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Abstract

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 which ignore some options for investment. This paper reports on findings of an innovative experiment to test this in rural Ethiopia. Firstly, individuals were randomly invited to watch documentaries about people from similar communities who had succeeded in agriculture or small business, without help from government or NGOs. A placebo group watched an Ethiopian entertainment programme and a control group were simply surveyed. Secondly, the number of invitees was varied by village to assess the importance of peer effects in the formation of aspirations. Six months after the screening of the documentaries, aspirations had improved among treated individuals but did not change in the placebo or control groups. Effects were larger for those with higher aspirations at baseline. We also find evidence of treatment effects on savings and credit behaviour, children's school enrolment and investments in children's schooling, suggesting that changes in aspirations can translate into changes in forward-looking behaviour. There are also positive treatment effects on a set of related measures from psychology and sociology, including a measure of locus of control, which theory predicts should behave in similar ways to aspirations. We also find that peer effects result in further impact on educational spending and induce more work and less leisure. That a one-hour documentary shown six months earlier induces such actual behavioural change offers challenging and promising areas for further research and the design of poverty-related interventions.

1 Introduction

Governments and non-governmental organisations have long offered solutions to help poor people escape poverty that try to build up the poor'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 which 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 the poor, such as linked to risk and credit in agriculture (Karlan et al., 2013), in seasonal migration (Bryan et al., 2012) or in 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 land fertility (Goldstein and Udry, 2008) and 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 to us, 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, 2012; Hanna et al., 2012). All decision-makers, rich and poor, exhibit such bounded rationality. Furthermore, poor people 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, “better”, behaviours (Thaler and Sunstein, 2008). Failures and biases in people’s mental processes no doubt matter: impatience, discounting of long-term implications of choices, or struggles to commit have been shown to lead people to choose lower-return options among the choices available, which is particularly 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 deprived and 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 offers of help from government or NGOs. We found that this intervention changed aspirations, 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 limits 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 our 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 (2001) and Ray (2006) argue that individuals largely form aspirations by observing the outcomes of 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, in Indian villages where girls had female role models because the village was randomly assigned to reserve a seat on the village council for a woman, 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 show depicting traditional Ethiopian song and dance, 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, 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 worked more, took less leisure and improved their spending on children's education, even if they did not see the documentaries themselves.

Other experimental studies find that providing concrete information about untapped opportunities can boost investment. Jensen (2012) finds that, in Indian villages randomly selected for recruitment visits publicising opportunities in call centres, young women increased their labour market participation and enrolment in relevant courses, delayed their marriage and their first 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 do not suggest particular actions that ought to be taken and do not draw general conclusions about opportunities for others based on the experience of the documentary subject. Similar to 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 model in small but perceptible ways.

The next section offers a discussion of the concept of aspirations in the social psychology and economics literature. Section 3 describes the experimental design and the data, including the measures of aspirations. Section 4 describes the experimental design, Section 5 describes the direct impact of the treatment on aspirations and expectations, and Section 6 describes the effects on future-oriented behaviour. Section 7 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 indicators. Section 8 offers the analysis of peer effects. Section 9 concludes.

2 The concept and measurement of aspirations

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) write 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 have a number of features in common. 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, 2001; 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 been most 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.¹

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.² Although educational and occupational aspirations are discernible examples, 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 might 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 specific measures of aspirations in four 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 (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 Seyoum Taffesse, 2014). Beaman et al. (2012) use a similar approach, where each aspiration constituent is measured as a categorical variable and the weights used for standardisation are not person-specific. Income, measured in Ethiopian birr (ETB) includes cash income from all activities. Wealth caputed

¹There is also a work in economics using aspirations as reference point in relation to learning and behaviour under risk (MacLeod and Pingle, 2005; Golman and Loewenstein, 2012).

²As goals that provide rationale for corresponding behaviour, aspirations may be viewed as a type of preferences. Bowles (2004) characterised preferences explicitly as “reasons for behaviour”. 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).

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. 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 the dimension’s significance for them, which accounts for heterogeneity in valued attributes of life.³ 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 $A_i = \sum_k \left(\frac{a_i^k - \mu_k}{\sigma_k} \right) w_i^k$.

3 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” where they are exposed to the lives of potential role models from a similar background to theirs who have improved their economic position. There is a substantial literature on aspirations in laboratory experiments.⁴ 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.

3.1 The study site and the experimental design

The field experiment was conducted in Doba, an administrative district 380 km east of Addis Ababa, Ethiopia’s capital city, during 2010-11. The documentaries all featured relatively poor rural inhabitants in grain-growing areas, so the study site was selected to be similar. Doba is relatively poor and food insecure: it was one of the first districts selected for the national Productive Safety Net Program (PSNP) in 2005. The programme is targeted at the most chronically food-insecure districts in the country. In the 2007 Census, only 1.5 per cent of Doba’s population lived in urban areas and 99 per cent were subsistence farmers growing sorghum and maize (Central Statistical Authority, 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 sampled sixty-four of these villages for the intervention. The screening and the baseline survey took place between September and November 2010. One team of 30 enumerators moved from village to village to ensure homogeneity in how the screenings were conducted. In each village, the enumerators compiled a list of all households and randomly selected eighteen households. Six were allocated to the treatment group, six to the placebo group and six to the control group. A baseline survey was conducted with all household heads and their spouses at their home (n=2,063, see Table 1).

The district is extremely remote: the majority of villages surveyed were only accessible by 4x4 vehicle and some required camel transportation. There was 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 screenings of the documentary were a highly unusual event.

³To get a concrete number for the weight attached to each dimension, we gave each respondent twenty beans and a piece of paper divided into quadrants. Enumerators explained which dimension of life each quadrant represented and asked respondents to allocate the beans according to the relative importance they gave to each of the four dimensions proposed.

⁴For 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.

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, to a screening session in a few days time.⁵ Households were told that they could only attend the screening at the time and place written on the ticket. The name and survey identifier of each respondent was written on the ticket and households were told the ticket was non-transferable. Respondents were also told that they would each receive a bag of sugar as compensation for their time after the screening. To avoid priming effects, all respondents were simply told that the screening was an entertainment show. The six households in the control group did not receive any tickets. They were not told that other households were invited to a screening session, but a follow-up appointment was made to interview them at their homes on the same day as the screening. They were also told they would receive a bag of sugar at the follow-up interview.

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

3.2 Documentary and placebo content

In 2009, we ran a competition in Ethiopia. Development agents and NGO staff in rural areas were asked 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, in which they narrated their life story, by Next Studios, an Ethiopian production company. Of the ten documentaries, four were selected for the intervention, two about men and two about women.⁶ The documentary subjects were from other districts in the Oromia region, of which Doba is a part. It was thus almost impossible that respondents would know anyone in the videos and we have no evidence that this occurred. Each documentary is 15 minutes long and in Oromiffa, the local language in Doba.

The documentaries have some common themes. Firstly, none of the 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 status to those around them. They all took slightly different courses of action to those around them, such as starting or expanding a small business, diversifying their source of income, improving their farming practices, or acting outside cultural norms by marrying for love or by adopting non-traditional divisions of household responsibility between spouses. Secondly, spouses and mentors featured in the documentaries highlighted the personal qualities of the subjects, such as perseverance, determination and reliability. The subjects also emphasised the importance of setting goals and working towards them. Thirdly, individuals succeeded largely through their own efforts and by drawing on assistance from community members and available resources, not through outside government or NGO intervention.

The literature on aspirations highlights that people may form unrealistically high aspirations in the absence of "reference points" or potential role models who come from similar backgrounds (Genicot and Ray, 2010; 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).

⁵Bernard et al. (2011) describe the key measurement and identification challenges that informed the experimental design.

⁶The documentaries, with English subtitles, are available online at <https://www.youtube.com/user/CSAEOxford>. A summary of some examples is given in Appendix C.

The other concern is that such an intervention will not raise aspirations enough, if documentaries are about very poor individuals who face challenges that are irrelevant to the audience. Audience member reports suggest the documentaries were also well-pitched: 73 per cent of audience members 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 thus started from a similar position to the audience but provided an example of a better life to which the audience could reasonably aspire.

In such an isolated area, a screening event 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, as in other studies which have examined the effect of information provision (Card et al., 2012) or the effect of watching soap operas (Berg and Zia, 2013). So, while one third of households were given an invitation to watch a screening of four of the documentaries, one third of households were given an invitation to watch a hour-long Ethiopian TV show featuring performances of traditional song and dance. One third of households were surveyed but did not receive any invitations.

3.3 Peer-level treatment

As noted above, it is likely that an individual’s peer networks affect her aspirations. To summarise, 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 revision of aspirations and changes in behaviour in response to the treatment, we generated exogenous variation in the extent to which an individual’s network was exposed to the treatment. In each screening site of four villages, we assigned two villages to be “intense treatment” villages and two to be “intense placebo” villages. In the “intense treatment” villages, we randomly selected 18 additional households to receive tickets to the documentary but did not collect data on these individuals. In the “intense placebo” villages, we randomly invited 18 additional households to the placebo session.

3.4 Compliance and experimental integrity

Compliance levels are reported in Table 2. If all the households sampled had a head and 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 surveyed or interviewed in the baseline or given tickets because they were away, ill or had just given birth. The remaining individuals were interviewed. Compliance among those allocated tickets is high, 96 and 92 per cent for surveyed and non-surveyed individuals respectively, despite an average 29 minutes travel time to the screening site.⁷ There are no significant differences in compliance between groups.

Individuals who had moved or were away were tracked within the district. Attrition between baseline and follow-up is thus very low, with only 49 individuals (2.2 per cent of the sample) not found for the second round. There are no significant differences in attrition rates between groups. For the remainder of the paper, we examine the 2,063 individuals who appeared in both rounds. For all continuous outcome variables used in the paper, we trim the sample. Individuals who report values on the outcome variable which are four standard deviations or more above or below the sample mean have that value of the outcome replaced as missing. This applies to 26 observations on the expectations

⁷We consider an individual as compliant if we recorded that they arrived at the correct screening. For 153 individuals, the time of arrival of individuals at the screening venue was not recorded, but individuals were also not marked as absent. If these individuals are included as compliant, compliance rates rise to 98 and 97 per cent for surveyed and non-surveyed households. Only 24 respondents attended the incorrect screening. This was because of the tightly controlled ticketing: respondents were checked at the door to the screening and had to produce colour-coded tickets bearing their name and the time of screening to which they were invited.

measure (1 to 2 per cent of the sample of individuals) and 61 observations on the measure of aspirations (3 per cent of the sample).⁸

Table 3 reports tests of balance for both treatment and placebo experiments on a variety of individual and household level variables. No significant differences are found across samples in education, gender or age of the individuals. There are also no differences 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 this is a small proportion (6 per cent) of the sample on average. We find some imbalance in asset values (including tools, furniture, vehicles, electrical goods) between the placebo and the control group.

Instead of a consumption module, we use a food insecurity index to capture actual and perceived levels of food insecurity. We use an adapted version of the United States Department of Agriculture’s food insecurity questionnaire, which codes households as food-secure, food-insecure without hunger and food-insecure with hunger, based on a set of standardised questions about actual recent experiences of hunger and concerns about having insufficient nutritious food (Bickel et al., 2000; Carlson et al., 1999). The adapted version has been used in other studies in Ethiopia (Hadley et al., 2008). We find (using a chi-squared test) that the distribution of households over categories is different ($p=0.09$) for the treatment and control groups on the food security measure. Across groups, 58 per cent of households fall into this category, reflecting high levels of food insecurity in the district. We ensure to control for unbalanced variables to prevent the lack of balance affecting estimation of the treatment effect.

3.5 Aspirations and outcomes at baseline

We test the impact of the experiment on aspirations and a specific set of other outcome indicators that capture future-oriented behaviour. As discussed in Section 2, we use an aspirations index based on a standardised weighted average of aspirations measured over four dimensions: income, wealth, education and status. Table 4 reports expectations and aspirations at baseline, using non-standardised variables, i.e. as recorded in the survey. Both mean expectations and especially aspirations are relatively high. For example, current household income levels at baseline were ETB 6,243 (about USD 347 at exchange rates at the time of the survey, 1 ETB=USD18). Durable wealth was valued at around ETB 7,420 (USD412). Respondents were expecting to do rather well in ten years from now, expecting a multiple of current income and wealth. Income and wealth aspirations are far higher, up to 20 times 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 income of about USD1,350). The gap between expectations and aspirations is smaller in the dimension of education and social status. In terms of education, mean expectations are full secondary education (12.43 years), and aspirations are somewhat higher (12.91 years), including some college education for many. The much lower cross-sectional variance in the aspirations measures is striking, reflecting a larger tail of low expectations in education than in aspirations. Currently, about 44 per cent of individuals said they were consulted for important decisions in the village – our measure of social status – and they aspired to reach 75 per cent. There are no differences between the treatment, placebo and control group in any of these variables.

We are cautious about reading too much in the difference between our measures of expectations and aspirations in the data.⁹ As discussed in Section 2, expectations are conceptually different from aspirations, but their formation

⁸We use the same procedure on the other outcome variables considered, and we lose similar percentages of observations in this way. The consequence is a small difference on some outcome variables in magnitude of coefficients with or without trimming.

⁹Note that standardised translations of survey questionnaires were used to avoid heterogeneity in translation into Oromiffa, an issue of importance for the type of nuance needed to effectively distinguish between aspirations and expectations.

is related: someone’s aspirations are likely to shape the mental model used to form expectations. In the survey, respondents were first asked about aspirations and then about their expectations, so that framing effects may well influence the expectations answer further. Table A.1 in Appendix A shows that aspirations and expectations at baseline are strongly correlated, with a correlation coefficient of 0.33. Figure A.1 also shows that, except for a very small percentage of the sample at low values of the status variable, there is, as would be expected, stochastic dominance in these measures of aspirations over expectations: the percentage of respondents aspiring for at least a particular level of each dimension is higher than the percentage of respondents expecting to reach that level.

Table A.2 reports correlates of the aspirations index and aspirations on each component with a set of characteristics that are likely to be correlated with aspirations. Relationships are largely in the expected direction. Women have significantly lower aspirations than men on all dimensions except wealth, where their aspirations are lower but not significantly so. Individuals who are literate report 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 3,760 ETB in income aspirations – and aspire to higher levels of education for their children.. Older individuals have higher status aspirations but not wealth or income aspirations.

Beyond its impact on aspirations, we investigate whether our experiment affected actual behaviour. In the survey, we measured a number of outcomes that reflect various forms of forward-looking behaviour. We did not investigate whether individuals undertook different productive activities, such as diversifying into non-agricultural activities or moving into farming new crops or using different farming methods, as they would have required at least a full agricultural year to implement and the endline survey was done only six months after the experiment. Instead, we examined a range of other behavioural changes that suggest that individuals were planning more for the future or making investments that might improve their future economic position. We focus on time allocation, household savings and credit behaviour and children’s education. Table 5 reports descriptive statistics for outcome variables at baseline.

To capture time allocation to work and leisure, the household head was asked to report on the amount of time each member of the household spent on various types of tasks on a typical day during March.¹⁰ 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).¹¹ 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 collected information on respondents’ cash savings, including at banks, in a co-operative, with a voluntary savings and loan group (such as the 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 (including those with no savings) amounted to ETB 83 – roughly USD 5 at the time of the survey. We asked about all loans larger than ETB 15 (just under USD 1), including formal loans from co-operatives, banks or micro-finance institutions, loans from money-lenders or voluntary savings and loan groups or iqqub and loans from friends or family. At baseline, 43 per cent of individuals had taken out a loan of more than ETB 15 in the past six months. On average, individuals had taken out a total of ETB 176 (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 (without any interest payments required) if given the opportunity. This is intended to capture in a simple way the demand

¹⁰During the baseline and endline surveys, the time allocation questions referred to the month of March in 2010 and 2011 respectively. We thus avoid seasonal differences in time allocation between baseline and endline. These would in any case be unrelated to treatment.

¹¹We 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.

for credit taking into account Ethiopia’s current financial repression, rationed credit and general likely credit market failures in the remote setting involved. In particular, respondents were asked to suggest the loan size they would be interested in taking on if offered the opportunity for a loan with a 1, 5 and 10 year maturity.¹² The descriptive statistics show that the amounts individuals would borrow increase with the length of the repayment period, from ETB 6,079 if the loan is payable in one year, to ETB 12,905 if the loan is payable in five years, to ETB 23,118 if the loan is payable in ten years. This is consistent with the finding 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.¹³ At baseline, the average household had 1.24 children aged 6 to 15 enrolled in school. 20 per cent of households with children in this age group had no children enrolled in school. We 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, payment for schooling fees (such as for registration or examination) and donations to the school – at baseline 197 ETB or about USD 11 per year. This is consistent with estimates in other rural parts of Ethiopia (Orkin, 2012).

There are no significant differences between treatment, control and placebo for the time allocation and savings variables. There is some imbalance in the credit and education variables: the treatment group appears to have taken out somewhat more credit than the control group, but would take out smaller loans than the control group if offered the chance. Education spending and enrolment is also significantly higher for the treatment group at baseline. This is because the treatment group have significantly higher numbers of children than the control group. If we test the differences between groups controlling for the number of children, there are no significant differences between groups (on school expenses, $p=0.109$ for treatment vs control and $p=0.325$ for placebo vs control; on number of children in school $p=0.592$ and $p=0.233$ respectively). In the analysis below, we use specifications that control for baseline values of the outcome variables to account for any imbalances at baseline.

4 Empirical strategy and results

4.1 Empirical strategy: direct effects

We first examine direct effects on individuals from the experiment. We use the same specifications to examine whether exposure to the documentary causes changes in aspirations and 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 4.1 offers 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 session and P_i is an individual-level dummy variable equal to one if she was invited to a placebo session. δ_1 is the direct effect of being invited to a documentary screening; while ρ_1 is the effect of being invited to a screening of the Ethiopian traditional song and dance show. η_i is an individual-level error term.

$$y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \mu_v + \eta_i \quad (4.1)$$

¹²The exact questions were “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? (2) How much would you ask for if the loan is payable in 5 years? (3) How much would you ask for if the loan is payable in 10 years?”

¹³These 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. 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.

We account for village fixed effects using a set of village-level dummies, μ_v , which absorb village-level shocks, village characteristics and unobserved differences between screening sessions. We further account for potential clustering at household level by correcting standard errors for clustering at household level. In effect, within villages, allocation to different treatment statuses occurs at household level, but many outcomes are measured at individual level and observations for each spouse within households are likely not independent.

These estimates are the Intention-to-Treat (ITT), although given the extremely high rates of compliance, these effects are unlikely to differ substantially from the Average Treatment Effects on the Treated (ATT). We do not compute the ATT because there are too few non-compliers to estimate parameters correctly.¹⁴ We use the entire sample of respondents who were given tickets, including those non-compliers who missed the screening they were invited to or attended the incorrect screening.¹⁵

In Equation 4.2, we include y_{i1} , the baseline value of the dependent variable. This enables us examine the change in the outcome variable between the baseline and endline, but not to impose any structure on the relationship between the outcome at baseline and endline. ANCOVA regression controlling for the outcome variable at baseline is also more efficient than either difference-in-difference estimators or simple post-treatment estimation when the outcome variable is measured with noise, as is likely the case for most of our measures (McKenzie, 2012).

$$y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \gamma y_{i1} + \mu_v + \eta_i \quad (4.2)$$

In Equation 4.3, we include a vector of additional controls X_{i1} measured at baseline. We use variables that we have theoretical reason to believe might influence aspiration and other outcomes, as these are most likely to increase precision by explaining variation in the outcome variable. All controls are captured at baseline. We include age, gender, the highest grade the respondent completed at school, whether the respondent is single (unmarried, divorced or widowed), household wealth (captured by the total value of the household's assets excluding their land and house) and whether the household is food insecure, using the indicators as described in Section 3.4.

$$y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \gamma y_{i1} + X'_{i1} \pi + \mu_v + \eta_i \quad (4.3)$$

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.

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.

4.2 Treatment effect on aspirations

In Table 6, we report the treatment effect of the intervention on both our measures of aspirations and expectations. These variables were not only collected six months after the intervention (reported in Panel 2), but also straight after showing the documentaries (Panel 1). In all regression tables, the first column is the specification in Equation 4.1, including only the treatment and placebo dummies and village fixed effects. The second column is the specification in

¹⁴In addition, estimating the ATT assumes that treating the compliers has no effect on non-compliers. We find some 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.

¹⁵We only drop the 49 individuals who were not found for the follow-up survey.

Equation 4.2, which adds the lagged value of the dependent variable. The third column is the specification in Equation 4.3, which includes the lag and the vector of controls X_{i1} . As can be seen, there is little difference between these three specifications.

The first panel of Table 6 reports on aspirations straight after the screening.¹⁶ Respondents in the treatment and placebo group were interviewed at the screening venue, while respondents in the control group were interviewed at the same time as the screening at their homes. Being invited to a documentary screening session has a positive and significant effect (δ_1 in Equation 4.3) 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. Aspirations and expectations among the treated group are also significantly larger than among the placebo group ($\delta_1 - \rho_1$), 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 4.3).

The second panel 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.¹⁷ The last panel tests 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 has persisted over time and is not a temporary change in mood immediately after an inspiring screening.¹⁸ However, there is evidence that the difference in aspirations between treatment and placebo group has significantly narrowed over time, in line with the results as shown in Panel 1 and 2. The difference in expectations between treatment and placebo group has also narrowed, but the decrease is not statistically significant.

It is striking that six months after screening four 15-minute documentaries, a persistent significant effect is found for those treated, even if the size of this effect has declined over time (including through spillover effects). To investigate how credible this is, and whether this may have had further impacts, including on other people, we asked the treated and placebo individuals how they felt about the screening six months after the intervention. As shown in Table 7, high proportions of respondents liked the screenings, with a significantly higher proportion liking the documentary (96 per cent) than the placebo (73 per cent). The majority of respondents had 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 have discussed the screening they saw a lot 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 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 provides further support for the possibility of spillover effects. We discuss these further in Section 8.

¹⁶There 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 the same across groups and uncorrelated with treatment.

¹⁷There is no significant difference between the treatment and placebo groups ($\delta_1 - \rho_1$) once control variables are included. Our 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.

¹⁸These results support the validity of the measure: results are not significantly different when the same measure is administered to the same individuals by different enumerators six months apart.

In Table 8, we investigate the direct and indirect effect of treatment on the various components of the aspiration index. Each column reports a separate estimate of Equation 4.3 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, where primary schooling is relatively accessible to most families as a means to improve their future opportunities and there have been fast increases in educational enrolment and completion in recent years. Access to primary 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 gone down 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 many of the barriers to enrolment, although there still are costs of stationary, uniforms and some levies that parents need to pay (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).

While plausible, these results are unexpected, 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.

The results have two further implications. This change has occurred in one specific dimension, which suggests that the intervention does not simply focus individuals on the future and alter their discounting of future utility or disutility, but rather changed only some of their aspirations. Secondly, results highlight that the effects of improvements in aspirations can be intergenerational. 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.

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 4.4, 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.

$$y_{i2} = \alpha + \delta_1 T_i + \theta_1 Z_i + \theta_2 T_i * Z_i + \mu_v + X'_{i1} \pi + \eta_i \quad (4.4)$$

Results are presented in Table 9; we show only the results for aspirations.¹⁹ We note that the aspirations boost from the experiment after the screening is only for those with above median aspirations and those with above median wealth. There is significant depreciation of this effect, as shown in the bottom panel, and the boost in aspirations after six months remains only for those with above median aspirations to start with. Those with lower initial aspirations to start with (in an overall deprived setting) are not affected by the screenings. We find no other significant heterogeneous treatment effects by age, gender or level of education in this sample.

¹⁹Given 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.

5 Effect of treatment on future-oriented behaviour

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 A.3.

Table 10 reports on the impact on savings and actual credit. We examine 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 a 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 89 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 72 ETB more savings than the placebo group, although the difference is not significant ($p=0.15$ on the Wald test of the difference).

In the second panel of Table 9, we examine credit behaviour. We find that the treatment group took out 22 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 ($p=0.17$).

As a final credit-related variable, we 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 11. 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, it may be a relatively easy way to measure investment with results in the relatively short run. In the first three columns of Table 12, 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 0.19, a 15 per cent increase from the baseline average of 1.23 children enrolled in school.²⁰ The number of children in school in placebo group households increases by 0.12, significant at the 10 per cent level, so we cannot reject that the treatment and placebo effects are the same.

In the second three columns of Table 12, we examine spending on schooling for children in the household, a total of the amount spent on uniforms, stationery and books, textbooks, payment for schooling fees (such as for registration or examination) and donations to the school. Treated households spend 33.83 ETB more on school expenses than the control group (or 17 percent more), a difference which is significant at the 10% level. There is no significant effect on the placebo group.²¹

²⁰These effects may underestimate the true 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. But some households may have received the treatment after the last point when they could have enrolled their children in school, so they couldn’t have enrolled their children in response to the treatment, even if they had wanted to.

²¹Again, 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. The endline measure suffers from some measurement error, because households were treated between August and October 2010. So some households would have had limited time to increase school expenditure after the intervention if they had 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. Although there is nothing related to education in the screened documentaries, we find strong results in relation to children’s education, whether it is through changes in aspirations or through changes in actual behaviour.

6 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 attribute the effects to the content of the documentary, separate from the effect of a screening and gathering in a remote rural area. The experiment also suggests 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. Similarly, 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. However, this does not establish that aspirations are the main, or even a relevant, mechanism through which changes occur. Other, possibly more important, changes may have occurred in the mind-set of participants and led to changes in their behaviour. In this section, we explore a number of possible traits and psychosocial outcomes that may support or contradict the plausibility of the aspirations channel as a relevant explanation. We also explore statistical robustness; in particular whether statistical corrections for the testing multiple outcomes alters our main conclusions.

6.1 Treatment effect on time and risk preferences

The documentaries showed individuals who were goal-oriented and planned and focused 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 risky actions, such as investing in new technologies or farming methods or diversifying their economic activities, and watching the documentaries may have encouraged respondents to be slightly less risk-averse. A change in time or risk preferences is not clearly conceptually related to a change in aspirations. If the treatment affects time preferences, it suggests we cannot draw conclusions about the likely psychological mechanism behind the changes in behaviour we observe in response to the treatment. Behavioural changes could arise because of changes in aspirations or because of changes in the extent to which respondents weight consumption in the present and planning for the future. We thus examine whether the intervention had any effects on discount rates and on risk preferences. We use particular (survey-based) measures, as in Cole et al. (2013) in India and Hill et al. (2011) in Ethiopia.

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 today of 1 ETB received in future. Table 5 shows that the mean in the data is 0.54 and there are no differences between groups at baseline. In Table 13, we find no treatment or placebo effects on the respondents’ discount rate. Changes in savings, investment in children’s education and other future-oriented behaviours cannot be explained by treated individuals becoming more patient.

To measure risk preferences, we used Binswanger-style lottery choices (Binswanger, 1980), as in Cole et al. (2013) and Hill et al. (2011). Individuals were presented with two hypothetical decisions. The first asked respondents which of five payouts they would choose if the payout was determined by a coin toss. The second asked about the amount of price risk individuals would choose when selling surplus grain output and had the same structure of payouts but

multiplied by 100. Both have also been used in Ethiopia before (Hill et al., 2011). Details of the measure are in Appendix B. Table 5 shows that that mean coefficient of partial risk aversion at baseline is 0.99 on the coin measure and 1.22 on the grain sale measure and there is no difference between groups at baseline. Table 13 shows that there is no significant difference between three groups in the coefficient of risk aversion post-treatment, indicating that changes in risk aversion are unlikely to explain the observed changes in behaviour.

6.2 Treatment effect on locus of control and perceptions of causes of poverty

There is a long tradition in psychology and sociology focusing on measurable concepts that capture aspects of people’s self-image and their perception of their ability to shape their lives and future. In short, these are constructs that affect their ability to act. If our intervention affects aspirations and if that in turn shapes their behaviour, it ought to affect these measures too. Appendix B gives details on all these instruments.

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 earlier efforts by Rotter et al. (1972) used in economics, for example by Heckman et al. (2006) and Heckman and Kautz (2012).²² 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. These measures are part of the group of psychological constructs known as “core self-evaluations” (Judge et al., 2002, 1997), which capture whether individuals believe they can act effectively to bring about desired results, or, alternatively, whether they emphasise the role of external factors in determining their life outcomes.

There is some evidence suggesting that people with higher positive core self-evaluations are more likely to pursue the attainment of aspirations (Shah and Higgins, 2001), although there is limited work in psychology measuring aspirations among adults using specific values on various dimensions of life outcomes.²³ But, 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 (Levenson, 1974; Strickland, 1965), higher self-efficacy (Locke and Latham, 1990) and higher positive core self-evaluation (Elliot et al., 1997). In addition, 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), so it is possible that our intervention will affect such 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 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 echo the groups used in the IPC scale.

Table B.3 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 negatively and significantly correlated with

²²Early 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). Subsequent empirical research finds that beliefs about control are multidimensional and not captured well by this single dimension (Lefcourt, 1991).

²³There is extensive work suggesting correlations between children’s educational and occupational aspirations (using measures similar to this paper), their self-efficacy and locus of control, and their subsequent educational and occupational outcomes, in both psychology (Lent and Hackett, 1987; Holland and Gottfredson, 1975) and economics (Wydick et al., 2013).

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 14 gives treatment effects. For locus of control, those who saw the documentary score significantly higher than the placebo and control groups 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 scale; this seems plausible as they are not just the reverse of the Internality scale. 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 after six months. They are significantly more likely than the control group to agree with items that offer individualistic explanations for poverty. However, the placebo also had a positive effect on the group of individualistic items. 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 regarding aspirations, and the changes in the mindset of people about their ability to affect their own lives. These scales are entirely separately administered, but we would expect 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.

6.3 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 measures capture the presence of various emotions in the individual at a point in time (Diener et al., 2009; Kahneman and Deaton, 2010). Locus of control is an important predictor of life satisfaction (Judge et al., 2002). In addition, there is evidence that individuals with an internal locus of control are more likely to set and pursue goals or aspirations, and that doing so makes them happier and more satisfied (Shah and Higgins, 2001; Elliot et al., 1997). So our intervention might be expected to alter life satisfaction if it alters locus of control and aspirations.

We measure life satisfaction by showing respondents a picture of a ladder with 10 steps (Cantril, 1965). 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.²⁴ We report each measure separately in Table 14. To some extent, the measure may simply captures differences in personality traits, specifically the propensity to be satisfied with life (Schimmack et al., 2002). However, we control for the person’s answer at baseline so that we capture only changes in their subjective assessment since the intervention. We find no significant effects of the treatment on the measure of well-being asking about the best life, or on the average of the two measures. We find a significant positive difference between the treatment group and both the placebo group and the control group on where participants place themselves on a ladder measuring happiness.

²⁴Although the Cantril ladder refers only to the best and worst life, the European Social Survey uses global measures related 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).

7 Corrections for multiple testing

As a final robustness test, 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 Seyoum Taffesse, 2014). However, we have also examined two groups of further outcome variables. First, we have tested for changes in actual behavioural outcomes, for all six basic outcomes on which we collected data, namely 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 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 A.5, we assess the robustness of our results after controlling for the false discovery rate. We rely on the Benjamini et al. (2006) two-stage procedure within families of outcomes. As argued in Anderson (2008), this procedure is less conservative than familywise error rate control procedures, which merely assesses the probability that at least one false discovery is made.²⁵ Further, we do not rely on summary index tests. These procedures are statistically more powerful and are well suited to assess the overall impact of an intervention but give little consideration to each particular outcome. Here, we are more interested in separately testing for the intervention’s impact on all outcomes, whether they relate to shorter or longer term effect on aspirations or expectations, or to various classes of behavioural outcomes.

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 A.5 reports the naive p-value obtained from running each estimate independently, as well as the q-value that accounts for multiple testing within each family. Results based on q-values are qualitatively similar to that obtained from naive p-values. In particular, aspiration-related outcomes remain positive and significant after controlling for false discovery rate, as do those elements of locus of control and perception of poverty related to beliefs that individuals can themselves affect their socio-economic conditions. While inference regarding financial outcomes is no longer statistically significant at a 10% threshold, results related to children’s education remain strongly significant. Overall, this seems to suggest that our results are reasonably robust to the possibility of false discoveries.

8 Peer Effects

8.1 Design and measures

In this section, we test for the presence of treatment spillovers within village communities. As discussed in Section 2, aspirations may well be influenced by peers’ aspirations. Ray (2006), for example, has argued that aspirations are thought to be positively related between members of a given group or “aspiration window”. If true, one may expect second order effects, whereby aspirations and behaviour of individuals are affected by changes in aspirations and consequent behaviour of treated individuals. With expected positive peer effects on individuals in placebo and control groups, 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).

²⁵More conservative familywise error rate control procedures may be more crucial when the implications of a false discovery have large consequences, such as for policy. However, this is not the case in this particular study, which is more exploratory, so we use the less conservative procedure.

It is difficult to identify how an individual’s peer network affects their aspirations, 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 3.3, where an additional 36 individuals per village were exposed to documentaries in half of the villages in our sample, while 36 additional individuals per village were exposed to placebo screening in the other half. Two caveats are in order. First, our experimental design does not include a pure control group, that is villages where no individuals were exposed to treatment. Within budget constraints, we chose to maximise statistical power on direct treatment effect, 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, in villages of 75 households on average.²⁶ If one assumes linearity in peer effects, whereby each additional treated individual contributes to spillovers in a village, this variation may suffice to identify the presence of peer effects.²⁷

We partially account for this limit by capturing variation in the exposure of a respondent’s own network to treatment due to both the individual and the village-level treatment. For this, 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 to the placebo screening. 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. For 93 per cent of the respondents, all four individuals cited lived within the same village, as would be expected given the remoteness of these communities. Only 14 per cent of the respondents listed any of their siblings within the four individuals, suggesting that any peer effect cannot be fully explained by family-level characteristics.

With imperfect correlation between one’s village and one’s social network, this design offers the advantage of generating an almost continuous distribution of network-level intensity of treatment (Baird et al., 2012). Table A.4 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. Similarly, in intense-placebo villages, more of the respondent’s peers had seen the placebo. However, it is also possible that a person in an intense-placebo village may have had all four peers 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 accounts for four peers while individuals may talk to many more

²⁶To minimise effects that may originate in village size, we only sampled villages with 50-100 households.

²⁷However, in the presence of threshold effects, is it unclear whether these village-level variations in treatment intensities lie on either side of a given threshold.

people directly or indirectly exposed to documentary in reality, something accounted for by village-level treatment intensity.

8.2 Specification

We test for the presence of peer effects based on modified versions of Equation 8.1. Specifically, we account for village-level treatment intensities through a dummy variable I_v equal to one for villages where an additional 36 individuals were invited to a documentary session, and zero otherwise.

With a similar specification, we assess the effect of treatment intensities in individual-level network of peers. n_i^T measures the number out of four of the four closest friends listed that have been invited to a documentary session, while n_i^P measures the number of peers invited to a placebo screening. Thus we account for non-independence of observations within village, by clustering all standard errors at the village-level. Our parameters of interest are labelled as δ_2 , the effect of each additional peer being treated onto an individual level of 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 . The probability of having a given number of peers treated is in part determined by village-level treatment intensities, as is clear from Table A.4. We thus cluster standard errors at village-level, to account for non-independence of observations within villages.²⁸ As the treatment is at the village-level, we cannot use village fixed effects as in previous specifications. We use screening site-level fixed effects μ_s to account for site-specific characteristics.

$$y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \lambda I_v + \gamma y_{i1} + \mu_s + X'_{i1} \pi + \eta_i \quad (8.1)$$

One may be concerned that treated individuals within the same peer group react homogeneously to treatment without ever discussing the documentary or observing the ways in which their peers' behaviour changes after watching the documentary. If true, one may observe positive values of δ_2 even if no peer influence occurred. In the terminology of Manski (1993), this is equivalent to a "sorting effect", whereby similar behaviours observed within networks occur because similar individuals self-select into networks and thus behave homogeneously even if not interacting with one another. If this is true, however, one should only observe these effects amongst treated individuals. Thus, we further expand Equation 4.3 to include interactive terms between one's treatment (placebo) status and the number of his/her treated peers, and estimate the corresponding parameters δ_3 and ρ_3 . If equal to zero, these parameters are indication of limited sorting effects - as well as limited additional effect of peers' treatment (placebo) on individuals in the treatment (placebo) group.

$$y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \delta_2 n_i^T + \rho_2 n_i^P + \delta_3 T_i * n_i^T + \rho_3 P_i * n_i^P + \gamma y_{i1} + \mu_s + X'_{i1} \pi + \eta_i \quad (8.2)$$

8.3 Results

We group all peer-related results into one summary table, Table 15, in which results from Equation 8.1 and 8.2 are presented in Panels A and B respectively. Results in the first two columns show some marginally significant evidence of increased aspirations within intense treatment villages, above and beyond direct effects identified in previous sections (p-value = 0.11). No such effects are found in Panel B. Further, we find no clear evidence of any sorting effects as reported by parameter estimates of δ_3 .

In Panel B, we find evidence of increased spending on children's education for those households where a larger share of the household head's peer network was invited to screening (although no effect is present at village level in Panel

²⁸We 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. As Cameron et al. (2008) recommends, 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.

A). No such effect is found in response to increased number of peers invited to a placebo screening. We do not uncover any evidence of sorting effects to which these results may be attributed. In this estimation, attending the documentary increases spending on children’s education by 61 ETB. For each additional peer who attends the screening, spending on children’s education increases by 34 ETB. This is a large peer effect – roughly half the individual treatment effect. There is no evidence that the number of peers treated affects the number of children enrolled in school.

This is some of the first work in a developing country experimental setting to examine peer effects among parents and on educational spending. There is other evidence that children whose peers are more likely to be attending school are also more likely to attend school. In the PROGRESA programme in Mexico, in randomly selected villages, poor families were eligible to receive a cash grant per child 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 per cent 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 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).

There is also consistent evidence that households in high-intensity villages increased their time dedicated to income generating activities and decreased that which was dedicated to leisure. Although non-statistically significant, point estimates of parameters δ_2 and ρ_2 for these outcome variables give further support to the village-level finding. There is no clear evidence of peer effects onto financial indicators related to savings and credit. Lastly, there is no clear evidence of any sorting effects as reported by parameter estimates of δ_3 and ρ_3 . Overall, while weakly identified, the results provide some support to peers’ influence in affecting behavioural outcomes.

9 Conclusion

This paper has attempted to test whether aspirations and future-oriented behaviour can be altered using an innovative experimental design. The experiment involved exposure to a one-hour documentary in which four people from similar backgrounds to the audience tell their life story of how they improved their economic status. Our results point to significant improvements in individuals’ aspirations measured six months later. Results are robust to a symmetrical placebo experiment and alternative measures of attitudes towards the future. We also find that treatment effects are stronger for those individuals with above-median baseline aspirations. They do not differ by gender or level of education, however. Although none of the documentaries featured success related to education, we find the most significant effect on individuals’ revision of their aspiration vis-à-vis their children’s education.

We also assess the reduced-form effect of our intervention on individuals’ actual behaviour. We find consistent evidence that being invited to a documentary screening has directly impacted individuals’ use of financial tools related to both savings and credit. This is further supported by a positive treatment effect on a hypothetical demand for loan question. We also find strong effects on the number of children enrolled in school and total spending on children’s education.

We find some evidence that effects on aspirations are in part mediated through the number of peers also exposed to treatment, and presumably by the discussions one has had with these peers. Together, these results give support to the hypothesis set forth by Appadurai (2001) and Ray (2006) that aspirations, although an individual attribute, respond to collective influence.

As a side contribution, these results further confirm findings by a recent and growing empirical literature on the effectiveness of video-based interventions in affecting perceptions and behaviours (see for instance 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 conflict resolution and inter-group tolerance in Rwanda). In terms of policies and program designs, our results call for an increased attention to the role of aspirations and the mechanisms underlying their formation.

Are we giving false hope? We cannot judge this. But we did not tell or suggest to individuals – rightly or wrongly – what path will lead them out of poverty, unlike most interventions and intervention-based studies that offer ‘solutions’ in microcredit, health, education and more. We only made our treatment group listen to stories told by their peers from similar backgrounds. The extent and nature of their response has surprised us, and asks further serious questions about the nature of poverty and how it affects people, as well as what may work in reducing poverty.

Table 1: Experimental design

	All villages	Intense treatment villages	Intense placebo villages
# individuals surveyed	64 2,063	32 1,034	32 1029
of which:			
Treatment individuals	675	337	338
Placebo individuals	702	354	348
Control individuals	686	343	343

This sample is used for analysis. It excludes 52 individuals who were given tickets but were not surveyed in Round 2 or were missing a questionnaire.

Table 2: Compliance and attrition

		Surveyed				Not surveyed		
		Treated	Placebo	Control	Total	Treated	Placebo	Total
Total sample drawn		769	768	767	2,304	1,154	1,151	2,305
	Household head's spouse dead or divorced	38	27	30	95	56	62	118
	Head or spouse not given ticket	41	24	30	95	8	3	11
Given tickets		691	717	707	2115	1090	1086	2176
Non-compliers	Missed screening	15	9	0	24	43	28	71
Non-compliers	Attended incorrect screening	3	11	6	20	0	0	0
Compliers	Arrival at correct screening not recorded ¹	11	10	25	46	45	62	107
Compliers	Arrival at correct screening recorded ²	662	688	673	2,023	1,002	996	1,998
Compliance rate ³		0.958	0.960	0.952	0.957	0.919	0.917	0.918
Missed Round 2		15	15	19	48	-	-	
Missing q'naire		1	0	3	4	-	-	0
Attrition rate ⁴		0.022	0.021	0.027	0.023	-	-	
Sample in this paper	Given tickets minus attriters	675	702	685	2,063	-	-	

¹ Some individuals were not recorded as arriving at the correct location, but were also not recorded as absent or attending the wrong screening.

² For treatment and placebo individuals were at the correct location if they were recorded arriving at the correct screening. Control group individuals were not invited to a screening, so they were at the correct location if they completed a follow-up interview at their home while the screenings were going on.

³ The compliance rate is the proportion of those given tickets who were recorded arriving at the correct screening.

⁴ The attrition rate is the proportion of those surveyed and given tickets who were not found in Round 2.

Table 3: Experimental integrity: balance tests on variables at baseline

	N	Total	Treatment	Placebo	Control	T-C	P-C
		Mean (Standard Deviation)				Difference p-value	
Age	2063	36.82 (12.43)	36.93 (11.44)	36.94 (13.17)	36.57 (12.61)	0.580	0.593
Male	2063	0.49 (0.50)	0.50 (0.50)	0.49 (0.50)	0.49 (0.50)	0.896	0.838
Highest grade attained	2063	0.30 (0.46)	0.32 (0.47)	0.29 (0.45)	0.29 (0.45)	0.209	0.878
Single, widowed or divorced	2063	0.057 (0.23)	0.074 (0.26)	0.050 (0.22)	0.047 (0.21)	0.034**	0.780
Total value of household assets (ETB)	2063	7,097.62 (8,664.73)	7,822.83 (10,888.32)	6,381.25 (6,914.02)	7,117.11 (7,706.06)	0.167	0.061*
Food secure	2063	0.20 (0.40)	0.23 (0.42)	0.20 (0.40)	0.18 (0.39)	0.091*	0.148
Food insecure but no hunger	2063	0.17 (0.38)	0.17 (0.38)	0.15 (0.36)	0.20 (0.39)		
Food insecure and hunger	2063	0.58 (0.49)	0.55 (0.50)	0.6 (0.49)	0.58 (0.49)		
Watches TV > once a month	2059	0.10 (0.30)	0.11 (0.31)	0.09 (0.29)	0.11 (0.31)	0.936	0.387
Listens to radio > once a month	2059	0.61 (0.49)	0.62 (0.46)	0.59 (0.49)	0.63 (0.48)	0.833	0.218
Travels outside district > once a month	2049	0.13 (0.34)	0.15 (0.36)	0.12 (0.33)	0.14 (0.35)	0.619	0.247
Has lived outside district	2028	0.10 (0.30)	0.11 (0.31)	0.08 (0.27)	0.10 (0.30)	0.653	0.139

*p below 0.1, **p below 0.05, ***p below 0.01. Standard deviations in parenthesis. We use a chi-squared test to examine whether there are significant differences between treatment and placebo or treatment and control groups in the distribution of households across the three categories of food insecurity.

We capture the total value of a household's assets by asking households how many of a list of assets they own and what they estimate the resale value of the assets to be. Assets include tools, furniture, electrical goods and modes of transport like carts and bicycles. We do not include land or houses in the assets measure.

For the food insecurity measure, the household head is asked whether they worry about running out of food, with items such as (1) "We worried whether our food would run out before we got money to buy more"; (2) "The food that we bought just didn't last, and we didn't have money to get more"; (3) "We relied on only a few kinds of low-cost food (e.g. no vegetables or meat) to feed the children because we were running out of money to buy food" and (4) "We had to eat some food we did not want to eat because we could not afford to buy other food (e.g. wild food, immature crops, discarded food)". It also asks if households had actually suffered from hunger, with items asking if adults or children had cut portion sizes, skipped meals or gone for a whole day without food because there was not enough money for food. The measure provides two thresholds used to classify households into the three categories.

Table 4: Experimental integrity: aspirations at baseline

	N	Total	Treatment	Placebo	Control	T-C	P-C
		Mean (Standard Deviation)				Difference p-value	
<i>Aspirations: level you'd like to achieve</i>							
Income	2047	146,057.00 (609,111.30)	154,779.70 (723,021.00)	154,403.70 (671,263.50)	128,907.80 (375,094.50)	0.409	0.386
Wealth	2049	152,577.10 (2,841,719.00)	59,837.65 (161,201.10)	205,533.10 (3,789,935.00)	189,945.70 (3,097,618.00)	0.277	0.933
Children's education	2015	12.91 (1.71)	12.96 (1.65)	12.89 (1.53)	12.87 (1.93)	0.339	0.790
Social status	2039	75.00 (31.92)	73.71 (32.91)	75.71 (31.30)	75.57 (31.58)	0.289	0.935
Index	2058	0.03 (0.56)	0.02 (0.46)	0.04 (0.61)	0.02 (0.59)	0.988	0.591
<i>Expectations: level you think you'll reach in 10 years</i>							
Income	2045	33,081.82 (231,346.50)	27,197.16 (55,606.32)	43,592.16 (38,6524.10)	28,066.72 (67,456.83)	0.797	0.303
Wealth	2049	28,073.07 (70,490.70)	27,547.24 (57,320.66)	29,295.88 (80,858.20)	27,339.65 (70,948.62)	0.953	0.634
Children's education	1936	12.43 (2.52)	12.55 (2.41)	12.33 (2.47)	12.41 (2.66)	0.338	0.553
Social status	2040	70.95 (29.30)	70.03 (30.42)	71.57 (28.16)	71.23 (29.35)	0.462	0.824
Index	2055	0.02 (0.56)	0.02 (0.44)	0.03 (0.65)	0.02 (0.56)	0.940	0.675

*p below 0.1, **p below 0.05, ***p below 0.01. Standard deviations in parenthesis.

The aspirations measure asked what level respondents would like to achieve on each of four dimensions: income, wealth, social status and children's educational attainment. The expectations measure asked what level respondents thought they would reach in 10 years. Income 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. Social status was measured as the percentage of community members who asked for their advice at times of important decisions. We asked respondents to weight the four dimensions according to their own assessment of the 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 index. The individual components of aspirations and expectations reported in this table are unstandardised.

Table 5: Experimental integrity: outcome variables at baseline

	N	Total	Treatment	Placebo	Control	T-C	P-C
		Mean (Standard Deviation)				Difference p-value	
<i>Baseline outcome variables at individual level</i>							
Average daily time in work (minutes)	1961	348.72 (219.28)	351.56 (210.96)	349.46 (223.05)	345.15 (223.69)	0.596	0.726
Average daily time in leisure (minutes)	1961	740.14 (182.58)	735.64 (174.30)	734.97 (186.48)	749.90 (186.37)	0.156	0.146
Has any cash savings	2063	0.22 (0.42)	0.25 (0.43)	0.21 (0.41)	0.02 (0.41)	0.088	0.876
Total savings	2057	82.97 (544.84)	105.76 (675.66)	73.20 (516.46)	70.60 (413.81)	0.247	0.918
Has taken out credit in last year	2063	0.43 (0.50)	0.45 (0.50)	0.45 (0.50)	0.40 (0.49)	0.116	0.081*
Amount of credit	2063	176.17 (330.40)	188.80 (354.40)	181.03 (320.36)	158.78 (315.46)	0.099*	0.193
Subjective discount factor	2039	0.54 (0.32)	0.55 (0.32)	0.54 (0.31)	0.55 (0.31)	0.888	0.575
Risk aversion (coin toss)	2037	0.99 (1.00)	1.06 (1.01)	0.95 (1.00)	0.96 (0.99)	0.074*	0.836
Risk aversion (market)	2026	1.22 (1.14)	1.16 (1.1)	1.26 (1.15)	1.22 (1.15)	0.329	0.56
<i>Baseline outcome variables at household level</i>							
Number of children 7-15 in household	1138	1.88 (1.51)	2.06 (1.53)	1.85 (1.48)	1.73 (1.49)	0.003***	0.261
Number of children 7-15 in school	1124	1.03 (1.10)	1.15 (1.14)	0.97 (1.06)	0.98 (1.10)	0.044*	0.894
Expenditure on children's schooling	1077	197.22 (289.27)	227.30 (320.63)	194.24 (285.24)	170.37 (256.62)	0.007***	0.227
Hypothetical loan repayable in 1 year	1131	6,079.67 (7,320.05)	5,757.03 (6,460.61)	5,808.53 (7,186.63)	6,668.87 (8,187.66)	0.090*	0.126
Hypothetical loan repayable in 5 years	1130	12,905.69 (17,465.41)	11,718.03 (13,599.74)	13,634.80 (21,330.40)	13,356.74 (16,556.08)	0.138	0.841
Hypothetical loan repayable in 10 years	1122	23,118.15 (35,907.51)	19,363.63 (22,588.51)	25,637.97 (46,545.85)	24,300.05 (34,196.06)	0.020**	0.653

*p below 0.1, **p below 0.05, ***p below 0.01. Standard deviations in parenthesis.

Savings is the total savings each individual respondent has in all possible savings places. 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) and not including any interest payments. To measure discount factors, respondents were asked to choose between a gift of 100 ETB immediately and another amount in one month. The subjective discount factor is the value today of 1 ETB received in future.

Discount rates are calculated from the amount respondents required to choose to wait one month to receive it. To capture risk aversion, we used Binswager (1980) lotteries, described in Appendix Table B.1. The number of children in the household between 6 and 15 includes all children resident in the house, including those who were not the children of the household head or their spouse. 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. At endline, we asked about the number enrolled at the start of the 2010/11 school year. We examine all households in the sample, including 107 households who have no children in this age group in the household in both rounds, to ensure the sample is comparable with other results. To measure school expenses we asked the household head about all spending for children in the household in the previous school year on uniforms, stationery, textbooks and school fees. The hypothetical demand for credit variable asked household heads "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?; (2) How much would you ask for if the loan is payable in 5 years? (3) How much would you ask for if the loan is payable in 10 years?"

Table 6: Aspirations and expectations indices straight after screening and after six months

	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	0.12	0.10	0.10	0.08
P: Treated-Placebo	0.04	0.04	0.05	0.03	0.03	0.08
	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	0.03
	(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	0.00	0.03	0.03	0.02
P: Treated-Placebo	0.60	0.51	0.82	0.11	0.10	0.22
Difference in post-screening and six-month treatment effects						
P: Treated _(t=1) = Treated _(t=2)	0.165	0.164	0.165	0.238	0.233	0.279
P: (Treated-Placebo) _(t=1) = (Treated-Placebo) _(t=2)	0.064*	0.070*	0.063*	0.149	0.163	0.235

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level in parenthesis. Controls: age, gender, highest grade attained, marital status, household assets, food insecurity. The aspirations measure asked what level respondents would like to achieve on each of four dimensions: income, wealth, social status and children's educational attainment. The expectations measure asked what level respondents thought they would reach in 10 years. The indices are in standard deviations. 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 includes the lag and controls. The last panel tests if the difference between the treatment and control and the difference between the treatment and placebo is significantly different between the straight after screening measurement and the measurement after six months. The second last row tests $\delta_{1,t=1} = \delta_{1,t=2}$. The last row tests $\delta_1 - \rho_{1,t=1} = \delta_1 - \rho_{1,t=2}$. 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 7: 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***
N answered question	638	668	
N 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 in parentheses. The last column gives the p value of the difference between the treatment and placebo group.

Table 8: Treatment effects on components of aspirations index

	Aspirations index	Income	Wealth	Education	Social status
Treated individual	0.03* (0.02)	3949.01 (12334.33)	-4573.58 (4336.40)	0.16* (0.09)	0.65 (1.32)
Placebo individual	0.03 (0.02)	11682.79 (12178.91)	-852.72 (4289.86)	0.09 (0.09)	1.01 (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	0.00	-7733.78	-3720.87	0.07	-0.36
P: Treated-Placebo	0.81	0.53	0.39	0.48	0.79

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level in parenthesis. Controls: age, gender, highest grade attained, marital status, household assets and food insecurity. This table reports on the aspirations index, which asked what level respondents would like to achieve on each of four dimensions. The index is in standard deviations. Income and wealth were measured in Ethiopian Birr (ETB). Education was measured in the years of schooling the respondent wished their eldest child to complete. Social status was measured as the percentage of community members who asked for the respondent's advice at times of important decisions.

Table 10: Savings and use of credit

	Has savings			Total savings		
Treated individual	0.05* (0.03)	0.03 (0.02)	0.03 (0.02)	122.56** (61.64)	106.19* (54.42)	89.02* (51.45)
Placebo individual	0.01 (0.02)	0.01 (0.02)	0.01 (0.02)	-1.84 (49.38)	-13.85 (44.85)	16.88 (41.50)
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	72.13
P: Treated-Placebo	0.12	0.33	0.50	0.04	0.02	0.15

	Took out credit			Total credit		
Treated individual	0.03 (0.03)	0.03 (0.02)	0.03 (0.02)	22.35* (11.75)	19.11* (11.61)	21.60* (11.57)
Placebo individual	-0.01 (0.03)	-0.02 (0.02)	-0.02 (0.02)	5.44 (11.74)	3.07 (11.52)	2.58 (11.40)
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	19.02
P: Treated-Placebo	0.12	0.10	0.04	0.18	0.20	0.12

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level in parenthesis. Controls are for age, gender, highest grade attained, marital status, household assets and food insecurity. Savings is the total savings each individual respondent has in all possible savings places. For credit questions, we asked whether respondents had taken out any loans larger than 15 ETB. 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) and not including any interest payments.

Table 9: 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)	-0.00 (0.04)	0.03 (0.02)
Treated*Above median aspirations	0.28*** (0.11)	0.06** (0.03)								
Treated*Below median age			0.06 (0.18)	0.01 (0.03)						
Treated*Male										
Treated*Highest grade			-0.09 (0.14)	-0.02 (0.03)			0.04 (0.03)	-0.01 (0.01)		
Treated*Above median assets									0.24* (0.13)	-0.03 (0.03)
Village F.E.	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}
Lagged term	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}
Controls	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}	Y_{es}
Respondents	1957	2058	1957	2058	1957	2058	1957	2058	1957	2058
Control group mean	0.03									
Difference in post-screening and six-month treatment effects										
P: Treated _(t=1) =	0.852		0.288		0.246		0.363		0.361	
Treated _(t=2)										
P: Treated* $Z_{(t=1)}$ =	0.040**		0.781		0.620		0.121		0.033**	
Treated* $Z_{(t=2)}$										

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level in parenthesis. This table reports on the aspirations index, which asked what level respondents would like to achieve on each of four dimensions. 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. Regressions control for age, gender, highest grade attained, marital status, household assets and food insecurity.

The last panel tests if the treatment effect is significantly different between the straight after screening measurement and the measurement after six months. The second last row tests $\delta_{1,t=1} = \delta_{1,t=2}$. The last row tests $\theta_{2,t=1} = \theta_{2,t=2}$. 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. Controls: age, gender, highest grade attained, marital status, household assets and food insecurity.

Table 11: Hypothetical demand for credit, if loan is repayable

	In one year			In five years			In ten years		
Treated individual	868.15 (1245.80)	1354.42 (1187.44)	827.89 (1194.54)	2127.78 (2287.84)	2712.78 (2143.30)	2298.35 (2101.48)	6211.66** (3090.15)	7057.54** (2917.12)	6699.43** (2918.84)
Placebo individual	1399.06 (1245.49)	1913.33 (1188.61)	1665.35 (1187.52)	-174.15 (2287.08)	-722.98 (2139.59)	-502.00 (2081.69)	-1424.10 (3099.60)	-2314.95 (2912.86)	-2429.63 (2889.34)
Village F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged outcome	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Households	1131	1125	1075	1126	1119	1069	1114	1100	1052
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	-558.92	-837.46	2301.93	3435.76	2800.35	7635.77	9372.49	9129.06
P: Treated-Placebo	0.67	0.64	0.48	0.31	0.11	0.18	0.01	0.00	0.00

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors in parenthesis. Outcome is at household level. Controls are for age, gender, marital status and highest grade attained of household head, household assets and food insecurity. We asked household heads "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?; (2) How much would you ask for if the loan is payable in 5 years? (3) How much would you ask for if the loan is payable in 10 years?"

Table 12: Investment in children's education

	Children 6-15 enrolled			Education spending		
Treated individual	0.28*** (0.09)	0.17** (0.07)	0.19*** (0.07)	60.73*** (21.62)	31.59 (19.52)	33.83* (19.76)
Placebo individual	0.08 (0.09)	0.10 (0.07)	0.12* (0.07)	31.72 (21.59)	20.15 (19.36)	25.94 (19.46)
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	1123	1082	1128	1118	1068
Control group mean	1.23	1.23	1.23	197.42	197.42	197.42
Treated-Placebo	0.20	0.07	0.07	29.01	11.44	7.89
P: Treated-Placebo	0.02	0.31	0.27	0.18	0.56	0.69

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors in parenthesis. Outcome is at household level. Controls are for age, gender, marital status, highest grade attained of household head, household assets and food insecurity, as well as time taken to travel to the nearest primary school. The number of children in the household between 6 and 15 includes all children resident in the house, including those who were not the children of the household head or their spouse. 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. At endline, we asked about the number enrolled at the start of the 2010/11 school year. We examine all households in the sample, including 107 households who have no children in this age group in the household in both rounds, to ensure the sample is comparable with other results. To measure school expenses we asked the household head about all spending for children in the household in the previous school year on uniforms, stationery, textbooks and school fees.

Table 13: Time discounting, self-control 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	-0.11
P: Treated-Placebo	0.32	0.31	0.28	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 in parenthesis. Controls are for age, gender, marital status, highest grade attained, household assets and food insecurity. To measure discount factors, respondents were asked to choose between a gift of 100 ETB immediately and another amount in one month. The subjective discount factor is the value today of 1 ETB received in future. Discount rates are calculated from the amount respondents required to choose to wait one month to receive it. To capture risk aversion, we used Binswager (1980) lotteries, described in Appendix Table B.1.

Table 14: Locus of control, perceptions of causes of poverty and life satisfaction

	Locus of control			Causes of poverty			Wellbeing	
	Chance	Others	Internality	Fate	Structural	Individual	Best life	Happiest life
Treated individual	0.01 (0.16)	-0.04 (0.18)	0.33** (0.15)	-0.38* (0.20)	0.11 (0.12)	0.41** (0.16)	0.01 (0.11)	0.20* (0.12)
Placebo individual	-0.02 (0.15)	0.01 (0.17)	-0.03 (0.15)	-0.02 (0.20)	0.17 (0.12)	0.34** (0.16)	0.11 (0.10)	0.01 (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	2022	2045	2031	1999	2055	2037
Control group mean	13.35	12.62	16.19	9.86	8.29	11.48	4.85	6.86
Treated-Placebo	0.03	-0.05	0.37	-0.36	-0.06	0.06	-0.10	0.20
P: Treated-Placebo	0.86	0.75	0.02	0.08	0.64	0.70	0.35	0.10

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level in parenthesis. Controls are for age, gender, marital status, highest grade attained, household assets and food insecurity. The highest score is 20 for locus of control scales (strongly agree with all five items) and 16 for perception of poverty scales (strongly agree with all four items). The measures are described in B.1.3. We measure life satisfaction by showing respondents a picture of a ladder with 10 steps (Cantril, 1965). 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.

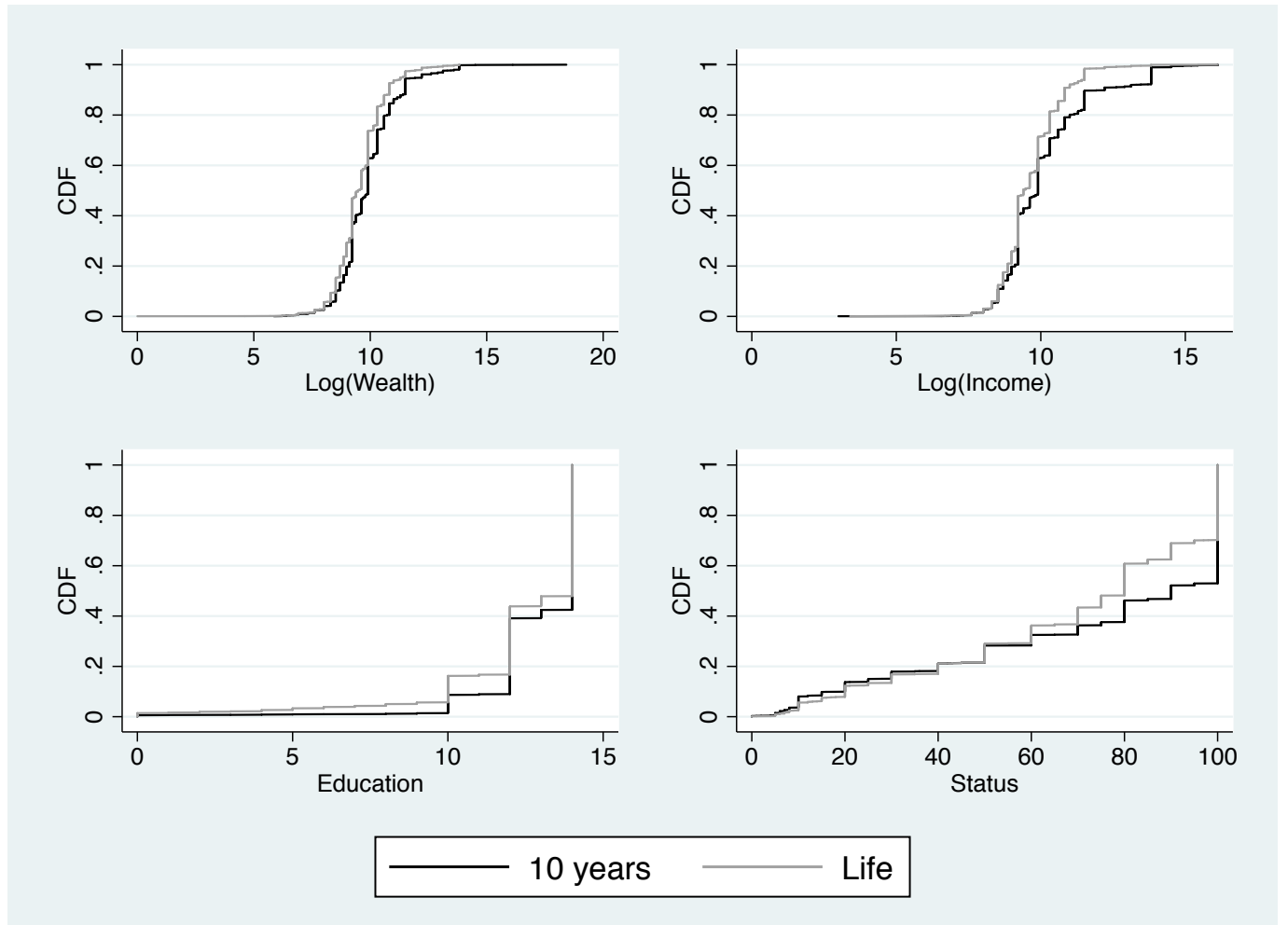
Table 15: Peer effects

	After 6 months			Time allocation		Savings and credit		Children's education		
	Aspirations	Expectations	Time in work	Time in leisure	Total Savings	Total Credit	Children 6-15 enrolled	Educational spending		
<i>Panel A: Village-level treatment</i>										
Individual treatment	0.04* (0.02)	0.06* (0.02)	5.7 (8.46)	3.92 (12.21)	87.52 (55.01)	21.47* (11.86)	0.17** (0.06)	37.84 (22.98)		
Village-level treatment intensity	0 (0.04)	-0.01 (0.06)	14.39* (7.67)	-19.94* (10.85)	10.54 (37.06)	-2.01 (9.82)	0.03 (0.06)	6.73 (16.59)		
<i>Panel B: Peer-level treatment</i>										
Individual treatment	0.04 (0.02)	0.07** (0.03)	11.38 (10.21)	1.19 (15.56)	124.54 (77.42)	16.72 (16.57)	0.15* (0.08)	60.88** (25.88)		
Peer treatment	0.01 (0.02)	0.02 (0.02)	8.05 (6.34)	-2.63 (8.67)	14.05 (28.92)	-1.02 (9.04)	0.01 (0.05)	33.88 (18.12)		
Individual treatment*	0	-0.02 (0.02)	-6.65 (8.79)	3.1 (12.51)	-46.85 (65.55)	6.02 (13.61)	0.03 (0.07)	-27.17 (25.76)		

*p below 0.10 **p below 0.05 ***p below 0.01. Village-level clustered standard errors in parenthesis. All estimates include individual placebo treatment. Panel 2 estimates also include peer placebo and interaction between peer placebo and individual placebo. All estimates include controls for age, gender and highest grade attained of household head, household assets, food insecurity, screening site-level fixed effects and lagged outcome variables. Regressions on education outcomes control for the time to travel to the nearest primary school.

Appendix A

Figure A.1: 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 blue) and aspirations (in red) 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 they are used in the index. We do not examine whether aspirations are higher than expectations for each individual on each dimension, because there may be measurement error at individual level. Examining the whole distribution is thus more appropriate.

Table A.1: Baseline correlates of aspirations index and components of aspirations index

	Aspirations index	Income	Wealth	Education	Social status
Age	0.00 (0.00)	-689.27 (1393.01)	4481.13 (3507.54)	0.00 (0.00)	0.10* (0.06)
Male	0.17*** (0.02)	125230.33*** (28661.21)	49120.66 (44836.66)	0.37*** (0.08)	7.02*** (1.14)
Total value of household assets (ETB)	0.00*** (0.00)	3.75*** (1.34)	2.84 (3.85)	0.00** (0.00)	-0.00 (0.00)
Highest grade completed	0.02*** (0.01)	1059.18 (4485.97)	73045.44* (43888.23)	0.09*** (0.01)	1.00*** (0.29)
Single, widowed or divorced	-0.02 (0.04)	-32071.38 (29500.15)	-23909.10 (56323.32)	-0.10 (0.22)	-1.94 (2.58)
HH food insecure no hunger	0.05 (0.04)	96170.52* (53014.84)	296341.31 (277595.24)	0.05 (0.13)	-2.64 (2.34)
HH food insecure and hunger	0.02 (0.03)	7394.72 (33208.05)	9306.56 (106719.41)	-0.14 (0.12)	4.73** (1.85)
Constant	-0.16** (0.08)	25928.90 (70795.96)	-304786.04 (228967.99)	12.07*** (0.42)	79.10*** (3.88)
Village F.E.	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Respondents	1963	1954	1954	1920	1944
Control group mean	0.02	128907.79	189945.66	12.87	75.57

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level 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.3: Time (in minutes) in work and leisure

	Time in farm work			Time in leisure		
Treated individual	7.16 (7.82)	1.83 (8.68)	5.43 (7.35)	5.82 (10.59)	9.99 (10.82)	5.20 (10.58)
Placebo individual	-1.31 (8.30)	-8.83 (9.38)	-1.99 (8.02)	6.66 (11.23)	14.90 (11.56)	10.87 (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	7.42	-0.85	-4.91	-5.67
P: Treated-Placebo	0.29	0.23	0.34	0.94	0.66	0.61

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level in parenthesis. Controls: age, gender, highest grade attained, household assets, food insecurity and marital status.

Table A.4: 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>	<i>2,063</i>			
No peer has seen documentary	948	45.95	25.63	66.38
1 peer has seen documentary	670	32.48	37.62	27.31
2 peers have seen documentary	331	16.04	26.6	5.44
3 peers have seen documentary	97	4.7	8.7	0.68
4 peers have seen documentary	17	0.82	1.45	0.19
<i>Distribution of peer-level placebo</i>	<i>2,063</i>			
No peer has seen placebo	991	48.04	69.44	26.53
1 peer has seen placebo	659	31.94	24.76	39.16
2 peers have seen placebo	327	15.85	5.42	26.34
3 peers have seen placebo	71	3.44	0.39	6.51
4 peers have seen placebo	15	0.73	0	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 A.2: Pairwise correlations at baseline between aspirations 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	0.09***	1.00

*p below 0.1, **p below 0.05, ***p below 0.01. 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. 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.5: Correction for multiple testing

Family	Outcome	<i>n</i>	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	0.04	0.053
	Aspirations index after six months	2058	0.03	0.09	0.072
	Expectations index after six months	2054	0.05	0.01	0.042
Time allocation	Time in farm work	1950	5.43	0.46	1
	Time in leisure	1950	5.2	0.62	1
Credit and savings	Took out credit	2063	0.03	0.17	0.191
	Total credit	2044	21.6	0.06	0.191
	Has savings	2063	0.03	0.27	0.191
	Total savings	2051	89.02	0.08	0.191
Hypothetical demand for credit	Loan repayable in 1 year	1075	827.78	0.48	0.471
	Loan repayable in 5 years	1069	2298.35	0.27	0.37
	Loan repayable in 10 years	1052	6699.43	0.02	0.064
Children's education	Children 6-15 enrolled	1082	0.19	0.005	0.011
	Education spending	1068	33.83	0.08	0.042
Self-concept	Locus of control: Chance	2008	0.01	0.94	0.887
	Locus of control: Others	2035	-0.04	0.81	0.887
	Locus of control: Internality	2022	0.33	0.02	0.087
	Perceptions of causes of poverty: Fate	2045	-0.38	0.06	0.137
	Perceptions of causes of poverty: Structural	2031	0.11	0.36	0.405
	Perceptions of causes of poverty: Individual	1999	0.41	0.01	0.087
	Well-being: Best life	2055	0.01	0.94	0.887
	Well-being: happiest life	2037	0.2	0.1	0.177
Time and risk preferences	Discount rate	2037	-0.01	0.71	0.899
	Risk aversion : coin	2035	-0.09	0.12	0.563
	Risk aversion: market	2024	-0.03	0.61	0.899

We use the Benjamini et al. (2006) procedure within seven families of outcome variables. The table reports the naive p-value obtained from running each estimate independently, which is reported in the other tables in the paper. These estimates control for age, gender and highest grade attained of household head, household assets, food insecurity, village fixed effects and the lagged outcome variable. Regressions on education outcomes control for the time to travel to the nearest primary school. Standard errors for individual-level outcomes are clustered at household level. We also report the q-value that accounts for multiple testing within each family.

Appendix B

B.1 Risk and time preferences and psychosocial measures

B.1.1 Rate of time preference

To construct individual subjective discount factors, we use a survey-based measurement tool, as in Cole et al. (2013) in India and Hill et al. (2011) in Ethiopia. The scale and logistics of the study meant that a survey-based tool was chosen over an experimental tool. We find very similar distributions on these measures to Hill et al. (2011) for Ethiopia.

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 today of 1 ETB received in future. We asked if respondents would prefer to receive 100 ETB now or 125 ETB in one month. Those who chose 125 ETB have a monthly discount factor between 1 and 0.8. In other words, for these individuals, one ETB in one month is worth between 0.8 and 1 ETB today. We assign them the mid-point of 0.9. Those who chose 100 ETB were asked if they would prefer 100 ETB now or 150 ETB in one month. Those who chose 150 ETB have a monthly discount factor between 0.8 and 0.667, so we assign them the midpoint of 0.733. We then ask those who have a discount factor lower than 0.667 how much they would need to be given in one month to choose to wait.²⁹

We report individual discount rates at baseline in Table 5. The mean subjective discount factor at baseline in the data is 0.54. Discounting is relatively high: this could reflect 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). Baseline occurred towards the end of the rainy, hungry season and endline at planting time, but unlike Duflo et al. (2008) we find no significant difference between discount rates at baseline and endline ($p=0.213$). We find no significant differences in discount rates between treatment, placebo and control groups.

B.1.2 Risk aversion

To calculate risk preferences, we use survey-based instrument following the line of enquiry by Binswanger (1980) and in line with Cole et al. (2013) and Hill et al. (2011). We used two sets of questions on hypothetical decisions. The first, as in Cole et al. (2013), listed five possible payouts they could receive if the payout was determined by a coin toss and asked them to choose which they would prefer. The lotteries offered increased in both mean and variance. The second question, as in Hill (2009), asked about the amount of price risk individuals would choose when selling surplus grain output and had the same structure of payouts but multiplied by 100. In both cases, payouts were ordered from most to least risk averse. Both have also been used in Ethiopia before and the distribution of individuals across categories is very similar to that in the Ethiopian Rural Household Survey (Hill et al., 2011). The use of a constant probability for each payout, as in a coin toss, is simple to explain to respondents.

As in Hill (2009) and Binswanger (1980), we use 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, which is fixed regardless of the level of payoff, and M is the certainty equivalent of a given lottery. Appendix Table B.1 indicates the mean and variance of each lottery, and the risk preference parameters that would be associated with each choice under the assumption of the specific expected utility functional form. We follow how Hill (2009) scales this coefficient to assign risk aversion “numbers” to the discreet classes, as shown in Appendix Table B.1.

Table 5 shows that the mean coefficient of partial risk aversion at baseline is respectively 1.216 (market) and 0.988 (coin toss). At baseline, respondents are significantly more risk averse when answering the question about maize prices fetched at market ($p=0.000$). This is to be expected, as the payouts are larger on the maize question and higher stakes are often associated with more risk-averse behaviour (Holt and Laury, 2002). We show tests for balance in risk preferences at baseline in Appendix Table B.2, and conclude that the distribution of individuals across categories is largely balanced for both measures of risk preference.

²⁹This 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 combat these problems (Andersen et al., 2008; Andreoni and Sprenger, 2012). Given the limited numeracy of our respondents, and the experience in other Ethiopian surveys, we refrained from using this measure. We also run results on the log of the discount factor but find no difference in the magnitude, sign or significance of coefficients.

Table B.1: Structure of payoffs in risk aversion lotteries

(1) n	(2) Payouts (coin toss)	(3) Heads Tails	(4) Expected value	(5) Std. dev.	(6) $\Delta E/\Delta SD$	(7) Risk aversion	(8) S compatible with choice	(9) Value given
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

To capture risk aversion, individuals were presented with two hypothetical decisions. The first asked respondents which of five payouts they would choose if the payout was determined by a coin toss. The payout options are shown in Column 2 and 3. The second asked about the amount of price risk individuals would choose when selling surplus grain output and had the same structure of payouts but multiplied by 100. Column 8 shows the range for the coefficient of partial risk aversion, calculated from a constant partial risk aversion utility function of the form $U = (1 - S)M^{1-S}$, that corresponds to each of the five payouts. To calculate the value for our measure of risk aversion corresponding to each payout (in column 9), 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.

Table B.2: 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. Standard deviations in parenthesis. Options 1 to 5 correspond to the choices in Table B.1. 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.

B.1.3 Locus of control and attributions for poverty

The concept of 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.

We used a selection of items from each scale, omitting those which were not appropriate to the context. Our Internality scale, with Cronbach’s alpha at baseline=0.753, includes: “When I make plans, I am almost certain/guaranteed/sure to make them work”, “I am usually able to protect my personal interests”, “When I get what I want, it’s usually because I worked hard for it” and “My life is determined by my own actions”. Our Powerful Others scale, with Cronbach’s alpha at baseline=0.744, includes: “I feel like what happens in my life is mostly determined by powerful people”, “My life is chiefly controlled by other powerful people”, “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” and “In order to have my plans work, I make sure that they fit in with the desires of people who have power over me”. Our Chance scale, with Cronbach’s alpha at baseline=0.676, includes: “To a great extent my life is controlled by accidental/chance happenings”, “Often there is no chance of protecting my personal interests from bad luck happenings”, “When I get what I want, it’s usually/mostly because I’m lucky”, “My experience in my life has been that what is going to happen will happen” and “It’s not always wise for me to plan too far ahead because many things turn out to be a matter of good or bad fortune”.

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) and not the shorter version 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.

In particular, we measure Individualistic items (Cronbach’s alpha at baseline=0.703) using: “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" and "They are not motivated because of food aid (e.g. direct support programme, food parcels)". The original scale refers to welfare rather than food aid. Fatalistic items (Cronbach's alpha at baseline=0.898): "They have bad fate/destiny", "They lack luck", and "They have encountered misfortunes". We dropped the item "They are born with less talent/they are less gifted" because it was poorly translated and did not cluster closely with the other three items in factor analysis. Structural items (Cronbach's alpha at baseline=0.626): "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" and "They lack opportunities due to the fact that they come from poor families".

Table B.3 shows correlations between these variables and our measures of aspirations. As expected, there are positive correlations between higher aspirations and expectations, having an internal locus of control, and agreeing with the idea that individual behaviour results in poverty (all correlations except between aspirations and the idea that individual behaviour causes poverty). Aspirations and expectations are negatively correlated with attributing poverty to fate, and negatively (but not significantly) correlated with agreement with attributing life outcomes to chance in the locus of control scale.

Table B.3: Pairwise correlations at baseline between aspirations and psychosocial measures

	Aspirations and Expectations		Locus of control			Causes of poverty			Wellbeing	
	Aspirations	Expectations	Chance	Others	Internality	Fate	Structural	Individual	Best life	Happiest life
Aspirations	1.00									
Expectations	0.33***	1.00								
<i>Locus of control</i>										
Internality	0.10***	0.16***	1.00							
Chance	-0.02	-0.02	-0.29***	1.00						
Others	0.00	-0.03	-0.05**	0.59***	1.00					
<i>Causes of poverty</i>										
Fate	-0.06***	-0.09***	-0.31***	0.49***	0.29***	1.00				
Structural	0.01	-0.05**	0.04*	0.10***	0.09***	0.30***	1.00			
Individual	0.00	0.07***	0.34***	-0.09***	-0.02	0.06***	0.31***	1.00		
<i>Wellbeing</i>										
Best life	0.01	0.13***	0.09***	-0.09***	-0.06***	-0.05**	-0.06***	0.10***	1.00	
Happiest life	0.21***	0.20***	0.14***	-0.05**	-0.01	-0.09***	-0.05**	-0.08***	0.31***	1.00

*p below 0.1, **p below 0.05, ***p below 0.01. 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. 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.

Appendix C

C.1 Summary of two documentaries

C.1.1 Teyiba Abdella

Teyiba Abdella lives in Girawa district of Eastern Hararge zone, Oromia Region. Most people in the district are involved in mixed agriculture, cultivating both crops and livestock. The next most prevalent activity is trade. Trade is now a major activity for Teyiba, although she is also engaged in farming.

Teyiba married her husband, Aliya Yousuf, by choice although her parents objected to their marriage and refused to give her their blessings. At that time, both Teyiba and Aliya had no assets and started their married life with hardly any income. Their fellow villagers contributed one birr each to help them start their life together. Using the neighbours' contribution as seed money, Teyiba began trading wheat flour on a small scale. She used to walk to the market at least for three hours carrying 50 kilograms of wheat flour on her back. A woman who owns a flour mill in the market town observed these efforts and offered her credit to purchase flour. After selling the flour she obtained on credit, she paid back her debt and saved her profits. Because she paid back her debts on time, the miller started giving her up to 100 kilograms of wheat on credit. After a couple of years she expanded her trade to poultry. She also bought a donkey to carry her heavy loads to the market.

Teyiba and her husband have opened their own shop. They have also built themselves a house and acquired a plot of land in the nearby village to build another house. Teyiba's husband does most of the household chores while she undertakes most of the business activities. Teyiba does not accept the criticism that some of her villagers have about her being the major breadwinner of her household while her husband is the main homemaker.

Although Teyiba is engaged in trade as her main activity, she also works diligently on their farm. People in the village have a high regard for her and acknowledge her and her husband's achievements. They admire her hard work and commitment. Teyiba's husband also admires her for her strength and believes she is a great role model for people in their village.

C.1.2 Bashir Malim

Bashir Malim is a farmer living in Warri village, roughly 658 kilometres south of Addis Ababa. He is 27 years old, married, with two children. He 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, Bashir started crop production.

Since he has no formal education except for basic literacy, he sought out an agricultural expert in a local NGO, consulted him about good farming practices and implemented everything he learned. He started planting vegetables such as tomatoes, onions and potatoes and sold his output in the market. After experiencing a good harvest, he bought a pair of oxen.

Two or three years later, after saving some money, he went back to the agricultural expert and asked the NGO to purchase him a water pump from Addis Ababa, using money he had saved. After acquiring the water pump, he further expanded the area he had under cultivation. The pump made watering a larger area much easier than using buckets. He started planting papaya, sugarcane, maize and other crops. He also rented additional land and increased his productivity by improving his soil fertility. He became an owner of a large herd of cattle. He is also engaged in beekeeping and producing tree seedlings for sale. During 2007, when tree planting was very much encouraged by village administrations, he managed to produce and distribute seedlings to seven peasant associations and a local NGO in the area. Extension agents and fellow farmers in the area speak of him as someone who is an innovator and hard worker with good savings habits.

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