughly 658 kilometres south of Addis Ababa. He is 27 years old, married, with two children. He has no formal education but is considered a model farmer in the area for his considerable achievement in a short period of time. 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 started planting vegetables, which he sold at the market, and bought a pair of oxen after a good harvest. Three years later, Beshir used money he had saved to purchase a water pump from Addis Ababa, with the help of the agricultural expert. Beshir was able to water a larger area with his pump than with buckets, so he rented additional land to expand his farm. He started planting papaya, sugarcane 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 and producing tree seedlings for sale. 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.

C.III 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 looks unhappy. When pushed, he explains the reason humorously and with great hesitation: his wife is having an extra-marital affair. Like the documentaries, the segment is in Oromiffa.