Learning *with* Others: A Field Experiment on the Formation of Aspirations in Rural Ethiopia

Tanguy Bernard, Stefan Dercon, Kate Orkin, and Alemayehu Seyoum Taffesse*

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Abstract

Several recent studies draw attention to the difficulties faced by the poor in forming the type of aspirations that can induce more expansive future-oriented behaviour (Appadurai, 2001; Ray, 2006). This communal deficit of aspiration can, in turn, contribute to the slow improvement of economic well-being amongst the poor. This paper reports on findings of an innovative experiment to test this theory in rural Ethiopia. Firstly, individuals were randomly invited to watch documentaries about people from similar communities who had succeeded in agriculture or small business. A placebo group watched an Ethiopian recreational TV show 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 one's 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 and younger individuals. The number of an individual's peers also invited to a documentary further contributed to these changes, confirming the importance of the collective in revision of initial aspirations. Lastly, we find evidence of treatment effects on savings and credit behaviour, children's school enrolment, investments in children's schooling and time dedicated to farming/leisure, suggesting that changes in aspirations may translate into effective changes in forward-looking behaviour.

1 Introduction

A growing recent empirical literature puzzles over the fact that people often fail to invest, even though returns are sometimes very high. Examples include Goldstein and Udry (2008) and Duflo, Kremer, and Robinson (2008) in agriculture; Miguel and Kremer (2004) in health; or Munshi and Rosenzweig (2006) in education. The same evidence also shows that such behavior is often even more acute among poorer populations (see Banerjee and Duflo. (2007) for a review). A variety of mostly complementary explanations have been forwarded over the years. In a first class, investments do not occur because one's expectations of privately appropriable returns are simply too low. The problem here arises primarily from the individual's environment, through such issues as market failure, lack of sufficient information (Yamauchi, 2007), or social constraints (Platteau, 2000). In contrast, a second class of explanations shifts the focus towards the manifested attributes of decision makers. These may encompass issues of identity (Hoff, Karla and Pandey, 2006), or psychological issues related to impatience, commitment and psychological barriers (Mullainathan and Shafir, 2009; Duflo, Esther, Michael Kremer and Robinson, 2011).

A third perspective attempts to blend external constraints that the poor face with the potential effect these constraints may have on the internal logic governing choice by these people. The argument can be informally stated as follows. Attributes of decision-making crucially rely on the set of beliefs and perceptions one has regarding her physical and social environment - a set that evolves with learning through experience. This approach clearly relates to an older literature in social psychology dealing with relevant issues (see, for instance, Rotter, Chance, and Phares (1972); Bandura (1971); Fishbein and Ajzen (1975); Ajzen and Fishbein (2005)). In a recent comprehensive account for instance, Ajzen and Fishbein (2005) describe how a person's attitude with respect to a particular behaviour is conditioned by her beliefs vis-à-vis the outcome associated with that behaviour as well as the opinion of relevant others vis-à-vis that behaviour. In the language of economics, this

*Bernard (IFPRI): T.Bernard@cgiar.org. Dercon (Oxford/DFID): stefan.dercon@economics.ox.ac.uk. Orkin (Cambridge): ko318@cam.ac.uk. Seyoum Taffesse (IFPRI): A.SeyoumTaffesse@cgiar.org. This document is an output from research funding by the UK Department for International Development (DFID) as part of the iiG, a research programme to study how to improve institutions for pro-poor growth in Africa and South-Asia. The views expressed are not necessarily those of DFID. Bernard and Taffesse were funded by Seven in the Open Enterprise Solutions to Poverty competition to produce the documentaries used in the intervention. They would also like to acknowledge the support of the IFPRI Development Strategy and Governance and Markets, Trade and Institutions divisions. The views expressed in this document are not necessarily those of the funders. The authors thank Sonia Bhalotra, Simon Quinn, Laura Camfield, Matthew Collin, Karla Hoff, Debraj Ray and participants at the RES, CSAE, ABCDE and Young Lives conferences and seminars at Oxford, IFPRI, NYU and Warwick for their feedback.

is equivalent to stating that individual preferences are determined, at least partly, by individual beliefs and the latter, in turn, are significantly responsive to social interactions. A deeper understanding of these mechanisms and their relationship with economic outcomes may lead to a better appreciation of individual-community symbiosis in the context of economic change generally.

In this vein, several recent studies draw attention to a particular impediment facing the poor when forming the type of aspirations - a set of future-oriented preferences - that can induce more expansive future-oriented behaviour, which can explain why they refrain from making well-being-enhancing investments. In particular, and Ray (2006) argue that an individual largely forms aspirations by discerning the outcomes of individuals whose behaviours she can observe, but also with whom she can identify. More broadly, although an individual attribute, the capacity to aspire is largely socially determined and, in turn, can contribute to the slow improvement of economic well-being amongst the poor (Genicot and Ray (2010)). [1]

Two recent papers provide evidence of such mechanisms in Nicaragua and India. Using exogenous variation in the composition of groups (Macours and Vakis, 2009) or a partial population design (Beaman et al., 2012), both studies find improvement in aspirations and future-oriented behavior of people randomly exposed to women in leadership positions in their community. However, neither study can empirically disentangle the effects of increased information from the effects of concrete policy interventions, although secondary evidence tends to support the former. In this paper, we provide further empirical evidence on the process of aspiration formation, through a specifically designed experiment in rural Ethiopia.

1.1 Paper outline

Our paper is based on a two-stage randomized control trial of a pilot video-based intervention in rural Ethiopia, in which we screened short documentaries to residents of 64 villages in the Eastern part of the country. Each documentary featured an individual from the same area and reported on how he/she managed to significantly improve his/her economic circumstances by establishing or expanding a small business or improving farming practices.

In each village, twelve individuals were invited to a documentary screening, while twelve other individuals served as a control group. We assess potential social spillover using the number of one's reported peers who were invited to a documentary screening. Further, in 32 of the 64 villages, an additional thirty-six individuals were invited to the documentary, thereby generating an additional exogenous variation in the number of an individual's peers exposed to this information. Finally, to account for potential effects arising simply from the event of gathering for a screening occurring in relatively remote villages where access to television or movies is limited, we also implemented a symmetrical placebo design, in which individuals were invited to watch a standard Ethiopian television show.

We measure the effect of the intervention six months after screening, using a previously designed and tested aspiration index combining individuals' aspirations for their future income, wealth, social status and children's education (Bernard and Taffesse, 2012). We find that watching the documentaries has a positive direct effect on individuals' aspirations. Our results further point towards important social spillovers, wherein the number of one's peer who saw the documentaries positively affects one's aspiration, irrespective of her own treatment status. Results are robust to several specifications and alternative measures of individuals' peer network. Overall, these results give support to Ray's conception of aspirations as being, in part, socially determined. Our results further suggest that these effects are stronger for relatively younger individuals and those with higher aspiration levels at baseline.

We then examine each dimension of aspirations separately. Although none of the documentaries screened related to education, we find the largest effect of treatment on the participants' aspiration regarding their children's educational attainment. This suggests that education is viewed as a future-oriented investment and improvements in aspirations are as or more likely to affect the next generation rather than those whose aspirations shift.

Finally, we find that the intervention had some effects on future-oriented behaviours. We find consistent evidence that documentary sessions affected individuals' propensity to save, as well as time dedicated to income generating activities and hypothetical demand for credit. Above all, we find clear evidence of effects on spending on children's education and on children's school enrolment, essentially mediated through peers' exposure to ducumentaries.

[1] It is important to mention a number of relevant strands of the literature respectively focusing on social interactions (Manski, 2000), economics of identity (Akerlof and Kranton, 2000), non-cognitive skills (Cunha and Heckman, 2008), and a potentially more comprehensive psychology and economics of personality (Almlund et al., 2011). Bernard, Dercon, and Taffesse (2011) discuss these strands and their possible import to the aspirations failure perspective.

1.2 Contributions

Our paper thus contributes to the literature in several ways. First, we provide, to our knowledge, the first robust experimental evidence on an intervention designed specifically to influence aspirations without any other policy changes – a pure aspirational intervention. We can examine the role of groups in changing aspirations and whether changes are linked to future behaviour without concerns that our intervention also causes other economic changes that confound or magnify the change in aspirations.

Second, we show that social interactions play an important role in the evolution of individual aspirations. Because the intervention occurs over a short time period, we argue this plausibly arises from discussions between individuals and their peers rather than because individuals see peers changing their behaviour. Social learning literature in economics is often framed as learning from others by watching their behaviour and learning from their outcomes. In a sense, our results suggest the existence of a 'learning with other' type of effect.

Lastly, this paper adds to the growing empirical literature on the influence of media on behavioural outcomes (e.g. La Ferrara, E., Chong, A., and Duryea; Jensen, R. and Oster (2009); DellaVigna, S. and Kaplan (2007); Paluck (2009)), suggesting that media-based development programmes may have considerable benefits. Overall, our results suggest that, particularly in remote areas relatively underexposed to television and other media, motivational documentaries may be relatively inexpensive interventions to encourage forward-looking behaviour. These could be used on their own or integrated into other interventions such as savings and loan programmes or programmes to encourage school enrolment.

In Section 2 we detail our experimental setting, the measures used to assess treatment effects, and the estimation strategy. Estimates of the effect of treatment on aspirations are presented and discussed in Section 3. In Section 4, we explore the effect of treatment on actual credit and savings, time allocation, school enrolment of children and expenditure on children's schooling. Section 5 concludes.

2 Empirical setup

Empirical investigations into the formation of aspirations raise a number of identification challenges. First, at the individual level, (lack of) aspiration and (lack of) economic well-being may reinforce one another in a continuous feedback loop. Absent exogenous variations in either one of them, any assessment of the causal effect running from poverty to aspirations, or vice versa, is likely to be biased. Second, it is possible that aspirations reflect an individual's cognitive world – his or her zone of similar, attainable individuals, which Ray (2006) calls individual's "aspiration window". If this is the case, identification of the effect of a person's aspiration window on their aspiration level faces similar difficulties to identification as that of the causal effect of any behaviour of a group on the outcomes of a group member. As highlights, the direction of causation may be blurred by sorting effects (where individuals form groups of similar peers), correlated effects (where peer groups are subject to the same shocks), or reflection biases (where one cannot distinguish between the effect of group-member interactions from the mere summation of individual behaviour).[2]

The present study explores the role of relevant information in the formation of aspirations and assesses the importance of peer effects within this process. It relies on a field experiment in which four 15-minute documentaries were screened to randomly chosen men and women in rural Eastern Ethiopia. The documentaries were made specifically for the purpose of the experiment and in Oromiffa, the local language. They featured individuals from the same region who, through their own perseverance and hard work, had managed to improve their socio-economic well-being significantly despite adverse initial conditions. Two examples of these stories are described in Appendix. Importantly, none of the individuals featured had become excessively rich or powerful, so the documentaries represent records of remarkable yet replicable pathways out of poverty. The documentaries described men and women from communities near enough to seem very similar to the respondent but distant enough that it was nearly impossible that respondents would know anyone in the videos.

2.1 The study site

We conduct our experiment in rural Ethiopia. In fact, fatalism is customarily, if not always formally or explicitely, attributed to Ethiopians - particularly those who are poor. The apparent intention, in such instance, is to characterize the lack of proactive and systematic effort to better their lives and the implied acceptance of their circumstances that many Ethiopians seem to display. This view certainly appears consistent with the language used by the disadvantged to describe their lives and the difficulty thereof (see for instance Rahmato and Kidane (1999) for an account of relevant local expressions).

Specifically, the study takes place in Doba Woreda, an administrative district of the West Haraghe Zone of the Oromia region located 380 km east of Ethiopia's capital city, Addis Ababa. Villages in Doba are relatively small,

[2] See Manski (1993, 2000) or Moffitt (2001) for discussion of identification issues, Yang (2007), Kling, Liebman, and Lawrence F. Katz (2007) or Sacerdote (2001) for empirical studies relying on variations in group compositions, and Duflo and Saez (2003) or Bobonis (2009) for partial population designs.

with an average of about 400 individuals in our sample. District inhabitants are mostly Muslim smallholder farmers growing sorghum and maize. Only 1.5% of the population are considered urban dwellers. Overall, Doba is relatively poor and food insecure, and was one of the first districts selected for the national Productive Safety Net Program (PSNP) in 2005, which is targeted at the poorest and most chronically food-insecure districts in the country.

The study site is remote. The majority of villages were only accessible for 4x4 vehicles, while some even required camel transportation. There is limited exposure to video-based media: at baseline 10 per cent of respondents watched TV at least once a week, 29 per cent watched at least once a month and 62 per cent watched about once a year or never.

2.2 Experimental design

Sixty-four villages were randomly selected from the Central Statistical Agency's list of villages for the district. These were then grouped into 16 screening sites with four villages in each site, as shown in the figure below. Within each village, eighteen households were randomly selected and allocated to one of three groups: a treatment group, a placebo group and a control group. For all groups, the household head and his/her spouse were interviewed at their home.

At the end of the interview, the six households allocated to the treatment group received two tickets - one per spouse - to a screening session featuring four fifteen-minute documentaries. Households were told that their tickets were non-transferable and that they could only attend the screening at the time written on the ticket. The name and survey identifier number of each respondent was written on the ticket. Respondents were also told that there would be a small gift given as compensation for their time after the screening. It is unlikely that there were any priming effects in relation to aspirations. Respondents were not told that there was any purpose to the screening, but were simply told it was an entertainment show.

It is possible that in such an isolated area, the screening of any video affected individual behaviour independently of its content. Six households were therefore allocated to a placebo group. Their tickets - one per spouse - gave them entrance to a screening session in the same venue as the treatment group, but at a different time. At this screening, a standard Ethiopian TV entertainment was shown. Respondents were given exactly the same explanation as the treatment group. [3]

The six households in the control group did not receive any tickets and served as a within-cluster control group. 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 documentary and entertainment screening. They were also told they would receive a small gift as compensation for their time and received the same gift as the treatment and placebo groups.

Screenings were held for respondents from four villages at once at a roughly central location, usually a school or farmer's training centre.

We generated further exogenous variation in the number of one's peers invited to the documentary session. In two of the four villages per screening site, we distributed additional invitations to documentary screening sessions to 18 randomly selected households - one ticket per spouse - but did not collect data on these individuals. In the other two villages, no extra invitations to documentaries were distributed. Instead, we randomly invited 36 spouses from 18 households to the placebo session. [4]

Table 1: Experimental design

		All villages	Treatment villages	Placebo villages
# villages		64	32	32
# individuals surveyed		1,943	976	967
	of which:			
	Treatment individuals	629	318	311
	Control individuals	653	324	329
	Placebo individuals	661	334	327

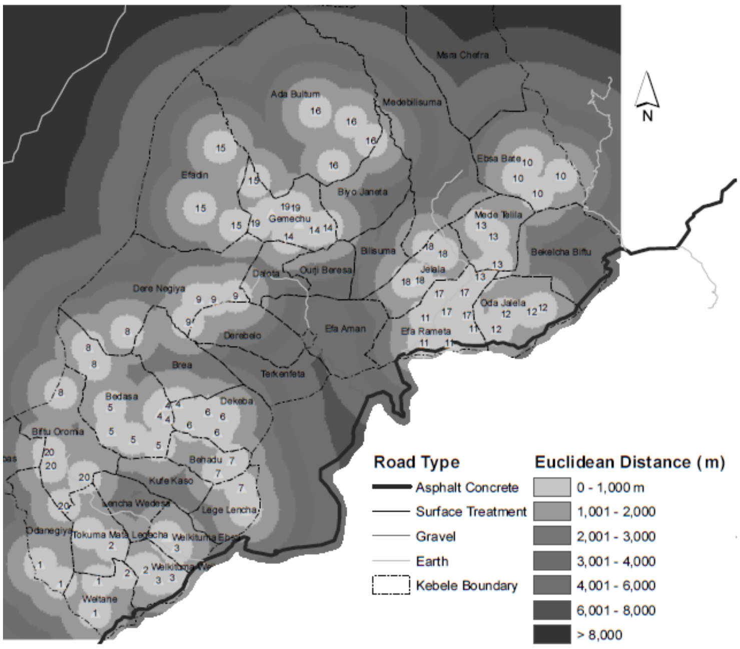
*This is the sample used for analysis and drops observations with missing values on control or outcome variables.

In sum, in each of the 16 screening sites, there were two sessions. In the documentary session, there were 24 couples from all four villages that had already been surveyed. There were a further 36 couples from two of those villages (the "intense treatment" villages) who had not been surveyed but were merely given tickets. In the placebo session, there were also 24 couples from all four villages that had already been surveyed. There

[3]See Card et al. (2010, 2012) or Berg and Zia (2013) for other recent studies examining the effect of information provision and using a placebo-based design.

[4]See Baird et al. (2012) for a related discussion on the broad family of saturated designs to identify social interaction mechanisms.

Figure 2.1: Village location within Doba Woreda, Eastern Ethiopia



Note: Each number denotes a village. Villages with the same number were grouped are in one screening site. Only 16 of the 20 sites in the Figure are part of the study: the extra four sites were sampled in case an area could not be reached.

were a further 36 couples from two of those villages (the “intense placebo” villages) who had not been surveyed but were merely given tickets.

2.3 Compliance

Compliance levels are reported in Table 2. If all the households sampled had had a head and spouse, there would have been 768 individuals in each of the three groups, as shown in the first row. However, 165 individuals were single, widowed or divorced. A further forty individuals were not surveyed or given tickets as it was clear that they would not be able to attend the screening (because they were away, ill or had just given birth).

Compliance among those effectively allocated tickets is high, despite an average 29 minutes travel time to the screening site. Furthermore, respondents almost always attended the correct screening, with only 14 attending the wrong screening or attending a screening when they were in the control group. [5]

Compliance reaches 97 per cent on average among surveyed households and 94 percent among households invited to screenings but not surveyed. We do not find any large difference in compliance between groups.

Table 2: Compliance and attrition

		Surveyed				Not surveyed		
		Treated	Placebo	Control	Total	Treated	Placebo	Total
Sampled		768	768	768	2,304	1,152	1,152	2,304
	Single or spouse dead	43	30	37	110	23	32	55
	Not given ticket	11	8	11	30	8	2	10
Given tickets*		714	730	720	2,164	1,121	1,118	2,239
	Missed screening	23	15	–	38	54	31	85
	Wrong screening	1	2	11	14	0	0	0
	Arrival not recorded	10	6	–	16	54	81	135
	Complied	680	707	709	2,096	1,013	1,006	2,019
Compliance rate (%)		0.952	0.968	0.985	0.969	0.904	0.900	0.937
Missed Round 2		6	7	7		–	–	

*Controls are not invited to a screening. This denotes controls who are surveyed.

2.4 Peer-level treatment

Within-village random allocation of invitations to documentary ensures exogenous variations in the extent to which one’s peers were directly exposed to documentaries. These are further affected by village-level variations in the number of invitations that were distributed. In fact, if peer network are sufficiently correlated with the unit of treatment (one’s village) our design ensures exogenous variation within one’s number of peers that were exposed to the treatment. In addition, with imperfect correlation between one’s village and one’s social network, this design offers the added advantage of generating almost continuous distribution of network-level intensity of treatment, further helping identification of network-level treatment effects (Baird et al., 2012).

We measure respondent’s peer-level treatment through two sets of questions administered at baseline. In the first network measure, we asked each surveyed individual to list their four closest friends, and assessed the extent to which these were listed amongst the list of invitees to either treatment or placebo screening sessions.[6] We limit the number of peers cited to four 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, in line with the remoteness of these communities, allowing for our design to effectively generate variations in these peers’ exposure to treatment or placebo. Only 14 per cent of the respondents listed their siblings within the four individuals, suggesting that any peer effect cannot be fully explained by family-level characteristics.

As shown in Table 3, the distribution of peer-level treatment intensities is perfectly symmetric across intense-treatment and intense-control villages, with higher intensities found in the treatment villages. The opposite is true for peer-level placebo intensities. Importantly, high and low peer-network treatment and placebo intensities

[5]This was possible because documentaries and movies were held at different times and people were only told the time for the screening they were invited to. Colour-coded tickets bearing the respondents’ name and the time of screening they were invited to and were checked at the door. Screening sessions were in closed rooms and a large team of nearly 30 enumerators controlled entry. People could conceivably have swopped tickets and lied about their names. However, the enumerators who conducted baseline surveys checked tickets for entry to the screening, so this is unlikely.

[6]Respondents did not know their peers’ treatment status. They were asked to list their peers in the baseline interview before they were assigned to treatment, placebo or control. All households were interviewed for the baseline before any screenings occurred. The respondents’ peers may have already been interviewed and assigned to treatment, but they knew only that they had received a ticket of a certain colour to attend a screening, not what the screening involved.

are found in both intense-treatment and intense-placebo villages, that is: a person in an intense-placebo village may sometimes have more of his/her peers treated than a person in an intense-treatment villages.

Table 3: Network measures

	All villages	Intense Treatment villages	Intense Placebo villages
Panel A. Number of treated/placebo among four people known best			
Distribution of peer-level treatment			
No peer has seen documentary	45.4	25.27	65.88
1 peer has seen documentary	32.43	37.36	27.4
2 peers have seen documentary	16.46	27.03	5.7
3 peers have seen documentary	4.77	8.68	0.78
4 peers have seen documentary	0.94	1.65	0.22
Distribution of peer-level placebo			
No peer has seen placebo	48.17	69.34	26.62
1 peer has seen placebo	31.98	24.62	39.49
2 peers have seen placebo	15.85	5.71	26.17
3 peers have seen placebo	3.33	0.33	6.38
4 peers have seen placebo	0.67	0	1.34
*Panel B. Number of treated/placebo with whom individual discusses regularly**			
Distribution of peer-level treatment			
No treated individual regularly discussed with	53.82	53.41	54.25
1 treated individual regularly discussed with	31.87	30.88	32.89
2 treated individual regularly discussed with	14.3	15.71	12.86
Distribution of peer-level placebo			
No placebo individual regularly discussed with	55.6	55.27	55.93
1 placebo individual regularly discussed with	30.16	30.55	29.75
2 placebo individual regularly discussed with	14.25	14.18	14.32

*Discusses regularly =1 if positive response to at least one of
'In the past 12 months, have you discussed farming or business matters with name?',
'In the past 12 months, have you discussed matters relating to, savings, credit or other financial issues with name?',
'Is name a member of the same Village Savings and Loans group as you?'.

For comparison and robustness purposes, we use a second network measure, for which we randomly selected six individuals in total from the list of treated, placebo, and control individuals within in each village. We selected two individuals who received the treatment, two who received the placebo and two who received the control and asked respondents which individuals they interacted with regularly, by discussing farming, business matters, savings, credit or financial issues.

In Panel B of Table 3, we report the distribution in the number of positive answers (from 0 to 2) for individuals who received the treatment and individuals who received the placebo. By construction, there are no variations across intense-treatment and intense-placebo villages. However, someone with a larger network may be more likely to know more people and therefore respond positively to questions about whether they interacted with a particular person, which may bias later estimates. In all estimations involving this measure, we therefore control for such effects using the total number of individuals known amongst the six individuals proposed.

2.5 Measures of aspirations

Aspiration measures vary substantially from one study setting to another. Previous studies have for instance relied on depression scales, positive feelings towards the future, locus of control, goals and others to characterize aspirations (see Bernard and Taffesse (2012) for a review). While all may be strongly related to one another and to the idea of aspirations in general, they may carry somewhat different meanings and potential policy implications, calling for a more straightforward measurement tool. To this end, this study relies on a new measure of individuals' aspirations, which was tested for validity and reliability in summer 2009 within 16 villages of central Ethiopia (see Bernard and Taffesse (2012)). Our indicator rests on respondents' answers to questions related to their income, their wealth, their social status and their children's educational attainment.[7]

[7] Income and wealth measured in Ethiopian Birr. Education was measured in the years of schooling the respondent wished their child to complete. Social status was measured in percentage of village-community members asking for advice at times of important

For each of these dimensions, respondents were asked for their self-assessment of the level they wished to attain. For comparison purpose, respondents were also asked the level they thought they would reach within ten years. Although very correlated, these two sets of questions sometimes led to different responses. We use these latter measures for robustness check of our main aspiration-related results.

To facilitate comparisons and aggregation across the four considered dimensions, we first standardize answers by removing the sample mean from each observation and dividing by the standard deviation of the said dimension in the sampled population. Each dimension is then unit-free and can be readily used towards an aggregated index with other dimensions. A further issue relates to the importance that a person attaches to particular dimensions of her life outcomes. With heterogeneous preferences, some respondents may, for instance, value more social status within their community than their level of wealth, while it may be the opposite for others. Also, while each may report high aspirations levels for both dimensions, unless required to reveal their idiosyncratic preferences, the aggregate indicator will not capture these distinctions. Thus, respondents were also asked to weight the four dimensions according to their own assessment of the dimension's significance for them.[8] These weights are then used to aggregate the standardized responses to each of the four dimensions into an aspirations index.[9]

In Table 4, we report a set of correlates of aspirations at baseline, for the synthetic aspiration measure as well as for each dimension separately. Note that aspirations related to children's education were only asked to respondents with children, lowering the overall sample for this indicator. [10]

Table 4: Baseline correlates of aspirations index

	Aspirations index	Income	Wealth	Education	Social status
Age	0.00	−0.00	0.00	0.00	0.01**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Male	0.14***	0.04	0.06	0.25***	0.17***
	(0.02)	(0.03)	(0.04)	(0.05)	(0.05)
Any formal education	0.06*	−0.08	0.02	0.22***	0.10
	(0.04)	(0.08)	(0.05)	(0.05)	(0.06)
Total value of household assets (ETB)	0.00	0.00	−0.00	0.00***	−0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Father had any formal education	0.01	−0.04	−0.03	0.02	−0.01
	(0.03)	(0.04)	(0.03)	(0.10)	(0.07)
Single, widowed or divorced	−0.06	−0.03	−0.01	−0.06	−0.17**
	(0.05)	(0.03)	(0.02)	(0.18)	(0.08)
Watches TV daily or weekly	−0.03	−0.04	−0.04	0.09	−0.13**
	(0.03)	(0.05)	(0.03)	(0.07)	(0.06)
Mosque more than once a week	−0.12**	−0.00	−0.16	−0.07	−0.23***
	(0.05)	(0.01)	(0.11)	(0.09)	(0.07)
Mosque weekly	−0.05	0.07	−0.15	−0.07	−0.00
	(0.04)	(0.06)	(0.10)	(0.07)	(0.06)
Constant	−0.11*	0.10	0.04	−0.37***	−0.31***
	(0.06)	(0.14)	(0.08)	(0.13)	(0.11)
Respondents (Villages)	1642 (64)	1642 (64)	1642 (64)	1642 (64)	1642 (64)
Adj. Wald test: P val	0.00	0.66	0.98	0.00	0.00

*p below 0.10 **p¡0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis. t stats and adjusted Wald tests. The base category for attending mosque is attending mosque monthly or less.

Specifically, we assess correlations with respect to a variety of variables which theory and other studies suggest would typically be strong correlates of aspirations (e.g. Bernard and Taffesse (2012)). Correlates of

decisions.

[8]To get a concrete number for the weight attached to each dimension in a context in which many respondents were illiterate, 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.

[9]Specifically, let a_i^k be individual i's aspiration response to applied to dimension k, and let w_i^k be the weight that individual i assigned to this dimension. The aspiration index can thus be expressed quite simply as: $A_i = \sum_k \left(\frac{a_i^k - \mu_k}{\sigma_k}\right).w_i^k$, where μ_i^k and σ_i^k measure the sample mean and standard deviation, respectively. This approach is somewhat similar to that of Beaman et al. (2012), apart from the fact that (i) each aspiration constituent is numerical (as opposed to categorical), and (ii) weights used are individual-specific, thereby accounting for heterogeneity in valued attributes of life.

[10]To facilitate comparisons across estimates, we thus restrict our entire sample to those individuals only, when using aspiration as outcome variable. Further, for all outcome variables used in the paper (including aspirations), we also restrict the sample to those individuals whose reported level of that outcome did not exceed 3 standard deviation above or below the sample mean.

the synthetic aspiration index are reported in the first column, and effectively synthesize the correlations found in the next four. Overall results indicate an important role for gender, with women aspiring significantly less than men. The difference between men and women is particularly strong for aspirations related to children's education. As one would expect, individuals with any formal education tend to report higher educational aspirations for their children and higher status aspirations for themselves. Wealthier individuals, as measured by the assets index, aspire to higher levels of education for their children, but perhaps surprisingly do not have higher aspirations on other dimensions. Higher levels of religiousity result in lower social status , although there is no clear reason for this.

2.6 Experimental integrity

To assess the extent to which randomized allocation of invitation produced comparable samples at baseline, we run a series of tests for the experimental integrity of our design. Table 5 reports standardised aspirations at baseline and shows that we find no differences between groups on any of the components of aspirations or on the aggregate index.

Table 5: Experimental integrity: outcome variables

	Total	Treatment	Placebo	Control	T-C	P-C
	Mean (Standard Deviation)				Difference p val.	
Baseline aspiration						
Standardized income aspirations	0.006	0.068	-0.032	-0.032	0.147	0.688
	(1.010)	(1.772)	(0.021)	(0.001)		
Standardized wealth aspiration	0.001	-0.001	0.040	-0.038	0.356	0.161
	(1.010)	(1.022)	(1.410)	(0.081)		
Standardized aspiration for children's education	0.000	0.037	-0.033	-0.001	0.498	0.572
	(1.003)	(0.992)	(0.957)	(1.056)		
Standardized aspirations for social status	-0.010	-0.048	0.005	0.011	0.213	0.906
	(0.906)	(0.681)	(0.993)	(1.000)		
Standardized aggregate aspiration	0.016	0.031	0.020	-0.002	0.202	0.405
	(0.506)	(0.555)	(0.579)	(0.352)		

* p below 0.1; ** p below 0.05; *** p below 0.01

Table 6 further reports a series of balancing tests for both treatment and placebo experiments on a variety of individual-level variables. No significant differences are found across samples in education, gender or age of the individuals; the frequency with which they watch TV, listen to the radio or travel outside the district and their attendance at mosque or church. Individuals in the treatment group are more likely to be single (unmarried, widowed or divorced), but only 4.8 per cent of respondents are single so this affects a small proportion of the sample.

2.7 Empirical strategy

We use this empirical setting to investigate two complementary mechanisms with respect to the the formation and revision of aspirations. First, we assess the informational effect by which an individual may revise her aspirations based on the experience of another individual that she may not know but whose environment and initial economic means are similar to hers. The direct effect of having been invited to a documentary screening provides first hand evidence of this mechanism (δ_1). Second, we assess the extent to which aspirations are socially determined, depending in part on peers' future-oriented preferences. For this, we rely on the number of one's peers who have been invited to a documentary session, irrespective of one's own treatment status (δ_2). As discussed above, we account for potential experimental biases using a perfectly symmetrical placebo experiment, allowing us to test for direct (ρ_1) and indirect (ρ_2) effect of a standard Ethiopian entertainment show onto one's aspiration.

We rely on an ANCOVA specification, where endline aspiration is regressed on treatment variables along with aspiration level measured at baseline. ANCOVA estimators are more efficient when outcome variables are measured with significant noise, as is usually the case for attitudinal data in general (McKenzie, 2012).

Our measures of peer network is also likely to be incomplete as it does not account for the fact that other peers may have been invited to documentaries outside of those that were cited, nor does it account for potential second order effects by which peers may further influence one-another. Thus, absent a 'pure control' group composed of individuals whose peers were non-affected by treatment (as would be the case in villages where no invitations would have been distributed), we are unable to measure the 'Treatment Effect on the Uniquely Treated' (Baird et al., 2012), and our estimates merely measure the difference between those individuals whose

Table 6: Experimental integrity: controls

	Total	Treatment	Placebo	Control	T-C	P-C
	Mean (Standard Deviation)				Difference	p val.
Age	36.769	37.035	36.795	36.483	0.410	0.658
	(12.769)	(11.481)	(13.071)	(12.504)		
Male	0.501	0.512	0.495	0.497	0.593	0.937
	(0.500)	(0.500)	(0.500)	(0.500)		
Any formal education	0.297	0.321	0.284	0.287	0.188	0.900
	(0.457)	(0.467)	(0.451)	(0.452)		
Single	0.048	0.060	0.045	0.037	0.052*	0.380
	(0.213)	(0.237)	(0.210)	(0.188)		
Father had any education	0.062	0.069	0.061	0.055	0.273	0.594
	(0.241)	(0.254)	(0.240)	(0.228)		
Watches TV more than once a month	0.102	0.102	0.094	0.109	0.717	0.386
	(0.302)	(0.303)	(0.292)	(0.311)		
Listens to radio more than once a month	0.612	0.622	0.595	0.622	0.992	0.308
	(0.487)	(0.485)	(0.491)	(0.485)		
Travels outside the district more than once a month	0.134	0.141	0.118	0.143	0.917	0.182
	(0.341)	(0.348)	(0.323)	(0.350)		
Goes to mosque more than once a week	0.179	0.180	0.161	0.196	0.457	0.104
	(0.383)	(0.384)	(0.368)	(0.397)		
Goes to mosque every week	0.600	0.617	0.602	0.581	0.184	0.430
	(0.490)	(0.486)	(0.490)	(0.494)		
Goes to mosque every month, rarely or never	0.066	0.076	0.067	0.057	0.169	0.420
	(0.249)	(0.265)	(0.251)	(0.231)		
Total value of household assets (ETB)	6484.53	6794.756	6097.32	6534.578	0.600	0.353
	(5359.621)	(5675.354)	(4946.477)	(5398.411)		

* p below 0.1; ** p below 0.05; *** p below 0.01

close network has been more or less directly exposed to the treatment. Because such bias are likely more important where more peers have been exposed, we control for the intensity of treatment across villages.

Our first specification is provided in Equation 2.1, where y_{i2} and y_{i1} measure aspirations at endline and baseline respectively, T_i is an individual-level dummy variable indicating whether the individual was invited to a documentary session and P_i whether she was invited to a placebo session. As discussed above, when our second network measure is used, we also control for the total number of individuals known by the individual. I_v is a village-level dummy variable indicating intense-treatment villages and η_i is an individual-level error terms. [11]

$$y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \gamma y_{i1} + I_v + \eta_i \tag{2.1}$$

We then assess whether these direct effects are robust to the introduction of peer-level treatment, and if the latter themselves helps explain chnages endline level of aspiration. In Equation 2.2, n_i^T thus captures the number of one's peers who were invited to a watch a documentary, and n_i^P those who were invited to watch the placebo

$$y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \delta_2 n_i^T + \rho_2 n_i^P + \gamma y_{i1} + I_v + \eta_i \tag{2.2}$$

To further assess the robustness of the results obtained, we use a set of screening site-level dummies, μ_s, to capture locality characteristics or locality-specific shocks, along with additional controls measured at baseline and represented by the vector X_{i1}. These include variables we have theoretical reason to believe might influence aspiration and other outcomes: age, gender, whether the respondent has had any formal education, father's education, exposure to television, and religiousity, captured by regularity of attendance at religious services. We also control for marital status, where there was a slight lack of balance across treatment groups at baseline.

$$y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \delta_2 n_i^T + \rho_2 n_i^P + \gamma y_{i1} + I_v + \mu_s + X_{i1}'\pi + \eta_i \tag{2.3}$$

[11] Randomisation of direct treatment was done at individual level within a village, so we do not need to cluster to account for group-level randomisation (Cameron, Gelbach, and Miller, 2008). However, our second tier of treatment – namely the variation in intensity of direct treatment across villages) was done at village-level. To account for potential non-independence in outcomes within villages, above and beyond that generated by treatment spillovers, all reported standard errors are clustered at village-level. We do not use the usual Liang and Zeger (1986)standard errors as these can be unreliable if there are fewer than about 100 clusters and we have 64 villages. As Cameron, Gelbach, and Miller (2008)and Donald and Lang (2007)recommend, 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.

Lastly, although direct and indirect treatments are independent by construction, one may be concerned that individuals within the same peer group react homogenously to treatment. If so, such "sorting effects", in the terminology of Manski (1993), may partly drive the results with δ_2 merely capturing similar reactions to treatment (the same would be true for ρ_2). This would however only be true for those individuals within the treatment group. Thus, an implicit way to test for such possiblity is to add interaction term $T_i * n_i^T$ to our specification. An associated parameter $\delta_3 = 0$ is then an indication that sorting effects are not driving the results. For sake of completeness, the same procedure is applied to the placebo experiment, as well as cross-interactions as per the following specification.

$$y_{i2} = \alpha + \delta_1 T_i + \rho_1 P_i + \delta_2 n_i^T + \rho_2 n_i^P + \delta_3 T_i * n_i^T + \rho_3 P_i * n_i^P + \delta_4 T_i * n_i^P + \rho_3 P_i * n_i^T + \gamma y_{i1} + I_v + \mu_s + X_{i1}' \pi + \eta_i \quad (2.4)$$

3 Effect of treatment on expectations and aspirations

3.1 Preliminary evidence

We start this section by an investigation of the likely presence of a treatment effect six months following screening, through individuals' responses to questions directly targeted at the experiment. In Table 7, we use the group of treatment and placebo individuals and assess the extent to which they appreciated the screening sessions they were invited to and their assessment of the effect that these had had within their community. We find a high appreciation of both types of screening, although with statistically significant advantage to the sessions where documentaries were screened. We further find evidence that documentaries led to discussions within the villages, more than the placebo sessions did. Finally, six months after screening, one third of the individuals invited to a screening of documentary had discussed its content with a neighbour at least once during the previous two weeks, indicating that the documentaries had made an impression on respondents.

Table 7: Assessment of documentaries and placebo

	Treatment (standard error)	Placebo (standard error)	Difference (p-value)
Liked a lot what I saw	0.968	0.725	0.000***
	(0.175)	(0.447)	
Discussed it a lot with my neighbours	0.874	0.709	0.000***
	(0.332)	(0.454)	
Discussed it at least once with neighbours over the past two weeks	0.939	0.736	0.000***
	(0.24)	(0.441)	
Content generated a lot of discussion within village	0.323	0.205	0.000***
	(0.468)	(0.404)	

We also asked respondents who had seen documentary, six months after screening, to think of the one documented story that they felt the most relevant to their own conditions, and to assess the featured person's initial and achieved conditions. 49 per cent of all treated individuals felt that the featured person started from a situation below or equivalent to theirs, but ended in a better-off situation. This in turn, suggests the existence of significant heterogeneity in self-assessment of individuals' conditions, along with a somewhat limited room for impact of the documentary sessions for the other half of treated individuals.

3.2 Treatment effect on aspirations

In the tables which follow, each column number heading corresponds to the equation it refers to in the previous section. In column III, individual-level controls include variables for age, gender, education, asset value, marital status, father's education, exposure to television and mosque attendance. These controls are the same for all later specifications.

Tables 8 and 9 present the main results of the study, namely the direct and indirect effect of treatment on individuals' aspirations. Table 8 reports the results using the module where respondents selected their four closest peers. Table 9 reports the results using the module where respondents were asked whether they interacted with six randomly selected people, two from the treatment group, two from the placebo group and two from the control group. In this table we also control for the total number of six randomly selected villagers whom the respondent knew.

We find positive and significant direct effect of having been invited to a documentary screening sessions on individuals' aspirations six months after the screening (δ_1), compared to being assigned to the control group. The effect is robust to the introduction of indirect treatment variables and individual-level controls and screening

Table 8: Aspirations with self-selected peers

	(I)	(II)	(III)	(IV)
Treated individual	0.03	0.03*	0.03*	0.05*
	(0.02)	(0.02)	(0.02)	(0.03)
Placebo individual	0.02	0.02	0.02	0.00
	(0.02)	(0.02)	(0.02)	(0.04)
# peers treated		0.02*	0.01	0.01
		(0.01)	(0.01)	(0.01)
# peers placebo		−0.00	−0.00	−0.00
		(0.01)	(0.01)	(0.02)
Treatment village		−0.00	−0.00	−0.00
		(0.03)	(0.03)	(0.03)
Baseline level	0.14***	0.13***	0.11***	0.11***
	(0.04)	(0.04)	(0.04)	(0.04)
Treated indiv. * # peers treated				−0.00
				(0.02)
Placebo indiv. * # peers treated				0.01
				(0.02)
Treated indiv. * # peers placebo				−0.01
				(0.02)
Placebo indiv. * # peers placebo				0.01
				(0.02)
Constant	0.01	0.03	−0.02	−0.02
	(0.02)	(0.08)	(0.08)	(0.09)
Screening site F.E.	*No*	*Yes*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*Yes*
Respondents (Villages)	1642 (64)	1642 (64)	1642 (64)	1642 (64)
Adj. Wald test: P val	0.01	0.00	0.00	0.00
$\delta 1$-$\rho 1$	0.01	0.01	0.01	0.04
P val: $\delta 1 = \rho 1$	0.66	0.53	0.61	0.15

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father's education, exposure to television, mosque attendance. # villagers known is the total number of six randomly selected villagers whom the respondent knew.

site fixed effects in columns II and III, with a magnitude of about 0.06 standard deviation of our aspiration index. In contrast, having been invited to a placebo session has no statistically significant effect on aspirations compared to being in the control group (ρ_1). Precision of these effects is however limited, and wald test of difference between these two effects do not permit rejection of equality of coefficients δ_1 and ρ_1.

In columns II and III, we further assess the presence of an indirect effect of treatment mediated by the number of one's peers who were invited to the watch a documentary screning (δ_2). Results in Table 8 provide an indication for the presence of such effect. Using an alternative measure of peers network, this effect is further confirmed in Table 9. Accordingly, each aditional person known among the two treated individuals that were proposed to the individual, leads to a 0.08 standard deviation increase in endline aspiration.

Laslty, in both Tables 8 and 9, column IV introduce a set of interaction terms to estimate parameters δ_3, δ_4, ρ_3 and ρ_4 discussed above. Accordingly, $\delta_3 = 0$ is an indication of the absence interactive effect of direct and indirect treatment. It is also an indication of the absence of sorting effect, further reinforcing the results found in previous columns. A somewhat surprising result however, is given by negative and statistically significant parameter estimated for ρ_3 according to which, among individuals invited to a placebo session those whose peers were invited to a documentary session ended up with comparatively lower aspirations.

Table 9: Aspirations with randomly selected peers

	(I)	(II)	(III)	(IV)
Treated individual	0.03	0.03*	0.03*	0.07**
	(0.02)	(0.02)	(0.02)	(0.03)
Placebo individual	0.02	0.02	0.03	0.03
	(0.02)	(0.02)	(0.02)	(0.03)
# peers treated		0.04**	0.04*	0.07***
		(0.02)	(0.02)	(0.03)
# peers placebo		0.02	0.02	0.00
		(0.02)	(0.02)	(0.02)
Treatment village		0.01	0.01	0.01
		(0.03)	(0.03)	(0.03)
Baseline level	0.14***	0.13***	0.11***	0.11***
	(0.04)	(0.04)	(0.04)	(0.04)
# villagers known		−0.01	−0.02	−0.02
		(0.01)	(0.01)	(0.01)
Treated indiv. * # peers treated				−0.05
				(0.03)
Placebo indiv. * # peers treated				−0.06**
				(0.03)
Treated indiv. * # peers placebo				−0.01
				(0.03)
Placebo indiv. * # peers placebo				0.04
				(0.03)
Constant	0.01	0.04	−0.02	−0.03
	(0.02)	(0.08)	(0.09)	(0.09)
Screening site F.E.	*No*	*Yes*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*Yes*
Respondents (Villages)	1642 (64)	1639 (64)	1639 (64)	1639 (64)
Adj. Wald test: P val	0.01	0.00	0.00	0.00
δ1-ρ1	0.01	0.01	0.00	0.03
P val: δ1=ρ1	0.66	0.71	0.81	0.19

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father's education, exposure to television, mosque attendance. # villagers known is the total number of six randomly selected villagers whom the respondent knew.

Together, although relatively weak in magnitude and in precision, results from these two tables indicate that there are effects both through individuals watching the documentary themselves and through social interactions afterwards. This is despite a relatively soft intervention, evaluated six months later on such attitudinal factors as aspirations (future-oriented preferences). Similar effects are also found using a slightly modified version of the aspiration index, wherein individuals were this time asked about the level they thought they would reach within ten years, for each of the considered dimensions. For sake of brevity, the corresponding table is reported in Appendix. In all following estimations, peer effects are measured through the measure used in Table 8, that is, the number of treated (placebo) individuals amongst the individual's four closest peers.

3.3 Heterogeneity of treatment effects

Next, we investigate whether direct and indirect treatment effects on aspirations differ by demographic characteristics, baseline aspiration levels and the intensity of treatment at village level. Results are presented in Table 10. In the first two columns, we split the sample into those who have an age below the median age, in the first column, and above the median age, in the second column. We find that younger individuals have a slightly larger direct treatment effect than in the sample as a whole. Older individuals do not significantly revise their aspirations if they are treated, suggesting that those who are younger are potentially more open to revising their aspirations. We find no evidence of heterogeneous response to treatment with respect to gender and education (results not shown).

In the second two columns, we assess heterogeneity with respect to initial levels of aspirations by splitting the sample into those individuals whose baseline responses were above or below the median level of aspirations. Individuals with above-median initial levels of aspirations increase their aspirations if they are treated, and end up with aspiration about 14 percent standard deviation higher than similar individuals within the control group. Furthermore, the average indirect treatment effect – through peers – is mostly determined on those individuals with higher aspirations to start with. In other words, individuals with low baseline aspirations are not affected by direct treatment or treatment of their peers. Together, these results therefore indicate that the type of “soft”, video-based intervention evaluated here is not sufficient to trigger attitudinal responses from those with very limited aspirations to start with.

Table 10: Heterogeneous treatement effects on aspirations with self-selected peers

	Age		Aspirations		Village treatment intensity	
	Below	Above	Below	Above	High	Low
Treated individual	0.07^{***}	-0.01	0.00	0.07^{**}	0.05	0.02
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Placebo individual	0.03	0.01	0.01	0.05	0.04	0.00
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
# peers treated	0.01	0.01	-0.00	0.02^{**}	0.00	0.04^{***}
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
# peers placebo	-0.01	0.02	0.01	-0.02	-0.03	0.00
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Treatment village	-0.01	0.01	0.02	-0.03		
	(0.04)	(0.04)	(0.04)	(0.03)		
Baseline level	0.09^{*}	0.15^{***}	0.17^{***}	0.02^{*}	0.14^{***}	0.06^{*}
	(0.04)	(0.05)	(0.05)	(0.01)	(0.05)	(0.03)
Constant	0.01	-0.01	-0.01	0.07	-0.09	0.05
	(0.07)	(0.12)	(0.12)	(0.07)	(0.14)	(0.06)
Screening site F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Respondents (Villages)	901 (64)	741 (64)	833 (64)	809 (64)	827 (32)	815 (32)
Adj. Wald test: P val	0.00	0.00	0.00	0.00	0.00	0.00
$\delta 1$-$\rho 1$	0.03	-0.02	-0.01	0.02	0.01	0.02
P val: $\delta 1$=$\rho 1$	0.20	0.58	0.77	0.30	0.69	0.42

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father’s education, exposure to television, mosque attendance. # villagers known is the total number of six randomly selected villagers whom the respondent knew.

Finally, we examine whether treatment effects differ across high and low intensity treatment villages. Recall that our network measure is only partial, in that we limit ourselves to only four peers. Clearly, individuals may talk to many more individuals in reality, some of whom may have been invited to documentaries. Further, there may be room for second-order interactions, in that one who has seen documentary may have discussed it with friends, who may have in turn discussed with other friends. Thus, let ${n_i}^{T*}$ measure the number of one’s peers that have effectively been invited to a documentary session. Our network measure only captures some of them. Let $\theta_i^T = n_i^{T*} - n_i^T \geq 0$ measure the number of peer interactions that are not captured by our indicator.

Two remarks are in order. First, from our randomized design and because all individuals were only asked about the four people they knew best, θ_i^T is independent from n_i^T , thereby satisfying the proxy assumption and leading to unbiased estimate of our parameter (Chen, Hong, Nikepelov, 2011). Second, θ_i^T may positively depend on the total number of invitations to treatment distributed in a given village. With a low number of invitations distributed, it is unlikely that all individuals within a given village be indirectly affected by peers’ direct exposure to treatment. If a large number of invitations are distributed however, the probability that all individuals – even those with $n_i^T = 0$ – will be affected is larger (see Baird et al. (2012) for discussion of spillover measurements using variable saturation designs).

In our context, while a larger number of invitations provided in a given village shifts the distribution of n_i^T upward, it may also positively shift the distribution of θ_i^T, and hence the probability that all individuals be indirectly affected by treatment. The extent to which this may positively or negatively affect our parameter estimate depends on the relative magnitude of these two effects. In other words, absent pure control observations (i.e. observations immune from any potential spillovers), an absence of effect may reflect two very different situations, one of no actual spillover and one of sufficiently important spillovers that most individuals have been indirectly exposed to treatment.

We test for the presence of such effects using the variation in village-level treatment intensity, in Table 10. We find no a priori evidence of peer effects in high-intensity villages, but clear evidence of such effect in low intensity villages. To further assess whether no apparent effects in high-intensity villages is indeed due to largely diffused spillover effects, we test for difference in means in baseline and endline aspirations between high-intensity and low-intensity villages, on the restricted sample of individuals with $n_i^T = 0$. While no differences are found at baseline, results suggest higher and significant endline aspirations in high-intensity villages, of 0.05 units of aspirations (corresponding to about 10 percent in baseline standard deviation), thus suggesting that the lack of apparent effect in high-intensity of treatment villages is in fact due to sufficiently large spillovers that most respondents were in the end affected by the intervention. This further reinforces results from Tables 8 and 9 showing that aspirations are responsive to peer-level treatment.

3.4 Component-specific treatment effect

In Table 11, we investigate the direct and indirect effect of treatment on the various components of the aspiration index. Each column reports a separate estimate akin to Equation 2.3 for Income, Wealth, Education and Social Status aspirations respectively. Results show strong and positive direct and indirect effects on aspirations towards children's education, and no such effects on any other dimensions.

Table 11: Components of aspirations index with self-selected peers

	Aspirations index	Income	Wealth	Education	Social status
Treated individual	0.03^{*}	-0.00	0.00	0.14^{**}	-0.01
	(0.02)	(0.00)	(0.00)	(0.06)	(0.04)
Placebo individual	0.02	0.00	0.00	0.06	0.02
	(0.02)	(0.00)	(0.00)	(0.06)	(0.04)
# peers treated	0.01	-0.00	0.00	0.03	-0.00
	(0.01)	(0.00)	(0.00)	(0.03)	(0.03)
# peers placebo	-0.00	-0.00	-0.00	-0.02	0.01
	(0.01)	(0.00)	(0.00)	(0.03)	(0.02)
Treatment village	-0.00	-0.00	-0.00	-0.01	-0.02
	(0.03)	(0.00)	(0.00)	(0.07)	(0.08)
Baseline level	0.11^{***}	-0.00	0.00	0.16^{***}	0.17^{***}
	(0.04)	(0.00)	(0.00)	(0.03)	(0.06)
Constant	-0.02	-0.04^{***}	-0.04^{***}	-0.02	0.06
	(0.08)	(0.00)	(0.00)	(0.18)	(0.19)
Screening site F.E.	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Respondents (Villages)	1642 (64)	1642 (64)	1642 (64)	1642 (64)	1642 (64)
Adj. Wald test: P val	0.00	0.00	0.00	0.00	0.00
$\delta 1$-$\rho 1$	0.01	-0.00	-0.00	0.08	-0.03
P val: $\delta 1$=$\rho 1$	0.61	0.58	0.68	0.18	0.47

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father's education, exposure to television, mosque attendance. # villagers known is the total number of six randomly selected villagers whom the respondent knew.

A further interesting aspect of these results is that none of the four documentaries featured a character with formal education, nor did they mention literacy or education in explaining their success. Together, these results suggest that the information displayed in the documentaries, however useful, is not the main driving factor, and in turn suggest a deeper change in individuals' future-oriented perceptions.

This effect on parents' aspirations for children education is plausible. Schools are relatively accessible in Ethiopia: since 1995/6, fees have not been charged for the first eight years of primary school. Government has dramatically improved access to primary schools: 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 on average 25 minutes walk from the nearest primary school. Households are also likely to have been exposed to efforts to improve awareness of the importance of primary education, encourage children to enrol and encourage households to contribute to local schools. Since 2002, district and village authorities and parent/teacher committees have been tasked with increasing enrolment and undertake activities such as going door-to-door over summer holidays to encourage children to register. School management committees also solicit funds from community members (Garcia and Rajkumar, 2008)

4 Effect of treatment on future-oriented behaviour

We then assess the direct and indirect effect of treatment on future-oriented behaviour. Specifically, we assess effects onto savings and credit behaviour, time allocation (measured by the time dedicated to income-generating activities compared to the time allocated to leisure), school enrolment of children and expenditure on children's education.

In Table 12, we report on balancing tests for the corresponding outcome variables, at baseline. The first group of variables are at individual level and were collected for both the head and the spouse. The next set of variables were collected at household level. There are differences between the treatment and control groups in expenditure on children's schooling, and between all three groups in the amount respondents would ask for in a hypothetical loan due in one year.

Table 12: Experimental integrity: outcome variables

	Total	Treatment	Placebo	Control	T-C	P-C
	Mean (Standard Deviation)				Difference	p-value
Baseline outcome variables at individual level (n=1943)						
Average daily time in work (hours)	5.587	5.620	5.614	5.528	0.657	0.684
	(3.771)	(3.628)	(3.845)	(3.835)		
Average daily time in leisure (hours)	12.804	12.788	12.638	12.988	0.340	0.094*
	(3.759)	(3.683)	(3.741)	(3.847)		
Has any cash savings	0.227	0.253	0.212	0.217	0.123	0.830
	(0.419)	(0.435)	(0.409)	(0.413)		
Total savings (birr)	69.085	78.200	56.655	72.888	0.821	0.446
	(396.407)	(417.1422)	(345.211)	(423.737)		
Baseline outcome variables at household level (n=768)						
Proportion of children aged 7-15 in school	0.413	0.412	0.421	0.405	0.834	0.631
	(0.376)	(0.374)	(0.377)	(0.377)		
Expenditure on children's schooling (birr)	210.818	228.561	228.798	175.311	0.015***	0.019***
	(262.632)	(275.503)	(228.251)	(216.644)		
Hypothetical loan repayable in 1 year ('000 birr)	5.593	5.58	5.142	6.039	0.380	0.072*
	(5.537)	(5.520)	(4.599)	(6.308)		
Hypothetical loan repayable in 5 years ('000 birr)	12.661	12.100	12.891	13.011	0.497	0.935
	(15.672)	(14.106)	(16.603)	(16.302)		
Hypothetical loan repayable in 10 years ('000 birr)	23.093	20.143	26.539	22.776	0.291	0.305
	(25.831)	(22.655)	(49.976)	(32.855)		

* p below 0.1; ** p below 0.05; *** p below 0.01

4.1 Savings and credit behaviour

Respondents were asked how much they currently had saved in each of four savings places. This could include savings at banks, in a co-operative, with a voluntary savings and loan group or an iqqub, with a friend or relative or at home.

If households aspired to improve their income, they would probably need to alter their productive activities rather than generate more income by diversifying into non-agricultural activities, as a number of the people featured in the documentaries did. However, it is unlikely that households would have had enough time to diversify their activities in the six months between baseline and endline.

Baseline levels of savings are rather low, with 78 per cent of the respondents having no cash savings of any sort. Across all three groups, the average stock of savings per individual amounted to 69 ETB – roughly USD 6 at the time of the survey. Further, nearly 80 per cent had not deposited any cash savings of any sort over the previous month, leading to an average savings per month of 37 ETB – about USD 4.

Table 13: Savings behaviour with self-selected peers

	Has savings			Total savings		
	(I)	(II)	(III)	(I)	(II)	(III)
Treated individual	0.03	0.03	0.03	77.89	77.38	66.69
	(0.02)	(0.02)	(0.02)	(50.16)	(49.46)	(47.52)
Placebo individual	0.03	0.03	0.03	−17.34	−15.73	−6.06
	(0.02)	(0.02)	(0.02)	(32.67)	(32.69)	(32.00)
# peers treated		−0.01	0.00		−10.50	−20.00
		(0.01)	(0.01)		(23.55)	(23.63)
# peers placebo		−0.02**	−0.01		7.38	3.30
		(0.01)	(0.01)		(18.37)	(18.51)
Treatment village		−0.03	−0.04		34.72	32.55
		(0.03)	(0.03)		(35.28)	(34.62)
Baseline level	0.51***	0.49***	0.39***	0.03	0.03	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	0.15***	0.20***	0.26***	152.49***	260.96**	91.86
	(0.02)	(0.04)	(0.06)	(27.78)	(108.30)	(130.42)
Screening site F.E.	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents (Villages)	1981 (64)	1981 (64)	1981 (64)	1973 (64)	1973 (64)	1973 (64)
Adj. Wald test: P val	0.00	0.00	0.00	0.19	0.00	0.00
$\delta 1$-$\rho 1$	-0.00	0.00	0.00	95.23	93.10	72.75
P val: $\delta 1$=$\rho 1$	0.97	0.95	0.92	0.05	0.06	0.10

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father's education, exposure to television, mosque attendance. # villagers known is the total number of six randomly selected villagers whom the respondent knew.

Thus, we investigate whether one's direct and indirect exposure to the documentaries has affected whether individuals have any savings and the the amount of those savings. We follow the same estimation strategy as for aspiration related outcomes, and rely on the defintion of peer's network used in Table 8.

In the first three columns of Table 13, we do not find any effect on the probability that an individual has savings. In the second three columns, we examine average savings across the sample, with individuals who do not have savings coded as having zero savings. We find evidence of positive direct effect of treatment, while the effect of placebo is negative. Although precision on the estimated coefficients is low, the reported Wald tests indicate a statistically significant difference between the two coefficients. Peer effects are however inconclusive, with none of the coefficients statistically different from zero.

Relatively similar effects are found with respect to the use of credit, as reported in Table 14. Here also, while not individually significant, coefficients on direct treatment effects are systematically positive and significantly different from coefficients on direct placebo effect. Peer effects are however inconclusive.

Finally, we examine hypothetical demand for credit by asking household heads how much they would borrow if given the opportunity. In fact, examination of actual borrowing behaviour faces the difficulty that there are extensive market failures in the credit markets to which respondents have access, making credit relatively difficult to obtain. This question sought to assess whether respondents' demand for credit in the absence of these market failures. Respondents were asked:

> Someone from a microfinance 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?

Findings are highly suggestive, given that the answers are only hypothetical, so participants do not have to be particularly realistic. Any treatment effects suggest only that respondents who saw the documentary were considering activities that might require use of credit and do not give any estimate of respondents' actual demand for credit.

The descriptive statistics show that the amounts individuals would borrow increase with the length of the repayment period. This is consistent with the finding that loan size is responsive to changes in loan maturity (Karlan and Zinman, 2005). A large proportion of household heads are not interested in taking any loans, and the amount increases as the length of the repayment period increases, suggesting that respondents' hypothetical borrowing may be constrained by uncertainty about their future economic status.

Table 14: Use of credit with self-selected peers

	Took out credit more than 15 birr			Total credit more than 15 birr		
	(I)	(II)	(III)	(I)	(II)	(III)
Treated individual	0.03	0.03	0.03	16.49	16.97	17.59
	(0.03)	(0.03)	(0.03)	(10.77)	(10.76)	(10.78)
Placebo individual	−0.02	−0.02	−0.02	−3.37	−2.28	−2.68
	(0.02)	(0.02)	(0.02)	(10.68)	(10.67)	(10.77)
# peers treated		−0.01	−0.00		−2.22	−2.32
		(0.01)	(0.01)		(5.44)	(5.60)
# peers placebo		0.00	0.01		0.41	0.36
		(0.01)	(0.01)		(6.42)	(6.35)
Treatment village		0.00	0.00		−5.14	−3.42
		(0.02)	(0.02)		(11.77)	(11.50)
Baseline level	0.20^{***}	0.19^{***}	0.19^{***}	0.14^{***}	0.13^{***}	0.13^{***}
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Constant	0.26^{***}	0.34^{***}	0.42^{***}	76.76^{***}	97.04^{***}	121.10^{***}
	(0.02)	(0.03)	(0.05)	(7.73)	(18.05)	(24.15)
Screening site F.E.	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Respondents (Villages)	1981 (64)	1981 (64)	1981 (64)	1924 (64)	1924 (64)	1924 (64)
Adj. Wald test: P val	0.00	0.00	0.00	0.00	0.00	0.00
$\delta 1$-$\rho 1$	0.05	0.05	0.05	19.87	19.25	20.26
P val: $\delta 1=\rho 1$	0.07	0.07	0.05	0.11	0.12	0.10

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father's education, exposure to television, mosque attendance. # villagers known is the total number of six randomly selected villagers whom the respondent knew.

Table 15: Hypothetical demand for credit self-selected peers

	Loan repayable in one year		Loan repayable in five years		Loan repayable in ten years	
	(I)	(II)	(III)	(III)	(II)	(III)
Treated individual	1740.40	1618.31	4175.59^{**}	3823.66^{**}	9319.75^{**}	8765.58^{**}
	(1289.86)	(1255.71)	(2057.87)	(1874.93)	(3818.16)	(3655.61)
Placebo individual	1736.25	1745.12	−734.04	−485.38	−2606.06	−2102.44
	(1192.45)	(1179.84)	(2075.07)	(1897.91)	(3626.26)	(3365.98)
# peers treated	631.33	434.37	148.56	−179.98	17.94	−475.55
	(1005.61)	(954.01)	(982.33)	(970.16)	(2531.15)	(2354.07)
# peers placebo	821.41	810.16	1103.29	1163.44	1543.01	1554.65
	(694.06)	(684.93)	(1028.43)	(976.90)	(1773.86)	(1656.68)
Treatment village	2279.88^{*}	2117.14	635.18	−51.40	−106.61	−1255.74
	(1264.82)	(1305.59)	(1972.24)	(1980.60)	(3445.72)	(3466.80)
Baseline level	0.98^{***}	0.86^{***}	0.60^{***}	0.52^{***}	0.36^{***}	0.31^{***}
	(0.18)	(0.17)	(0.09)	(0.08)	(0.09)	(0.08)
Constant	3865.69	200.06	14527.39^{***}	12644.09^{*}	24558.53^{***}	16641.35
	(4233.90)	(4645.42)	(5033.83)	(6953.23)	(8687.75)	(10958.69)
Screening site F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Households (Villages)	802 (64)	802 (64)	795 (64)	795 (64)	790 (64)	790 (64)
Adj. Wald test: P val	0.00	0.00	0.00	0.00	0.00	0.00
$\delta 1$-$\rho 1$	4.15	-126.81	4909.63	4309.05	11925.81	10868.03
P val: $\delta 1=\rho 1$	1.00	0.91	0.01	0.01	0.00	0.00

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father's education, exposure to television, mosque attendance. # villagers known is the total number of six randomly selected villagers whom the respondent knew.

We present results for estimates of Equations II and III, for each hypothetical loan maturity in Table 15. We find positive significant direct effects of the intervention on the amounts treatment individuals would ask for in five and ten years compared to the control group, but no indirect effects of the number of peers who saw the treatment. There is no effect on individuals who saw the placebo.

Overall results from Tables 13, 14 and 15 are remarkably consistent, pointing to a direct effect of treatment on the use of financial instruments (whether savings or credit) while no such effects are found for placebo. In all cases however, no indirect effect of treatment - that is through peers' exposure to treatment - is uncovered.

4.2 Time allocation

We then turn to assessing direct and indirect effect of the intervention on time allocation to work and leisure. The household head was asked to report amount of time each member of the household spent on a typical day during March (in 2010 in the baseline and 2011 in the follow-up, as shown in Table 16). In particular, respondents were asked to evaluate the typical daily amount of time dedicated to working on farm and/or business, as well as time dedicated to leisure (including eating, bathing and sleeping).

Table 16: Calendar for time allocation and education questions

Date	Event
September 2009	Start of 2009-10 school year; date examined for baseline enrolment question
March 2010	Period examined for baseline time allocation question
August-October 2010	Baseline and experiment
September 2010	Start of 2010-11 school year; date examined for endline enrolment question
March 2011	Period examined for endline time allocation question
April-June 2011	Endline survey

We use the time allocations reported for the household head and spouse and run the regression at individual level. There is some potential for measurement error: most household heads were men, so their reports of their own time allocation will probably be more accurate than their reports of their spouse's time allocation. This measurement error will largely be captured by the gender control. We nonetheless focus on time spent working on the farm, when spouses would be more likely to be working together. We do not examine time spent working in the home, which is difficult to measure because it is extremely fragmented and would probably be poorly estimated by men for their spouses.

At baseline, individuals spent an average of 5.6 hours in farm work, and 12.8 hours for leisure on a typical day, across all three groups. As shown in the calendar in Table 16, there was more of a gap between the baseline and the period examined in the baseline question than between the follow-up and the period examined in the follow-up question. However, figures are not significantly different in the control group between baseline and endline, which suggests that bias due to recall is minimal.

In Table 17, we report direct and indirect treatment effects on the time individuals dedicate to work. We do not find evidence of direct or indirect effects of watching the treatment or the placebo. However, results suggest that in villages which were more intensively treated, surveyed individuals increased the amount of time they spent in work by 0.07 standard deviations (13.58 minutes, 4.05 per cent) and decreased the amount of time they spent in leisure by 0.12 standard deviations (27.06 minutes, 3.52 per cent) compared to villages which were intensively treated with the placebo treatment.

4.3 Children's education

Finally, we examine effects on children's enrolment in school and the amount spent on their schooling. These were asked only to the household head at baseline and endline. Specifically, one of the outcome variables we examine is the proportion of children in the household between the ages of 7 and 20 who are enrolled in school. Enrolling or re-enrolling children in school and spending on schooling for children is not a meaningful decision for families with no children. We thus report results only for the sample of 819 households who have children between 7 and 20 in the household. At baseline, 31 per cent of children in this age group had never been enrolled in school and 28 per cent of households with children in this age group had no children enrolled.

In the first three columns of Table 18, we investigate whether our intervention affected children's enrolment in school. We examine the proportion of children in the household between the age of 7 and 20 who are enrolled in school. We do not examine children younger than 7 as children are supposed to enrol in Grade 1 when they have turned 7. Only primary school, Grade 1 to 8, is compulsory. If children have a smooth schooling trajectory, they should be enrolled until either 14 or 15, when they will have reached Grade 8. However, in practice many children stay enrolled after this time because late enrolment or slow grade progression has delayed their progress.

Table 17: Time in work and leisure with self-selected peers

	Time in work			Time in leisure		
	(I)	(II)	(III)	(I)	(II)	(III)
Treated individual	4.49	4.16	3.29	5.66	7.16	2.46
	(9.68)	(9.74)	(8.68)	(12.54)	(12.57)	(12.33)
Placebo individual	-2.64	-3.97	0.27	10.91	12.95	8.71
	(10.79)	(11.00)	(8.91)	(12.30)	(12.44)	(12.24)
# peers treated		13.44^{**}	2.57		2.07	-4.00
		(5.81)	(3.94)		(5.53)	(5.42)
# peers placebo		8.79	5.36		-3.50	-6.96
		(5.64)	(4.25)		(6.24)	(5.71)
Treatment village		8.32	14.58		-22.44^{*}	-24.43^{*}
		(13.12)	(8.96)		(13.34)	(12.37)
Baseline level	0.58^{***}	0.62^{***}	0.12^{***}	0.18^{***}	0.24^{***}	0.09^{**}
	(0.04)	(0.02)	(0.03)	(0.05)	(0.04)	(0.04)
Constant	117.53^{***}	73.37^{***}	102.26^{***}	662.42^{***}	630.60^{***}	566.67^{***}
	(18.76)	(22.64)	(18.88)	(38.63)	(34.71)	(34.42)
Screening site F.E.	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents (Villages)	1952 (64)	1952 (64)	1952 (64)	1951 (64)	1951 (64)	1951 (64)
Adj. Wald test: P val	0.00	0.00	0.00	0.01	0.00	0.00
$\delta 1$-$\rho 1$	7.13	8.13	3.02	-5.25	-5.79	-6.26
P val: $\delta 1 = \rho 1$	0.48	0.42	0.75	0.66	0.62	0.59

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father's education, exposure to television, mosque attendance. # villagers known is the total number of six randomly selected villagers whom the respondent knew.

Table 18: Investment in children's education with self-selected peers

	Proportion of children enrolled			Education spending		
	(I)	(II)	(III)	(I)	(II)	(III)
Treated individual	0.04	0.04	0.04	11.82	-0.54	1.26
	(0.03)	(0.03)	(0.03)	(22.99)	(21.52)	(20.76)
Placebo individual	0.04	0.04	0.04	6.28	-0.08	5.38
	(0.03)	(0.03)	(0.03)	(20.39)	(19.17)	(18.85)
Baseline level	0.40^{***}	0.38^{***}	0.36^{***}	0.45^{***}	0.27^{***}	0.21^{***}
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
# peers treated		0.04^{***}	0.04^{***}		35.04^{***}	32.21^{***}
		(0.01)	(0.01)		(12.69)	(11.67)
# peers placebo		-0.01	-0.01		4.64	6.78
		(0.02)	(0.02)		(9.23)	(9.23)
Treatment village		-0.03	-0.03		-27.23	-30.05
		(0.03)	(0.03)		(22.19)	(20.90)
Constant	0.48^{***}	0.63^{***}	0.58^{***}	140.67^{***}	48.62	-103.24
	(0.03)	(0.05)	(0.11)	(14.33)	(36.90)	(71.36)
Screening site F.E.	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Households (Villages)	819 (64)	819 (64)	819 (64)	789 (64)	789 (64)	789 (64)
Adj. Wald test: P val	0.00	0.00	0.00	0.00	0.00	0.00
$\delta 1$-$\rho 1$	-0.01	-0.00	-0.00	5.54	-0.46	-4.12
P val: $\delta 1 = \rho 1$	0.80	0.91	0.88	0.82	0.98	0.85

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father's education, exposure to television, mosque attendance. # of children aged 7-20 in household.
villagers known is the total number of six randomly selected villagers whom the respondent knew.

Table 19: Topics discussed by respondents with their peers

Number with whom the respondent had discussed	Farming or business		Savings or credit	
	Number	Percentage	Number	Percentage
0	650	39.59	1,231	74.97
1	308	18.76	197	12.00
2	238	14.49	96	5.85
3	192	11.69	66	4.02
4	114	6.94	28	1.71
5	76	4.63	10	0.61
6	64	3.90	14	0.85
Total	1,642	100	1,642	100

Respondents were asked whether they had discussed these topics over the last 12 months with each of 6 randomly selected villagers.

We examine the proportion of children in the household who were enrolled in school at the beginning of the 2009/10 school year (at baseline) and the 2010/11 school year (at endline). Again, the endline measure suffers from some measurement error. The school year starts on September 1st, but households are often able to enrol their children in school until the end of October because of government efforts to enrol as many children in school as possible. However, the households who received the intervention towards the end of the treatment period might not have been able to enrol their children in school even if they had wanted to. These effects may underestimate the true effect of the intervention on enrolment.

We find significant indirect effects on enrolment: the proportion of children in the household between 7 and 20 who are enrolled increases by 4 percentage point (a 10% increase from the baseline average), for each of the parents' peers who sees the documentary. We do not however find evidence of a clear direct effect of treatment.

Second, we examine standardised total 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. Primary schooling in Ethiopia is free, but households have to cover schooling expenses and are often asked to contribute voluntarily to the school.

Results for this variable should be treated with some caution. Firstly, 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. Secondly, there are significant differences between the three groups at baseline, with households in the control group spending significantly less on education than households in the treatment and placebo groups.

We find no direct effect of the treatment on expenditure. However, we find a positive significant indirect effect of 0.13 standard deviations. For every extra peer of the parents who is treated, households increase their spending on children's education by 32 birr, or 16.6 per cent. No such pattern is found for indirect effect of placebo.

Overall, results from this section call for the following observations. First, despite a relatively soft intervention - a one hour documentary screening - we find clear evidence of behavioral changes, six months following treatment. Second, different behaviors seem to be affected through different channels. In effect, we mostly find direct effect of treatment onto issues related to savings and credit, and indirect effect on issues related to children education. This may in part reflect the fact that savings and credit are less likely observed and discussed between peers. This is apparent in Table 19, which shows that people tend to discuss farming issues much more than they discuss financial issues. It may also reflect different response to peers' behavior, in particular for issues that involve a degree of altruism vis a vis later generations, through enhanced peer pressures. Third, these results are in line with those of Section 3. In fact, despite nothing related to education in the screened documentaries, the clearest effects are found in relation to children's education, whether it is through changes in aspirations or through changes in actual behavior. This in turn supports an effective causal pathways between the two sets of results.

5 Conclusion

Using an innovative experimental design, this paper has attempted to test for the existence of informational and peer effects in the formation of aspirations. Despite a relatively "soft" treatment intervention, in the form of a one hour screening of documentaries on small success stories in rural Ethiopia, our results point to significant improvements in individuals' aspirations measured six months later. We further show 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. Results are robust to a symmetrical placebo experiment and alternative measures of attitudes towards the future. We also show that treatment effects are higher for younger adults, and for those individuals with above-median baseline aspirations. They do not differ by gender or level of education. 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 education.

We also assess the reduced-form effect of our intervention onto individuals' actual behavior. 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 response to a hypothetical demand for loan question, which we find positively impacted by direct treatment effect. In all financial outcome variables however, we do not uncover any evidence of indirect effect as mediated through one's peers' exposure to documentary. In contrast, we find no direct effect of treatment onto behavior related to time allocation and children education. There is however consistent evidence of indirect effects, through peers' exposure to documentary which can be relatively large in magnitude. Each treated peer is associated with a 10% increase in the share of children enrolled in school, and a 16% increase in educational spending.

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 above and beyond that of learning from others' behaviour and corresponding socio-economic returns. Our results imply partly collectively determined aspirations, in line with recent literature on culture (e.g. Rao and Walton, 2004), identity (e.g. Akerlof and Kranton, 2002) and poverty. Together, these results further contribute to the growing economic literature on the formation of aspirations (e.g. Dercon and Krishnan (2009); Macours and Vakis (2009); Beaman et al. (2012)) and their importance for future-oriented behaviour and well-being outcomes. Our results also warrant further research with respect to intergenerational aspirations, as well as specific direct/indirect channels to affect particular types of behaviour.

As a side contribution, these results further confirm findings by a recent and growing empirical literature on the effectiveness of video-based interventions to affect perceptions and behaviours (see for instance Berg and Zia (2013) on financial education and financial behaviour in South Africa, Jensen, R. 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, and perhaps in the spirit of nudges where individuals are given mere directions for behaviour instead of actual resource transfers, our results call for an increased attention to the role of perceptions and the mechanisms underlying their formation.

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Appendix 1: Biographies of two people featured in the documentaries

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 on her being the major bread winner 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.

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 consulted an agricultural expert in a local NGO 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 his farming area. Rather than using buckets to water his farm, the use of a pump made watering a larger area much easier. 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.

Appendix 2: Alternate phrasing of aspirations question

Table 20: Alternative phrasing of aspirations question with self-selected peers

	(I)	(II)	(III)	(IV)
Treated individual	0.05^{**}	0.05^{**}	0.05^{**}	0.08^{***}
	(0.02)	(0.02)	(0.02)	(0.03)
Placebo individual	0.03	0.03	0.03	0.03
	(0.02)	(0.02)	(0.02)	(0.03)
# peers treated		0.03^{**}	0.02^{*}	0.03^{**}
		(0.01)	(0.01)	(0.01)
# peers placebo		0.02	0.02	0.02
		(0.01)	(0.01)	(0.02)
Treatment village		−0.01	−0.01	−0.01
		(0.05)	(0.05)	(0.05)
Baseline level	0.09^{**}	0.09^{**}	0.08^{**}	0.08^{**}
	(0.04)	(0.04)	(0.04)	(0.04)
Treated indiv. * # peers treated				−0.02
				(0.02)
Placebo indiv. * # peers treated				−0.01
				(0.02)
Treated indiv. * # peers placebo				−0.02
				(0.03)
Placebo indiv. * # peers placebo				0.00
				(0.02)
Constant	−0.03	−0.07	−0.14	-0.15^{*}
	(0.02)	(0.10)	(0.09)	(0.09)
Screening site F.E.	*No*	*Yes*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*Yes*
Respondents (Villages)	1642 (64)	1642 (64)	1642 (64)	1642 (64)
Adj. Wald test: P val	0.02	0.17	0.00	0.00
$\delta 1$-$\rho 1$	0.02	0.03	0.02	0.05
P val: $\delta 1 = \rho 1$	0.35	0.30	0.34	0.03

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parenthesis.
t stats and adjusted Wald tests are used.
Unreported controls for age, gender, education, marital status, asset-value, father's education, exposure to television, mosque attendance. # villagers known is the total number of six randomly selected villagers whom the respondent knew.