The Role of Aspirations and Identities in Decisions to Invest in Children's Schooling



Kate Orkin
Oriel College
University of Oxford

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Abstract

I demonstrate that household investments in children's education in Ethiopia are affected by parents' self-beliefs (such as their locus of control), parents' aspirations for children's educational attainment, children's conceptions of their roles and identities in the household and at school, and children's own preferences, all concepts not widely studied in development economics.

Two empirical chapters report on a field experiment in which randomly selected adults watched documentaries about role models who were poor but succeeded in agriculture or small business. Six months later, parents' self-beliefs and aspirations for children's education were higher in the treatment than in the placebo and control groups. Enrolment of children in school, spending on education, saving and use of credit also increased.

A third empirical chapter draws on longitudinal qualitative research to argue that children's preferences for their time allocation between work and school are strongly influenced by the desire to comply with valued identities as students and as independent earners and contributors to the household.

The fourth chapter suggests that understanding children's preferences might improve predictions about their reaction to education policies. The literature predicts an increase in time in school will not improve test scores: children will reduce effort because they desire a limited amount of learning. I find a reform to lengthen the Ethiopian primary school day improves test scores. Although this could occur through many mechanisms, one possibility is that children do not prefer to limit their desired amount of learning. This suggests that better evidence on children's preferences might improve prediction of the effects of policies to alter school inputs.

The conclusion reflects on whether the empirical relevance of concepts of self-beliefs, aspirations and identities implies that assumptions in standard models of decision-making in economics about the characteristics of beliefs and preferences ought to be rejected. I argue that these ideas can be captured by existing economic concepts of beliefs and preferences and by standard assumptions about these concepts. I suggest that, contrary to recent accounts building on human capital theory, self-beliefs should be viewed as beliefs, not non-cognitive skills. I consider aspirations as a type of preference, shaped by both objective constraints and self-beliefs. I consider identity as a preference for complying with a social role, but highlight that such preferences are often altruistic, rather than self-interested. In conclusion, I argue that economics should draw further on other social sciences, including psychology, to develop substantive theories of the formation and characteristics of beliefs and preferences. Doing so will suggest when it is appropriate to apply standard models and how their assumptions can be modified if their predictions do not hold.

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1

Introduction

Our childhood was a time of illiteracy, but now education is expanding...Children will be civilised and improved only if they learn.

– The mother of Hassen, a boy, Leki, rural Oromia, Ethiopia, 2008

I want to go to school to be a teacher because our country is yet to develop. I want to train children so that they become self-supportive.

– Tatek, a 15-year-old boy, in the nearby town, 2010

Education will liberate our children. It will let them take themselves out from darkness and get themselves to light.

– The mother of Geloye, a girl, in the same village, 2008

In the 1950s, economists viewed households' spending on education as consumption of a good that gave immediate utility. Family purchases of education, like purchases of any other good, depended on tastes, family income and the costs of schooling. Economists had little to say about what types of investment in education made economic sense, as people's tastes for any consumption good were so idiosyncratic that generalisations could not be drawn. Economic models did not capture that families might see time and money spent on education as an investment in a better future which could alter children's lifetime earnings dramatically, nor that a major part of the sacrifice of going to school was forgoing current income.

I. Conceptual background

In October 1962, the *Journal of Political Economy* published a volume on "Investment in Human Beings", beginning the "human investment revolution in economic thought" (Bowman, 1966, 111). Gary Becker, Theodore Schultz, Jacob Mincer and the Chicago School began to model households as production units (Mincer, 1958; Schultz, 1960, 1963; Becker, 1965). Households "produce" children's skills by investing children's time and effort in schooling and spending money on school inputs. These economists view decisions by the household "firm" about the amount to spend on social services such as health and education not as consumption decisions, but decisions about the optimal level of investment in "human capital" stock – a stock of productive skills held by a human being – which yields a future income stream. As with other investments, households estimate the likely returns to education based on their beliefs about individual children's ability and prospects for earnings in the labour market. They weigh up the returns to the investment against the resources they have saved and can borrow and decide the optimal level of investment. The primary cost of education and job training is the opportunity cost of forgone earnings, rather than the costs of education.

These models of household decisions form part of a broader area of research on investments in nutrition, health and education known as human capital theory. They continue to be widely used and are highly influential, in part because they explain many economic behaviours which could not be explained by previous theories (Heckman, 2014).[1] Encouraged by the predictive success of human capital models, the Chicago School argue for the application of the "economic way of thinking" (Becker, 1976) to predict individuals' behaviour on average in areas outside the original remit of economics, from education, health and fertility to crime and punishment (Heckman, 2014). Economics, they argue, can unify and simplify the social sciences.

Their models rely on only a few, simple, assumptions about human preferences, beliefs and choices. These assumptions define the characteristics of the preferences, beliefs and choices of a hypothetical rational agent who makes justified, reasonable decisions. For example, the axioms of ordinal utility theory state that a rational agent's preferences are transitive,

[1] For example, firms tend to pay for specific but not general on-the-job training, because the latter is held by the employee and can be taken with them to a new job (Becker, 1975). People invest time in job search rather than accepting the first offer possible (Stigler, 1961; McCall, 1970). People migrate to access better employment opportunities (Fields, 1975) or take jobs with a lower wage but more potential to learn on the job.

complete, context-independent and stable over time. Agents are assumed to know everything about alternatives and form correct beliefs based on this information.

Neoclassical economists also make the appealing argument that human behaviour, on average, resembles the behaviour of their prototype rational agent (Hausman, 2012). Thus, these few, simple assumptions describe both rational choices and actual choices. With these assumptions about human beliefs and preferences, economists build formal mathematical models to describe human behaviour which apply in a variety of contexts, across historical time, and in many domains of behaviour.

A. Schultz's development economics: Poor but rational

Many anti-poverty strategies of the United Kingdom or the United States well into the nineteenth century assumed that poverty is caused by poor people's deviant values, character failings, abilities or inability to understand the benefits of important investments like education (Ravallion, 2015; Narayan et al., 2000). Many wealthier people continue to believe that people are poor because of their individual failings, such as a lack of work ethic and irresponsibility, in both developed (Feagin, 1972; Hunt, 2004) and developing countries (Chase and Walker, 2013; Walker, 2014). Many developing country systems of welfare, health and education provision stigmatise recipients and construe them as undeserving and responsible for their poverty (Chase and Bantebya-Kyomuhendo, 2014; Narayan et al., 2000).

Neoclassical development economists disagree with this view that poor people are to blame for their situation. They emphasise that their assumptions that human behaviour is, on average, rational apply to all people, rich and poor, in all cultural contexts, and that poor people, like all human beings, make decisions that optimise returns given the constraints they face and grab available opportunities (Schultz, 1960, 1963, 1964). For example, given binding constraints on budgets or credit, low returns to education, or poor quality schooling, poor people may, rationally, decide not to invest in children's education:

> Poor people are no less concerned about improving their lot and that of their children than rich people are...[G]iven the economic constraints that these families face and the costs they incur in acquiring schooling, and the benefits to be derived from additional schooling, it is invalid to infer that they are underinvesting in view of their circumstances (Schultz (1981, 2-5), cited in Singh (1992, 646)).

The perspective that poor people are rational and doing the best they can to optimise, given severe constraints, persists in much applied development microeconomics today:

> What is striking is that even people who are poor are just like the rest of us in almost every way. We have the same desires and weaknesses; the poor are no less rational than anyone else – quite the contrary. Precisely because they have so little, we often find them putting much more thought into their choices: they have to be sophisticated economists just to survive (Banerjee and Duflo, 2011, iii).

In this view, poverty is driven by the resource constraints that prevent poor people investing in endowments of assets, health, skills or education or by market failures that prevent optimal allocation of resources. Accordingly, many anti-poverty policies build up the poor's endowments of health, skills or education by providing cheap or free access to these services, enable asset accumulation through micro-finance, micro-credit or income transfers, or target market failures.

B. Psychological influences on investment decisions

However, the neoclassical model of decision-making and the policy prescriptions derived from the "poor but rational" perspective increasingly appear overly simplistic. In development economics in particular, many studies show that poor people make investment decisions that are not predicted by standard models and may perpetuate poverty: borrowing too much, saving too little, underinvesting in health and education, and not taking up opportunities to diversify their sources of income or increase the revenue of their farms and businesses (Banerjee and Duflo, 2011, 191). This has profound implications for our model of poverty eradication. More of the same interventions – more schools, clinics, agricultural extension officers – may not be sufficient. Development economists, it appears, require a better understanding of how people make decisions.

For example, in Ghana farmers do not diversify into high-return crops like pineapples, despite the potential to nearly double their income (Goldstein and Udry, 1999, 2008). Farmers in Kenya do not use fertilizer, although doing so would yield average returns (net of fertilizer costs) of fifteen per cent per year (Duflo, Kremer, and Robinson, 2008, 2011). In Bangladesh, residents do not migrate during the hungry season despite potentially high-return jobs in urban areas and poor availability of farm work (Bryan, Chowdhury, and Mobarak, 2012). This

might be explained by risk aversion among very poor households, as migrants may not find jobs, but the level of risk aversion needed to explain these results is implausibly high.

Chapter 2, the first part of the literature review, gives an overview of research about the demand side of the market for schooling in developing countries: in particular, household decisions about the amount of time children spend in school, the effort children put into learning, and how much parents invest in children's education. I highlight four patterns in parents' and children's behaviour in this literature that are not predicted by the standard model. Firstly, households in rural, primarily agricultural areas in developing countries invest in children's schooling despite potentially low returns to education, given the low likelihood of completing secondary school and finding a formal sector job. Secondly, although cost constraints are a major limit on investment in schooling, household choices about paying out-of-pocket schooling costs, such as buying textbooks and uniforms, are much more price-sensitive than the standard model predicts. Small changes in out-of-pocket costs result in large changes in enrolment. Thirdly, households given information about the returns to education often increase investment in education substantially, suggesting that they may lack information about these returns. However, confusingly, other evidence suggests households have quite good information on the returns to education. Finally, the behaviour of children's peers has strong effects on children's own choices about enrolment and attendance, even in situations where standard models predict it should have no effect.

To attempt to explain such anomalies, development economists, particularly in education, increasingly turn to a growing literature on how psychological factors affect investment decisions (Kremer and Holla, 2009, 514-15). Chapter 3, the second part of the literature review, describes two areas of research in economics which apply insights from psychology. Firstly, some human capital theorists draw on personality psychology, a strand of social psychology, which defines and measures personality traits and "non-cognitive skills". These economists consider such traits as part of the endowment of capabilities that affect an individual's productivity, life trajectory and propensity to sacrifice immediate consumption to make long-term investments (Heckman, Stixrud, and Urzua, 2006; Heckman and Kautz, 2012, 2013; Borghans et al., 2011; Lindqvist and Vestman, 2011; Almlund et al., 2011). By considering psychological characteristics as part of a person's endowment of skills, which in any case vary between individuals in the standard model, researchers can continue to use standard

neoclassical models of decision-making. In particular, these skills or traits are thought to be relatively stable and context-independent.

Secondly, in behavioural economics, economists collaborate with cognitive psychologists to design laboratory experiments that model actual preferences and beliefs (Tversky and Kahneman, 1974; Slovic and Lichtenstein, 1968; Lichtenstein and Slovic, 1973). Research in this area demonstrates that people have limited capacity to process information and rarely assess or absorb all the information available to them. They often make decisions automatically, using habits or rules of thumb, rather than through deliberate, careful comparison of options. Relatedly, they often do not optimise, but satisfice: they make quicker decisions that are good enough. Their preferences are not context-independent, stable or consistent, but differ depending on how decisions are framed. Preferences are often altruistic, rather than self-interested. Behavioural economists thus suggest substantial modifications to the assumptions that are made about beliefs and preferences in standard economic models.

Broadly speaking, one can identify two ways of integrating psychological concepts and findings into economic models. One path, taken by economists working on non-cognitive skills, does not really require modifications to the assumptions standard models make about beliefs, preferences and skills. Behavioural economists take another path. They continue to use concepts of preference, belief and skills, but argue for a number of modifications to the way the standard model characterises people's preferences, beliefs and skills and the assumptions it makes about them. Furthermore, they suggest that economists should, as far as possible, use empirical research to test generalisations about the formation and consequences of preferences, beliefs and skills, rather than making assumptions about how they behave.

II. Thesis outline and contribution

In this thesis, I examine one specific type of decision: household investments in children's education in Ethiopia. In more developed countries, many of these decisions are taken by the state. In developing countries, the state often has limited capacity and resources to provide services and impose regulations. While there is some pressure from local government to enrol children, Ethiopian parents and children largely decide when children start school; whether they stay enrolled each year; how regularly they attend; how much time they spend in other activities, like work, which may conflict with schooling; and how much is spent on their textbooks and stationery.

Social psychologists and sociologists suggest three aspects of people's conceptions of themselves that profoundly shape their choices, particularly about investment decisions which require forming beliefs about possible outcomes far in the future. One aspect is people's "self-beliefs": their beliefs about their abilities, capacities and worthiness. These include people's locus of control, which differentiates individuals with an internal locus who think outcomes are contingent on their own behaviour from individuals who have an external locus and are fatalistic (Levenson, 1974; Rotter, 1966); and people's self-esteem, their overall sense of worthiness as a person (Rosenberg, 1965). These general beliefs are closely related to people's sense of their own self-efficacy, their beliefs about their own ability to achieve more specific outcomes (Bandura, 1997).

Neoclassical economists working on non-cognitive skills often used self-esteem and an internal locus of control as proxies for non-cognitive skills because they were the only measures available in longitudinal datasets (Almlund et al., 2011; Heckman and Kautz, 2012). More recently, these researchers acknowledge that initial definitions of non-cognitive skills were overly broad and that self-esteem and locus of control are not particularly good proxies for these skills. They have begun to focus more narrowly on personality traits as a subset of non-cognitive skills and have examined personality traits relatively thoroughly (Almlund et al., 2011; Borghans, ter Weel, and Weinberg, 2008; Camfield, 2010). Self-beliefs have not received the same thorough theoretical examination as personality traits.

A second aspect of people's conceptions of themselves is their aspirations. I consider aspirations as outcomes that people prefer among a set of outcomes they believe to be attainable. Behavioural economists are the only economists who have really considered aspirations (Selten, 1998; Ray, 2006; Dalton, Ghosal, and Mani, 2015). They argue that past outcomes act as a reference point which frame people's future aspirations and how they invest effort and resources. Social psychologists, in contrast, see aspirations as goals which are shaped by people's beliefs about objective constraints, available resources, and their past performance, but also by their beliefs about their own capabilities (Locke and Latham, 2002; Bandura and Locke, 2003).

The third aspect of self-beliefs is people's identity, their social position in a larger social structure in relation to others (Stryker, 1968, 1980). Both neoclassical and behavioural economists consider the role identity and social roles play in behaviour. Neoclassical economists argue that people derive utility from being members of a social category and

behaving in ways expected of members of such categories (Akerlof and Kranton, 2000, 2002). They build a preference for adhering to one's social role into people's utility functions. These preferences behave very much like other types of preferences in standard models. In contrast, behavioural economists view identities as schemata or mental models, that suggest which behaviours are possible and desirable in particular situations for individuals who play certain social roles (Hoff and Pandey, 2014; LeBoeuf, Shafir, and Bayuk, 2010). Preferences for adhering to social roles are evoked by particular cues, so they are context-dependent and unstable. These alternative theoretical perspectives epitomise the debate between the two paths for incorporating psychology into economics which I outlined above.

In standard economic models, concepts of aspirations, self-efficacy and locus of control, and identity do not play a role in decisions. People have perfect information, absorb and process all relevant information, assess their situations objectively, and form correct beliefs about the probability of various outcomes. They form preferences over the outcomes and make choices accordingly. In relation to the specific psychological concepts just mentioned, these models assume that people's beliefs about their own capabilities are accurate assessments. Their aspirations reflect their preferences among outcomes, but the limits they place on what outcomes they can attain are correct assessments of objective constraints and their own ability to act. Their identity within a social structure does not influence decisions: membership of a social category and compliance with the rules of behaviour expected of members of such categories has no tangible benefits and does not affect preferences between outcomes.

In four empirical chapters, I present work demonstrating that concepts of aspirations, self-efficacy and locus of control, and identity – people's construals of their capacities, their future, and their social roles – play an important role in explaining household investment decisions in children's schooling in Ethiopia. Below, organised under two research questions, I outline four new empirical results that contribute to the limited but growing applied literature in economics in this area. Contrary to the predictions of standard economic models, these concepts play an important role in economic decisions, in particular investment in children's education in a developing country context. In some instances, they may explain the puzzling patterns in household investments in education that I outlined above.

In the conclusions to each chapter, I also consider whether the fact that aspirations, self-efficacy and locus of control, and identities have empirical relevance and the characteristics they display in turn implies that standard models of decision-making in economics need to be

modified or even rejected. There appear to be two paths available. Some existing applications of psychology in economics, such as work on non-cognitive skills, leave the assumptions of standard models broadly intact. Other work, such as in behavioural economics, makes substantive adjustments to these assumptions. I consider the extent to which either of these paths is more appropriate for the concepts I use.

A. Do parents' self-beliefs and aspirations matter for decisions about investment in children's education?

My first research question is whether parents' self-beliefs – their locus of control and self-esteem – and their aspirations for their children's educational attainment affect their investments in their children's education. Chapters 4 and 5, the first two empirical chapters, present evidence from a randomised controlled trial which sought to alter parents' self-beliefs and aspirations in rural Ethiopia.

Individuals were randomly invited to watch documentaries about people from similar communities who had succeeded in agriculture or business and improved their socio-economic position from being poor to being relatively successful. The documentaries did not provide specific information on returns to particular behaviours or specific advice on behavioural change. There were no other changes in opportunities or constraints which only affected the treatment group in the six month period of the experiment. A placebo group watched an Ethiopian entertainment programme and a control group were simply surveyed. In addition, the number of people invited was varied by village to test for the presence of spillover effects which may result in bias in estimates of the treatment effect.

In a standard economic model, exposure to motivational inputs encouraging people to work hard, set goals, and be determined, but without providing much concrete information, would be unlikely to change beliefs, preferences or behaviour. People are either assumed to have perfect information and therefore accurate self-beliefs, or are assumed only to update beliefs in response to new information.

However, social learning theory in psychology suggests that self-beliefs about one's own competence and efficacy are powerful direct influences on effort and choices and that they shape people's aspirations, which also influence effort and choices. Furthermore, self-beliefs are subjective construals of one's capabilities and can be altered by resonant or emotional experiences or by receiving encouragement or motivation, even if people do not receive any

new information about their abilities. Social learning theory would argue that the video might affect self-beliefs, aspirations and behaviour.

In Chapter 4, I show that, six months after screening, the treated group had a more internal locus of control – they were more likely to believe they could alter their circumstances. They also had higher scores on indices of aspirations and expectations, driven largely by changes in parents' aspirations and expectations for the level of education they wanted their children to achieve. Aspirations and expectations for other aspects of life, such as parental social status, level of assets or level of income, had not changed.

In Chapter 5, I find also that treatment has effects on savings, use of credit, children's school enrolment and spending on children's schooling, compared to the control group and, in some cases, the placebo group. Results are not confounded by spillovers. These are estimated using two strategies, village-level variation in treatment intensity and variation in the intensity with which an individual's network is treated. Across the two chapters, results on self-beliefs, aspirations and expectations and educational investment are robust to corrections for multiple testing. Watching a one-hour documentary causes changes in self-beliefs and aspirations and also translates into changes in a range of forward-looking behaviours six months later. This suggests a challenging, promising avenue for further research and poverty-related interventions.

I also draw some conclusions from this experiment for the more theoretical research question I pose about whether the characterisation of beliefs and preferences in the standard model ought to be modified, and if so, how. Firstly, the literature on non-cognitive skills commonly treats locus of control and self-esteem as non-cognitive skills (Heckman, Stixrud, and Urzua, 2006; Heckman and Kautz, 2012, 2013; Borghans et al., 2011; Almlund et al., 2011). But the literature in social psychology conceives of self-beliefs – such as locus of control and self-esteem – as beliefs, rather than skills. And this empirical study also suggests that these beliefs about oneself behave much more like beliefs than skills: they can be altered without any direct practice or experience, simply through exposure to other people's experiences. Describing these concepts as beliefs, rather than skills, allows that self-beliefs are construals or interpretations of an individual's capabilities and may often be inaccurate.

Secondly, I suggest that self-beliefs are less stable over time than economists studying non-cognitive skills assume skills to be, and that they do evolve over time. However, they are relatively stable across different contexts at a point in time and are not as context-dependent as

beliefs are held to be in behavioural economics. In this data, people's underlying self-concept is correlated with beliefs about others' opportunities, risk preferences, and beliefs about the factors causing poverty. The intervention is linked to changes in this wide range of beliefs and preferences, and to changes in behaviour in a number of domains. This suggests that one's underlying self-beliefs remain stable across many domains of behaviour and changes in these beliefs may result in changes in many aspects of behaviour.

Finally, I argue that aspirations are the outcomes that people prefer among a set of outcomes they believe to be attainable, based on their beliefs about their own capabilities and about objective constraints and available resources. They thus capture elements of both beliefs and preferences. I argue that it is not necessary to take a behavioural economics perspective on aspirations, which conceives of aspirations as reference points and views people as satisficers, to view aspirations as important influences on behaviour. Viewing aspirations as expressions of self-beliefs, rather than viewing aspirations as reference points and self-beliefs as skills, accords with other findings in psychology (Levenson, 1974; Strickland, 1965; Bandura and Locke, 2003) and accommodates my findings that these two concepts are closely related and are both affected by the intervention. This relationship is not currently captured by either the non-cognitive skills literature or the behavioural economics literature.

B. Do children's preferences, particularly for adhering to social roles, matter for decisions about investment in children's education?

My second research question is whether children's conceptions of their roles and identities in the household and at school, and the value they attach to fulfilling these roles, affect investments. In Chapter 6, the third of the empirical chapters, I present evidence from qualitative data collected from parents and their children in an Ethiopian village in 2008 and 2010. I explore children's identities as students, financially independent workers, young adults preparing to have their own homes and farms, and obedient, caring sons and daughters. I examine their perceptions of appropriate behaviour for members of each of those categories and consider how these perceptions influence their choices, in particular about their time allocation to work and school and how they decide to spend money they earn from work.

I find that children's identity as students is a source of pride and status and the desire to behave in accordance with this identity is important in their daily choices of activity. This may explain one of the puzzles posed in Chapter 3: why students may stay in school despite

poor quality education and low returns. Children's desire to behave in accordance with identities as good, obedient children, and as independent young adults who contribute to their households, potentially explains why they continue to work in their households despite its negative effects on their schooling, and why they contribute some of their wages from paid work to their families as well as paying for their school expenses.

I also interrogate which of two alternative frameworks from economics on identities best explains patterns in the qualitative evidence. In one widely used perspective, which is closer in spirit to a neo-classical than a behavioural approach, Akerlof and Kranton (2000) argue that people derive utility from being members of a social category and behaving in ways expected of members of such categories. They build a preference for adhering to one's social role into people's utility functions, which means standard economic model of decision-making can accommodate individuals' preferences for adhering to an identity. In subsequent work they examine how identity explains children's decisions about investing time and effort in education (Akerlof and Kranton, 2002).

An alternative, "frame-dependent" theory of identity (Hoff and Pandey, 2014) draws on behavioural economics and cognitive psychology, which argues people make many decisions almost automatically using shortcuts or schemata to filter, categorise and interpret information (Tversky and Kahneman, 1974, 1124). Identities can be understood as schemata about which behaviours are possible and desirable in particular situations for individuals who play certain social roles. They are evoked by particular contexts or cues and are more or less salient in different contexts, or in the same context at different points in time (Hoff and Pandey, 2014).

I argue in favour of making two modifications to the Akerlof and Kranton (2000) model, rather than using the frame-dependent model of identity. One modification is to account for people holding multiple identities simultaneously, which the authors themselves suggest might be a possible extension to their model. Nevertheless, people have a consistent sense of self underlying their different identities which enables them to set goals and priorities and act to achieve them. They internalise their various social roles in one underlying self-image, which they carry from situation to situation (Owens, Robinson, and Smith-Lovin, 2010, 485)

The modification allows that individuals have a preference ranking over their different identities. When identities conflict, individuals form a preference between them. Their preference could even differ depending on the context. So my modification does not imply altering standard assumptions about preferences, unlike frame-dependent perspectives from

behavioural economics. Standard models can accommodate that preferences for complying with an identity are context-specific, because alternatives can be specified as bundles which capture everything affecting an agent's decision (Arrow, 1970).

Frame-dependent theories already acknowledge that individuals hold multiple identities simultaneously. However, I argue that they take other positions which do not match particularly well with children's discussion of their thought processes as revealed by qualitative work. Frame-dependent models primarily suggest decisions about whether to behave in accordance with an identity are made automatically. The Akerlof and Kranton (2000) model captures my qualitative findings that such decisions are mainly made deliberately and consciously. Even the cultural sociologists whom frame-dependent behavioural economists cite clarify that decisions about identity are often made consciously (DiMaggio, 1997, 271).

I also propose a second modification to the Akerlof and Kranton (2000) model. It currently does not allow that preferences for complying with social roles may be altruistic. The model assumes people's own utility is affected by the discomfort of others. People comply with social roles in part because this avoids causing discomfort in other members of their category. However, they are ultimately still acting to maximise their own utility. This may not capture that compliance with prescriptions is often entirely altruistic and based on considerations of morality and obligation.

In the last empirical chapter, Chapter 7, I show that understanding children's preferences might improve predictions about how they will react to education policies. The existing literature on household responses to education policies largely examines responses to policies which increase resource allocations to schools, but often does not capture that households can reallocate resources such that there is no net welfare gain from such a policy (Todd and Wolpin, 2003; Pop-Eleches and Urquiola, 2011; Glewwe, Kremer, and Moulin, 2009; Glewwe and Kremer, 2006). I apply this idea, but within the context of children's allocation of effort rather than household allocation of resources.

The literature on time in school suggests that children desire only a certain amount of learning and calibrate effort per hour depending on the number of hours they spend in school (Levin and Tsang, 1987). Accordingly, they will decrease effort in response to an increase in the school day, resulting in only small effects on achievement, a result found in much of the empirical literature in the developed world (Sims, 2008; Marcotte, 2007; Lavy, 2012; Pischke, 2007). Partly as a result of this evidence, international organisations like the World Bank and

UNESCO encourage many developing country governments to adopt four-hour school days and teach two groups of children each day (Bray, 2008).

Contrary to this prediction, I find a reform to lengthen the Ethiopian primary school day improved test scores. In 2006, the Ethiopian federal government directed school districts to abolish teaching in shifts and lengthen the school day from four to five-and-a-quarter-hours. I use a difference-in-differences specification controlling for time-invariant unobservables at school level on a unique longitudinal dataset. For eight-year-old children, a longer school day improved writing and mathematics scores, but had no significant effect on reading. However, effects are larger among children who are better-off: children who are not stunted, children from wealthier households and children in urban schools. The reform thus improves achievement on average, but may exacerbate gaps between wealthier and poorer children.

There are a number of possible explanations for this result. There may simply be constant (or increasing) marginal returns to time in school, although other evidence suggests this is not the case (Lavy, 2010). Teachers in Ethiopia may be highly motivated and have increasing marginal returns to teaching, so they manage to generate more achievement from students even if students do not like the extra hours. Or, alternatively, children may be highly motivated to learn and do not have a diminishing marginal return to time in school, at least up to a certain length of day. In relation to the broader arguments in this thesis, the chapter suggests that understanding children's preferences and likely responses to policies will improve the likelihood of successful predictions of the effects of policies.

C. Should assumptions in standard economic models be modified or rejected?

The primary concern of the first two research questions is empirical. My final research question is more conceptual. I ask whether the fact that aspirations, self-efficacy and locus of control, and identities have empirical relevance in turn requires that standard models of decision-making in economics need to be modified or even rejected. There could be two dimensions to this question. One is whether psychological concepts can be captured by existing economic *concepts* of beliefs, preferences, skills or constraints. The other is whether the characteristics of psychological concepts in empirical research suggest that the *assumptions* made in standard economic models about beliefs and preferences need to be modified. In Chapter 3, the second part of the literature review, I differentiate between two paths taken by economists when using psychological concepts in economics. I argue that both paths continue

to use standard economic *concepts*, although they are often slightly broadened (Hausman, 2012; Bruni and Sugden, 2007; Ross, 2005). I thus focus on the second dimension of the question, whether *assumptions* need to be modified.

One path, taken by economists working on non-cognitive skills and some economists working on identity and social norms, does not really require modifications to the assumptions made in the standard model about the characteristics of beliefs, preferences and skills. Preferences still behave according to the axioms of ordinal utility theory. Beliefs are still correct and objective reflections of the information available which are relatively stable and are updated only if new information becomes available. Skills remain largely stable across contexts, even if they may develop over time. All other standard assumptions about beliefs and preferences remain the same.

Behavioural economists take a second path. They argue that the assumptions the standard model makes about beliefs, preferences and processes of deliberation are often questionable approximations of actual beliefs, preferences and processes. They aim to describe agents' processes of deliberation and decision-making and the formation of beliefs and preferences using empirically grounded generalisations derived experimentally and from field research. In Chapter 3, I outline a number of modifications they suggest, as outlined above: people often make decisions automatically; they often do not optimise, but satisfice; their preferences and beliefs differ depending on how information is framed; and their preferences are often altruistic, rather than self-interested.

In the Conclusion, I pick a route between these two paths. I argue that concepts used in this thesis – self-beliefs, aspirations and identity – can also be captured using concepts of belief and preference. My empirical results imply that the ways in which these concepts are currently integrated into economic models requires some modification. However, because of the particular characteristics of self-beliefs, aspirations and identity, in the main my empirical results suggest they can be included in standard models without requiring many adjustments of standard assumptions about beliefs and preferences.

I argue that, contrary to recent accounts building on human capital theory, locus of control and self-esteem should be conceived of as a type of belief, self-beliefs, rather than as non-cognitive skills. Such beliefs may not be objective or correct assessments of people's capabilities, even where there is full information. However, I argue that standard economic concepts of beliefs, especially over long run outcomes where there is uncertainty, allow for

such beliefs. I argue that self-beliefs are relatively context-independent and remain stable across contexts, although they may evolve gradually over time.

I consider aspirations as a type of preference – the set of future outcomes that people prefer among a set of outcomes they believe to be attainable – but highlight that such preferences are shaped by people's beliefs, both about their own capabilities and about objective constraints and available resources. They are shaped by and thus strongly correlated with self-beliefs. I argue against behavioural economics models of aspirations as reference points which are based on and mechanically adjusted based on past outcomes (Genicot and Ray, 2014; Dalton, Ghosal, and Mani, 2015).

I consider identity as a preference for complying with a social role, in line with Akerlof and Kranton (2000). However, such preferences are often altruistic, ratter than self-interested. They may also capture which of multiple identities an individual prefers to comply with. This preference may differ by context and over time without violating assumptions about preferences in standard models. I accordingly also argue against "frame-dependent" models of identity in behavioural economics.

Secondly, I maintain that continuing to use standard models with some modifications has substantial benefits. Thirdly, however, this thesis also highlights the value of a more fine-grained, empirically grounded understanding of differences between various types of beliefs and preferences and how they are formed. My arguments for whether these particular psychological concepts are captured by concepts of belief and preference and whether they behave according to standard assumptions are empirically based, rather than assumed. They apply specifically to these concepts, but may not apply for other types of beliefs and preferences. Investigations of the characteristics of particular types of beliefs and preferences can illuminate when particular general assumptions about beliefs and preferences are more or likely to apply and thus improve economists' explanations and predictions of behaviour.

III. Data collection and prior publication

Chapters 4 and 5 report on a field experiment conducted with Dr Tanguy Bernard, Professor Stefan Dercon (one of my two supervisors) and Dr Alemayehu Seyoum Taffesse. I worked as the research assistant on the project and was then invited to become one of the authors. I selected, adapted and piloted the psychological measures of self-beliefs and measures of risk and time preference. I also designed and piloted the rest of the questionnaire. I was the only

member of the team present in the field from June to September 2010 and in April 2011. I designed the sampling protocol and conducted the randomisation. I was also responsible all the field logistics, under Dr Seyoum Taffesse's supervision, and organised and supervised the survey managers (Bezabih Tesfaye and Tewodros Abate) who ran fieldwork (both for the survey and the screening of movies). I also trained enumerators.

The other three co-authors raised funding for the survey. Drs Bernard and Seyoum Taffesse produced the documentaries used in the research and designed and piloted the measures of aspirations. Dr Bernard conducted the power calculations and decided on the treatment and placebo design. Dr Bernard and I collaborated on the design of the tests for spillovers. I cleaned the data, and conducted and wrote up the analysis, with detailed supervision from Professor Dercon and Dr Bernard.

Results have been published as a working paper (Bernard et al., 2014) and in conference proceedings from the Annual Bank Conference on Development Economics in 2014 (Bernard et al., 2015). I have substantially revised the working paper and publication for this submission. This includes revising the introductory sections of the working paper to apply a different theoretical framework based on social learning theory. I have also undertaken new analysis on psychological measures of self-beliefs and beliefs about others' opportunities. The work presented here may not accord with the perspectives of the rest of the team.

In Chapter 6, I discuss data collected from parents and their children in an Ethiopian village in 2008 and 2010. I collected the 2008 data for my MPhil thesis with fieldworkers. In 2010, the same children were included in a qualitative component attached to the Young Lives School survey. I designed the questionnaires, trained fieldworkers and was present during interviews. Research based on the 2008 data has subsequently been published (Orkin, 2010, 2011, 2012). This chapter adds the 2010 data and uses a new theoretical framework.

Chapter 7 is a revised version of a working paper (Orkin, 2013). I use longitudinal data from the Young Lives survey and data from a survey of schools attended by Young Lives children. I spent the 2009/10 academic year working for Young Lives on the design and implementation of the survey. Under the supervision of Professors Jo Boyden, Professor Martin Woodhead, and Dr Laura Camfield (the other of my two supervisors), I wrote proposals which won a grant for design and piloting of £150,000 from the Department for International Development (DfID) in the UK, and a grant of £500,000 from DfID in Ethiopia. I also worked

on a proposal which won £3.2 million for surveys in three countries from the Netherlands Ministry of Foreign Affairs.

Dr Camfield and I compiled initial drafts of the survey instruments, with comments from Professors Martin Woodhead and Angela Little, piloted the questionnaires for the survey, trained field researchers, and supported Workneh Abebe and Bezabih Tesfaye in running the survey. I had sole responsibility for the design of the questions used in this research. With Young Lives country staff, I also set up a task force of Education Ministry officials and donors, including the Deputy Minister of Education, to consult on the survey design and receive information on findings. Young Lives showcased this as a case study of research impact (Young Lives, 2011).

2

Do human capital models predict household education investments?

This chapter summarises some key predictions of the strand of microeconomic human capital theory that deals with the amount of time children spend in school, the effort children put into learning, and how much parents invest in children's education. Many of the predictions of these models hold in the developed world (Heckman, 2014): easing budget and credit constraints increases time in school, there are private monetary returns to education, and households respond to changes in their expectations about these returns.[1]

Some surprising empirical patterns arise in developing countries. Households often behave in ways not predicted by the standard model, suggesting that it may require extensions. In particular, education investment decisions are very responsive to small changes in cost. Households continue to invest in education despite potentially low returns. Exposure to information or messaging about education returns can change investment quite dramatically. Children are very influenced by the choices of their peers. A common theme is that beliefs about the value of investments in education play an important role, given the long time horizon over which returns are realised, and may not behave as the standard model suggests. Although research which exposes parents or children to information about the value of education is often interpreted as addressing an information asymmetry, the interventions provided may be interpreted using other theoretical frameworks

[1]There is, of course, an extensive literature in education on these issues, often with conflicting predictions to the literature in economics, but these differences are not discussed in this thesis, which draws on and speaks to the literature in economics.

I begin with some very brief context on recent Ethiopian history and the Ethiopian education system. Throughout the chapter, I show that many of the surprising patterns of behaviour found in other developing countries hold in Ethiopia.

I. The Ethiopian context

Ethiopia ranked 171 out of 181 countries on the UNDP's Human Development Index (HDI) (United Nations Development Programme, 2010). GDP per capita in 2013 was $505, and $1380 when adjusted for purchasing power parity (World Bank, 2013).[2] It is the second most populous country in Africa, with a population of 78 million in the 2007 census (Central Statistical Agency of Ethiopia, 2007). The majority, 84 per cent, of the population lives in rural areas (Ministry of Education, 2011b, 10). Eighty-three per cent of the population live in the highland temperate part of the country, where most are sedentary, subsistence farmers. The remainder are mostly pastoralists, living in the arid lowlands which make up over half the country's land mass. Challenges to provision of primary education include the dispersed rural population, ethnic diversity (with over 80 ethnic groups), frequent natural disasters, and political instability (Yamada, 2007).

A. Brief history

The original Abyssinia is current-day Amhara and Tigray, the north and northwest of Ethiopia. Regions to the south, east and west of Abyssinia, home to non-Semitic speakers who practise Islam or traditional religion, were colonised by Abyssinia in the late nineteenth century (Zewde, 2001). Emperor Haile Selassie ruled from 1930 to 1974, except during the 1936–1941 Italian occupation.

There was widespread unrest in the 1970s (Zewde, 2001). The military took advantage of popular unrest, fuelled by drought, famine, inflation and dissatisfaction with inequality. Soldiers deposed the Emperor in 1974 and established a highly centralised Marxist regime known as the *Derg* (meaning "committee" in Amharic) (Clapham, 1990, 151). Like the imperial government (Donham, 2002), the *Derg* regime was dominated by Amharic-speaking Orthodox Christians (Vaughan and Tronvoll, 2003, 46). The *Derg* greatly expanded the government and asserted aggressive central control over the regions (Clapham, 2002).

[2]GDP per capita based on purchasing power parity (PPP) is product converted to international dollars using purchasing power parity rates. An international dollar has the same purchasing power over GDP as the US dollar has in the United States.

Rebel groups fought a civil war in the north and southeast from the late 1970s. There were also severe droughts during the 1980s and a famine from 1984 to 1986. In 1987, a new constitution was approved by referendum, but *Derg* leaders remained in power. As the Cold War ended, the Soviet government withdrew its support for the *Derg*. In 1991 it was overthrown by a rebel coalition, the Ethiopian People's Revolutionary Democratic Front (EPRDF). The EPRDF won the first elections in 1994 (Zewde, 2001). Under the *Derg*, thousands had been killed, millions displaced, infrastructure destroyed, and gains in public service provision largely been reversed.

B. Primary education

Under the Emperor, before 1941, the Church opposed the establishment of a secular education system (Teshome, 2008). Although the first government primary school was opened in 1908 in Addis Ababa, there were few state schools and children could only access Christian Orthodox Church schools (Telila, 2010). After 1941, more state schools were built to train students for the expanding civil service. Schools were relatively well resourced (Clapham, 1990, 149), but educational access was limited to students from an elite background in urban areas (Teshome, 2008, 13). In 1961, the net enrolment rate (NER) was only 3.8 per cent (Kebede, 2006, 11).[3] The system was criticised for being bureaucratic, wasteful, overly academic, and irrelevant to the lives of most Ethiopians (Teshome, 2008).

The *Derg* implemented a huge literacy campaign, in which high school and university students were sent to rural areas (Degueffe, 2003). Expanding access to education was one of the *Derg* regime's most important modernisation projects (Clapham, 1990, 151), and served the dual purposes of disseminating Marxist ideology and improving industrial and agricultural productivity (Negash, 2006). Formal education was expanded: the number of schools increased from 3,196 in 1974/75 to 7,900 in 1985/86 and the net enrolment rate (NER) grew from 10 to 35 per cent between 1970 and 1989 (Negash, 2006, 19). But there were shortages of trained teachers and textbooks and pupil-teacher ratios were high (Clapham, 1990, 151). Famine and war during the 1980s partly reversed the improvement in access to

[3]The NER measures on-time school participation, i.e. the proportion of children of primary school age (7–14) who are actually enrolled (UNESCO, 2008). It is usually lower than the Gross Enrolment Rate (GER) because it excludes over- and under-aged students. In Ethiopia the NER is calculated by dividing the number of properly aged primary students (for Ethiopia, ages 7–14) by the number of children of school age (7-14) (Ministry of Education, 2013, 27).

primary education that had been achieved (Negash, 1996). In 1991, the NER in Ethiopia was only 20 per cent (Asgedom et al., 2006, 8).

The 1994 Education and Training Plan (ETP) outlined radical changes to education: a focus on girls' education, an ambitious school-building programme, the replacement of Amharic with local languages as media of instruction, the abolition of school fees, and a requirement that children be automatically promoted through the first three grades (Ministry of Education, 1994). Primary education was extended from six to eight grades to "increase the schooling received by the majority who do not go beyond the primary level" (Ministry of Education, 1998, 5), and this has been maintained despite donors arguing for a shorter primary cycle (World Bank, 2005). Schooling is divided into a first cycle (Grades 1–4), which teaches functional literacy, and a second cycle (Grades 5–8), which prepares students for general secondary (Grades 9 –10) and preparatory education (Grades 11–12).

The *Derg* placed all non-governmental schools, at that time 46 per cent of primary schools, under state administration in 1975 (UNICEF/World Bank, 2009, 49). Most schooling is still provided through the state sector. In 2006/07 a World Bank survey found that only 5.3 per cent of primary school children were in non-government schools, many of which are NGO-administered faith-based schools (World Bank, 2008, 145). Government data on this sector is limited.

Since 1997 the federal government has produced an Education Sector Development Programme (ESDP) every four years, which is integrated into the Poverty Reduction Strategy. In the first three ESDPs, government emphasised improving access to education. The Millennium Development Goals were tightly integrated into Ethiopia's education plans, with a focus on the goals of universal primary enrolment and gender equity (Shibeshi and Arsano, 2007). The Ethiopian government argues that universal primary education is necessary to train "educated farmers" who can adopt new technologies (Ministry of Education, 2005, 18-19), and emphasises increases in agricultural productivity as a primary source of growth (Ministry of Finance and Economic Development, 2010). Government also argues that improvement in skill levels is a precondition for industrial development and will provide skilled workers to manage infrastructure and staff the civil service (Ministry of Education, 2005, 2011a). Government also highlights the role of education in developing "good citizens" and the civics curriculum contains a substantial component of ruling party ideology (Poluha, 2004).

C. Enrolment and progression

Improvements in access to education in Ethiopia have been among the most dramatic in the world (UNESCO, 2008). In 1995/96, 3.1 million children were enrolled in primary school, an NER of 30.0 per cent (Ministry of Education, 1998, 5). By 2010/11, according to official Ministry of Education statistics, 16.7 million children were enrolled, an NER of 85.3 per cent (Ministry of Education, 2011b, 11).[4] The NER remained roughly constant at 85.4 per cent in 2011/12 and 85.9 per cent in 2012/13 (Ministry of Education, 2013, 27). In line with the focus on ensuring access, government implemented a massive school-building campaign from 1999 to 2010, and the number of primary schools soared from 10,964 to 28,349 (Ministry of Education, 2000, 2011b). Roughly 80 per cent of new schools were built in rural areas (Engel, 2010, 7).

The proportion of individuals with no schooling, while one of the largest in sub-Saharan Africa, is declining. In the 2005 DHS, 67 per cent of males and 52 per cent of females in 2005 had had no access to schooling, (Central Statistical Agency, 2006, 26). By the 2011 Demographic and Health Survey (DHS), 38 per cent of males and 53 per cent of females aged six and above had never attended school (Central Statistical Agency and ICF International, 2012, 27). A cohort comparison better demonstrates the magnitude of the improvement: in 2010, 98 per cent of women and 89 per cent of men aged 65 and over have no education, whereas only 17 per cent of girls and 18 per cent of boys aged 10-14 have no education (Central Statistical Agency and ICF International, 2012, 27).

Completion rates are also improving, but remain of concern. Although 75.6 per cent of children in the relevant age group in the population completed Grade 5 in 2012/13, only 47.8 per cent completed Grade 8, a full cycle of primary school (Ministry of Education, 2013, 6-7). Thus over half of children do not finish primary school, although there was major improvement in this indicator (only 18.7 per cent finished primary school in 2001/02 and 41.7 per cent in 2005/06 (Ministry of Education, 2006, 35)).

[4]The 2011 Demographic and Health Survey (DHS) finds a much lower net attendance ratio (NAR) for primary school of 64.5 per cent (Central Statistical Agency and ICF International, 2012, 26). The DHS uses a random sample of households in a random sample of census enumeration areas and estimates are appropriately weighted. The NAR has the same definition as the NER: the proportion of children aged 7 to 14 who are enrolled. However, it is calculated differently: the DHS examines the children aged 7 to 14 whose households are in the sample and how many of them are enrolled. It is potentially more accurate than the Ministry data but is only available for 2000, 2005 and 2011. The Ministry rate uses the Education Management Information System (EMIS) data collected from schools to calculate the number of enrolled children and the Population and Housing Census of 2007 to calculate the school-aged population. It appears the population figure used in the Ministry data, 18,287,214 children aged 7 to 14, has not increased since 2007. The Census reports 22,350,613 children aged 5 to 14 (Central Statistical Agency, 2007, 28). However, I have not explored whether this is a genuine error in Ministry calculations.

The dropout rate has recently worsened. At national level in 2011/12, 22.7 per cent of pupils enrolled in Grade 1 left school before reaching Grade 2 the next year and 6.1 per cent of children in Grades 1 to 8 dropped out (Ministry of Education, 2013, 34). In 2010/11 19.9 per cent dropped out during Grade 1 and 13.1 per cent in Grades 1 to 8 (Ministry of Education, 2011b, 35), and it is higher than in neighbouring countries (Engel, 2010, 7). As a result, the NER is 95.5 per cent in Grades 1 to 4 but only 47.3 per cent in Grades 5 to 8 (Ministry of Education, 2013, 6-7).

Ethiopia is a more extreme case of trends across sub-Saharan Africa. Enrolment in primary school has grown dramatically: by 2011 the NERs for primary and lower/secondary schools were 89 per cent and 82 per cent respectively (UIS-UNESCO, 2014). Between 1999 and 2009, the primary GER grew 3.1 percentage points per year, compared with only 0.8 percentage points per year in the 1990s, in a sample of 33 low-income sub-Saharan African countries (Majgaard and Mingat, 2012, 2).[5] However, rates of completion of primary school are very low. By 2009, average primary completion was only 63 per cent in the same sample, although the average primary completion rate improved 20 percentage points between 1999 and 2009, an average of 2 percentage points per year (Majgaard and Mingat, 2012, 3).

D. School quality

The Ethiopian government recognised that there would be trade-offs between quality and access. Successive National Learning Assessments (NLA) in 2000, 2004 and 2007 found that student scores declined (Ministry of Education/USAID, 2008, 9). Declines in achievement could be viewed as "a predictable consequence of the shift to a more universal system, in which students facing more difficult living conditions are now involved" (Dom, 2010, v).

In part, this may be due to an overall decline in per-capita resources. In the first Education Sector Development Plan (ESDP-I) in 1997, government stated that average class size would be allowed to rise from 33 to 50 to increase the amount of non-salary expenditure available for textbooks and equipment, and increased use might be made of the shift system to use teachers more efficiently (Ministry of Education, 1998, 6). As shown in Table 2.1, between 1994/95 and 2003/04 the proportion of qualified second-cycle teachers dropped and the pupil-teacher ratio and dropout rate worsened (World Bank, 2005, 153).

[5]The sample includes all sub-Saharan African countries eligible for lending from the International Development Association, except those with very small populations (Cape Verde, the Comoros, and Sao Tome) and those with highly incomplete data (Liberia, Angola and Somalia).

Table 2.1: Indicators of school quality in Ethiopia, 1995/96 – 2010/11

Indicator	1995/96	1999/2000	2003/04	2007/08	2010/11
Repetition rate, Grade 1 - 8 (%)	12.6	8.2	4.0	7.3	8.5
Dropout rate, Grade 1 - 8 (%)	8.5	18.9	19.2	15.7	13.1
Dropout rate, in Grade 1 (%)	28.5	30.3	31.4	18.3	19.9
Gross intake to Grade 8 (%)	–	18.7[1]	38.8	44.7	49.4
Pupil-teacher ratio, Grade 1 - 4	37.0[2]	56.0[2]	71.0	62.0	57.0
Pupil-teacher ratio, Grade 5 - 8	–	–	55.0	50.0	45.0
Pupil-textbook ratio, Grade 1 - 8	–	–	–	–	1.5
% qualified teachers, Grade 1 - 4	85.0	89.6	96.5	97.3	20.1[3]
% qualified teachers, Grade 5 - 8	20.9	18.4	32.4	66.3	83.3
NLA score, Grade 4[4]	–	48.1	48.5	40.9	–
NLA score, Grade 8[4]	–	41.1	39.7	35.6	–

Source: Ministry of Education (1999/2000; 2003/04; 2007/08; 2010/11); Ministry of Education/USAID (2008).
[1] 2001/02 statistic.
[2] Average for Grade 1 to 8.
[3] In 2010 the teacher qualification requirements were altered to require a two-year diploma to teach first cycle, not a one-year certificate.
[4] NLA scores are for 1999/2000, 2003/04 and 2007/08.

Nonetheless, in the 2005 election the opposition criticised the poor quality of education (Dom, 2010, 36), perhaps because of the general populace's frustration at heavy-handed enrolment drives and community fundraising efforts conducted by local officials (Engel, 2010, 8). The National Learning Assessment (NLA) focus groups with parents, teachers and students in 2004 found widespread agreement that students did not acquire the knowledge and skills they expected (Ministry of Education/USAID, 2008). Government has since made education quality a central policy theme and many key indicators have improved.

In late 2010, the donor-supported General Education Quality Improvement Programme (GEQIP) began. The programme focuses on four quality drivers: providing school grants to districts to be given to schools for school improvement, upgrading teacher qualifications, centralising development of the curriculum and printing and distribution of textbooks, and improving management capacity in schools and districts (Ministry of Education, 2008b).

E. Household investments in education

Until the 1950s the state bore all costs of education, but after the 1960s schools introduced registration, facility and maintenance fees, book rental costs and construction or extension levies (UNICEF/World Bank, 2009). The fees were levied by schools and varied from school to school. There was virtually no non-salary funding allocated to schools. A 1996 survey found government school fees per student were between 10 and 15 ETB (1.6 and 2.37 USD in

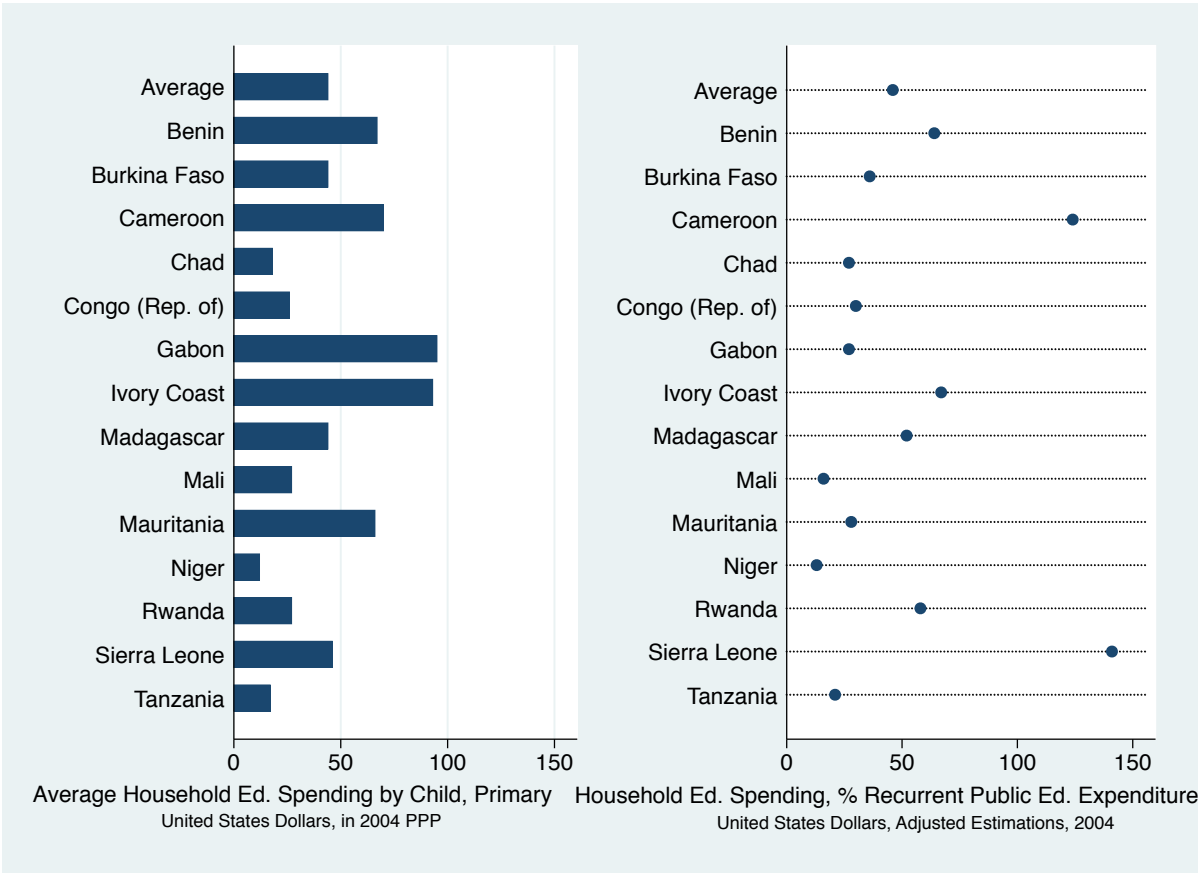
1995 prices) (Policy of Human Resource Development, 1996). Fees were abolished in 1995 and almost all schools had implemented the policy by 1996 (UNICEF/World Bank, 2009).

Fees were replaced by block grants provided by districts to schools for non-salary spending. Initially, the grant per student was set at the same rate as fees: 10 ETB for Grades 1 to 4 and 15 ETB for Grades 5 to 8 (UNICEF/World Bank, 2009, 69). However, total government non-salary expenditure fell from 17 per cent of the recurrent budget in 2001/02 to only 8 per cent in 2007/08 and the value of the grant per student declined (Dom, 2010, 19, 32). In addition, from 1994/95 to 1995/96, there was a 23 per cent increase in enrolment and a 29 per cent growth in Grade 1 intakes, and a further 13 per cent increase in the following year (UNICEF/World Bank, 2009). Fee abolition is believed to have been one factor leading to rapid growth in enrolment (Dom, 2010; UNICEF/World Bank, 2009). While this was largely the intent of the policy, it led to smaller budgets per student available to schools.

As is common in the rest of Africa when fees are abolished, school fees have reappeared in new forms. This particularly occurs if schools were not compensated fully for the substantial decline in revenue when school fees were abolished (UNICEF/World Bank, 2009). A 2006 survey found that many schools levied fees for registration, stationery, certificates, ID cards, sanitation and construction contributions, on average 25.5 ETB in primary schools (2.8 USD in 2006 prices) (UNICEF/World Bank, 2009, 54). Schools also ask for voluntary contributions from the community at large. In 2004/05, community contributions alone accounted for 4.9 per cent of recurrent expenditure in Amhara region, 5.7 per cent in SNNP, 5.1 per cent in Oromia and 9.2 per cent in Tigray (UNICEF/World Bank, 2009, 65). In this year, fees and community contributions frequently constituted between 10 and 20 per cent of the school district's budget (Garcia and Rajkumar, 2008). GEQIP provided school improvement grants direct to schools from 2010, but parents and communities remain responsible for raising a substantial portion of funds for school infrastructure (Dom, 2010, 19, 32): in Oromia in 2007/08, community contributions covered 40 per cent of classroom construction costs for primary schools (DFID, 2010, 4).

Families are often required to pay fees for textbooks, stationery and sometimes uniforms (although uniforms are not required in many rural schools). In 1999/2000, household spending on education in Ethiopia equalled 28.9 per cent of government recurrent spending on education across all levels of education and 25.2 per cent at primary level (World Bank, 2005, 100). In 15 African countries where representative household surveys were conducted between

Figure 2.1: Household spending on primary education in 15 African countries



Source: Foko, Tiyab, and Husson (2012). Average household education spending per child in primary school is from authors' calculations based on national survey data and IMF data. On average, households report spending $44 per year per child in primary school. This is equal to 46% of recurrent public education expenditure (from calculations based on household survey data, IMF data, and Country Status Reports). In both panels of the graph, data from different survey years are adjusted to 2004 US dollars.

2004 and 2010, Figure 2.1 shows that household spending on education on average equalled 46 per cent of government recurrent spending on education (Foko, Tiyab, and Husson, 2012). Households reported spending $44 per year per child in primary school on such schooling costs (at 2004 purchasing power parity), an average of 4.6 per cent of the household's annual spending. In an earlier study with a slightly different set of countries between 1999 and 2006, household spending on education was 42 per cent of recurrent government expenditure for education as a whole, and 37 per cent for primary education (Majgaard and Mingat, 2012, 94).

II. Human capital theory: Key predictions and puzzling findings

Although many predictions from human capital models hold in the developed world, some surprising empirical patterns arise in developing countries. Households often behave in ways not predicted by the standard model. Here, I highlight four puzzling findings which I later argue may be explained by some of the psychological concepts I examine – self-beliefs, aspirations and identity.

A. Schooling investments are more cost-sensitive than predicted

Human capital models assume that households trade off the direct and opportunity costs of attending school in the present with the discounted value of future returns. If costs are higher than discounted future returns, households will not enrol children in school.

To examine whether this prediction holds, I examined reviews considering the effect of interventions to reduce schooling costs on enrolment and attendance outcomes (see Table 2.2).[6] Kremer, Brannen, and Glennerster (2013) and Murnane and Ganimian (2014) are qualitative, narrative reviews. Glewwe and Krafft (2014) and Morgan, Petrosino, and Fronius (2012) conduct systematic reviews. Krishnaratne, White, and Carpenter (2013) and Petrosino et al. (2012) are meta-analyses: after a systematic review, they convert results to standardised point estimates and estimate the average effect size.[7]

As predicted by standard human capital models, policies to reduce schooling costs improve household investment of children's time in education. Indeed, it has proven "much easier to design policies to influence families' school investment decisions, at least in the short run, than to design policies that increase educational quality" (Murnane and Ganimian, 2014, 3).

School building programmes, such as in Indonesia (Duflo, 2001) and Afganistan (Burde and Linden, 2013), increase the capacity of the education system to enrol more children and reduce the costs of travelling to school, including the opportunity cost of children's time, so they consistently improve enrolment and attendance.

[6]Other reviews focus on the effects of policies on achievement but do not consider enrolment and attendance and hence are not included (Conn, 2014; McEwan, 2003; Evans and Popova, 2015).

[7]All studies focused on developing countries, except Morgan, Petrosino, and Fronius (2012) who focus on low-income countries. All include randomised controlled trials or quasi-experimental methods (including matching); except Kremer, Brannen, and Glennerster (2013) who only review RCTs, and Murnane and Ganimian (2014) who only review RCTs and natural experiments. Glewwe and Krafft (2014) focus on studies affecting school or teacher level variables and Morgan, Petrosino, and Fronius (2012) focus on elimination of user fees.

Table 2.2: Results of reviews of interventions to reduce schooling costs

Review/years		Building	CCT	Vouchers	Fee reduced	Fee eliminated	Scholarship	School meal	Materials
Meta-analyses [1]									
Krishnaratne et al.	Effect, enrolment	0.403**	0.217***	0.028	0.019			0.156**	
1990 - 2009	Num. studies	4	16	1	2			3	
	Effect, attendance	0.377**	0.198***		0.63***			0.210	0.047
	Num. studies	3	8		1			5	8
Petrosino et al.	Effect, enrolment/ attendance pooled	0.41***	0.17***	0.03**	0.3**	0.02	0.21**	0.14**	0.1**[1]
- 2009	Num. studies	1	13	1	1	1	6	5	1
Vote-counting [2]									
Glewwe et al.	Enrolment	3+ 1(+) 1(0)			1(+) 3(–)		2(+) 1(–)		2+ 1(+) 2(–) 1(0)
1990 - 2010									
Morgan et al.	Enrolment		1+			1+			2[1]
- 2010	Attendance								1+
Narrative [2]									
Kremer et al.	Participation	1+	2+			1+	1+		1[3]
Murnane et al.	Participation	4+	14+	1+; 1(0) 1–	3+			4+, 1–	2+

[1] The average effects are the standardised mean difference (Cohen's d) effect sizes. *** p<0.01; ** p<0.05; * p<0.1.

[2] + is a positive and significant effect, – is a negative and significant effect, (+) or (–) are positive and negative effects that are not significant, and (0) is a zero effect. The number is the number of studies with this effect. So 1+ 3(+) 1(0) means five studies were considered: one had a positive, significant effect; three had a positive, insignificant effect and one had a zero effect.

[3] This estimate is only for uniforms.

Interventions to offset some of the costs and opportunity costs of schooling, such as removing or reducing user fees, conditional cash transfers, or free school meals, improve enrolment and attendance in most of the systematic reviews in Table 2.2. Parents may be cash-constrained in the present, so that they cannot pay fees or bear the costs of children's forgone earnings or work for the household. If there are imperfect or non-existent credit markets, it is more difficult for households to finance the present direct costs and indirect costs of children's schooling by borrowing. Families will underinvest in schooling, even if educating a child will yield net returns: there appear to be some other behavioural or psychological barriers which prevent families investing in education.

Evidence is strong across the various systematic reviews for conditional cash transfers but less conclusive on school meals and the removal of user fees. For example, eliminating school fees in Uganda increased the probability that children attended school before age 8 by about 10 per cent for both genders and enrolment surged by two thirds (Grogan, 2009).

The first surprising finding, not entirely predicted by human capital models, is that school meals and removing user fees do not have conclusively positive effects on enrolment and attendance. If one assumes that there are returns to education and costs are the only barrier to schooling participation, removing user fees would be expected always to improve enrolment.

The second surprising finding is that imposing conditions on cash transfers seems to improve enrolment more than unconditional transfers. Most cash transfer programmes impose conditionalities, such as a certain level of school attendance. Although some studies suggest the cash injection has much of the effect (Baird, McIntosh, and Ozler, 2011; Benhassine et al., 2013), a systematic review finds conditions have additional benefit (Baird et al., 2013). Over 35 studies, programmes with explicit, monitored conditions result in a 60 per cent improvement in the odds of enrolment, while interventions with no conditions or some conditions that are not monitored result in an 18 to 25 per cent improvement. Again, if costs were the only barrier to schooling participation, there would be no difference between conditional and unconditional transfers.

A final surprising finding comes from interventions to reduce small out-of-pocket costs, such as uniforms and textbooks. As expected, these improve enrolment and attendance, affirming that budget and cash constraints are a barrier to investment. Many interventions generated larger changes in enrolment than would be predicted by standard human capital models, unless many households are for some reason right on the margin of sending children

to school (Kremer and Holla, 2009; Holla and Kremer, 2009).[8] Covering all the household costs of education in a trial in Kenya – payments for textbooks, household contributions to classroom construction, and uniforms – resulted in treatment group students staying in school 0.5 years longer after five years and advancing 0.3 grades further than control students (Kremer, Moulin, and Namunyu, 2003). Providing free uniforms in Kenya reduced absenteeism by 44 per cent overall, and by 62 per cent for students who did not previously own a uniform (and raised tests scores the following year by a quarter of a standard deviation) (Evans, Kremer, and Ngatia, 2009), and reduced dropout 15 per cent in another (Duflo, Dupas, and Kremer, 2012). Providing free textbooks had no effects on enrolment for Grade 3–7 students in Kenya but improved the proportion of Grade 8 students entering secondary school in schools that received them from 38 to 43 per cent (Glewwe, Kremer, and Moulin, 2009), a large effect for a relatively low-cost item. Reviews do not always find large, significant effects of providing materials (Glewwe et al., 2014; Petrosino et al., 2012), but there are enough large changes in school participation to constitute a surprising result.

There may be some other mechanisms at play. It is possible that costs are not the only barrier to enrolment. Parents and children may be reluctant to enrol, even when costs are reduced, so that it is necessary to make attendance compulsory. Conditions on cash transfers may give incentive for households to engage in otherwise economically inefficient behaviour. Conditions or free inputs may signal that school attendance is socially desirable. Removal of user fees may suggest services are less valuable and in fact discourage enrolment among some people. As one review notes, "At first blush, the finding that investments in schooling are sensitive to price might seem a vindication of standard models of human capital investment...a more careful examination of the results suggests the need to supplement these models" (Kremer and Holla, 2009, 514-15).

B. Peers affect choices

A second area of puzzlement is that children whose peers are more likely to be attending school are also more likely to attend school. This is not predicted by the standard model, which would suggest that households trade off costs, and the opportunity costs of children's time in work and chores, against future returns (Kremer and Holla, 2009). Under standard

[8]Similarly, health investments are more cost-sensitive than a standard model would suggest, a result found in decisions about purchasing water disinfectant (Ashraf, Berry, and Shapiro, 2010), mosquito nets (Hoffmann, 2009; Cohen and Dupas, 2010) or HIV tests (Thornton, 2008).

models, if some children go to school, labour supply among close substitutes should increase rather than decrease.

However, the opposite occurs. In Mexico, poor families in randomly selected villages were eligible to receive a cash grant for each child who attended school more than 85 per cent of school days. Attendance increased among eligible children at primary school by 5.8 percentage points from a base of 78 per cent, but also increased among children from families above the poverty cutoffs for programme eligibility, by 2.1 percentage points from a base of 78 per cent (Lalive and Cattaneo, 2009). Bobonis and Finan (2009) find that secondary school attendance among non-eligible children increased by 5.0 percentage points from a base of 68 per cent. In Colombia, untreated students in treated students' households did engage more in work, but untreated friends of treated students worked less and were more likely to go to school. This suggests that the standard model works within the household, but an alternative model which accounts for the influence of peers works outside the household (Barrera-Osorio et al., 2011).

C. Private returns may not outweigh opportunity costs; households still invest

Another prediction of human capital models is that investments in human capital improve individuals' market and non-market productivity and earnings over the life cycle. In developed country labour markets like the United States, where the bulk of employment is in the formal sector, there is relatively conclusive evidence that years of education attained increases individual earnings by around 10 per cent per year of education (Card, 2010).

More widely, the most popularly cited literature on returns to education suggests that rates of return are 10 per cent on average across developed and developing countries, and are higher in Africa than in the developed world and other developing regions (Psacharopoulos, 1985, 1989; Psacharopoulos and Patrinos, 2004; Montenegro and Patrinos, 2014). This work uses regressions of log earnings on years of schooling and potential labour market experience (Mincer, 1974): the most recent estimates yield the percentages in the first row of Table 2.3. The remaining three rows show the returns to different levels of education.

Using this method, returns to education for individuals in waged employment in Ethiopia are third-highest in the world, with an 18.5 per cent return to another year of schooling, after Rwanda (22.4) and South Africa (21.1) (Montenegro and Patrinos, 2014, 11). Earnings functions in Africa are still concave: returns are larger at primary than higher levels of

Table 2.3: Private rates of returns to education by level of education, 2004 – 2012

Return to	Ethiopia	Sub-Saharan Africa	South Asia	Latin America	World
A year of schooling	18.5	12.4	7.7	9.2	10.1
Total primary	32.7	14.4	6.0	7.8	11.5
Total secondary	16.2	10.6	5.0	5.4	6.8
Total tertiary	17.0	21.0	17.3	15.9	14.6

Source: Montenegro and Patrinos (2014). These figures use only the latest period of data available for each country, which is between 2004 and 2012 for most countries and 2005 for Ethiopia. Returns at different schooling levels are from the equation: $Ln(w_i) = \alpha + \beta_p D_{p_i} + \beta_s D_{s_i} + \beta_t D_{t_i} + \beta_1 X_i + \beta_2 X_i^2 + \mu_i$. $Ln(w_i)$ is the natural log of earnings, X_i is experience, and D_p, D_s and D_t are dummies equal to one if a person has achieved primary, secondary or tertiary schooling. If, for example, S_s is the total number of years of schooling for each successive level, and r_s is the additional return that a secondary school graduate gets compared to a primary school graduate, $r_s = (\beta_s - \beta_p)/(S_s - S_p)$. The analysis examines only waged employees, eliminating the self-employed, people in voluntary services and the top 0.5 per cent of the sample.

education. These findings are cited to justify extensive donor and government investment in education, particularly at primary level (Psacharopoulos, 1985, 1989; Psacharopoulos and Patrinos, 2004).

These estimates of returns use samples of workers in waged work, which includes only the economically active population working in the formal or informal sector and any people working in agriculture for wages, but exclude self-employed in any sector and subsistence farmers. They might represent returns to education for people with some probability of employment in a wage job. Indeed, these returns are similar to those estimated from panel data in urban areas: Girma and Kedir (2005) find returns of a year of education of 15 per cent in household panel data collected between 1994 and 1997 in urban areas (estimates drop roughly two percentage points when they instrument for schooling with parental schooling). For a sample of 15–29 year-olds in urban areas in 1990, Krishnan (1996) finds returns to primary education of 28.2 per cent in the private sector using an OLS specification, and 9.5 per cent in the public sector (selectivity-corrected estimates are 20.2 and 17.5 per cent respectively).

However, neither set of calculations accounts for a high probability of unemployment. In urban areas, as education systems expand, the supply of skilled labour often far outstrips growth in formal employment, increasing the level of qualification required for even menial formal sector jobs (Bennell, 1996).

Therefore, these returns to education should not be argued to represent the potential returns available to the whole population of developing countries. The use of these figures in this way by the World Bank and other institutions has been heavily criticised (Glewwe and Krafft, 2014; Glewwe, 1996; Bennell, 1996). In Ethiopia, for example, the bulk of the

economically active population will never hold a wage job. Agriculture accounts for 43 per cent of the gross domestic product or GDP (Central Statistical Agency of Ethiopia, 2009). Of the 28,684,000 Ethiopians aged 15 to 59 in the 2004 Welfare Monitoring Survey (WMS), 23.5 per cent are not economically active and 2.6 per cent are unemployed. Of the remaining 73.9 per cent (21,376,000 million people) who are employed, the majority, 76.5 per cent, work in agriculture, while only 7.7 per cent work in the formal sector, and 15.9 per cent work in the informal sector outside agriculture (Majgaard and Mingat, 2012, 240).[9]

Including off-farm self-employment and agriculture causes estimates of returns to drop dramatically: meta-analyses find rates of return to education in off-farm self-employment are, on average, 5.5 per cent in developing countries, although most studies do not correct for ability bias, which is likely to bias estimates of returns upwards (Van der Sluis, van Praag, and Vijverberg, 2005). Similarly, returns to education are much lower in agriculture (Phillips, 1994). These meta-analyses also find evidence of a threshold effect, with a limited return to low levels of primary education but some return to higher levels of education, the opposite of results using the simple Mincerian return calculation.

Furthermore, returns may differ by the type of agriculture. If there are exogenous technological or other shocks, educated farmers are better able to adapt and more productive (Schultz et al., 1975): education raises farmer productivity more in areas with greater weather instability (Gurgand, 2003) and during times of technological change (Foster and Rosenzweig, 1996). Better educated farmers adopt new technologies faster (Alene and Manyong, 2006; Feder, Just, and Zilberman, 1985; Lin, 1991; Asfaw and Admassie, 2004), including in Ethiopia (Croppenstedt, Demeke, and Meschi, 2003; Weir and Knight, 2004). However, in traditional agricultural settings where tasks are simple, schooling has only minor effects on productivity (Yang, 1997; Rosenzweig, 1995). Some evidence finds that more educated farmers are more productive on average (Ali and Flinn, 1989; Young and Deng, 1999; Phillips, 1994; Alene

[9]There are further problems: these calculations neglect that the bulk of formal employment in many low-income countries is still in the public sector (Pritchett, 2001; Gelb, Knight, and Sabot, 1991), despite job cuts following structural adjustment (Colclough, 1995). In Ethiopia, of the 1,651,000 people in formal employment in the WMS data in 2004, 42.5 per cent work in the private sector and 47.5 per cent work in the public sector (Majgaard and Mingat, 2012, 237). Human capital models assume markets function efficiently and earnings reliably indicate individual productivity, but public sector wage determination is usually non-competitive, distorting estimates in unpredictable ways (Glewwe, 1996; Bennell, 1996). In a random sample of urban Ethiopian workers in 2000, the median wages of private sector workers were two-thirds of those in government or public enterprises (Sandefur, Serneels, and Teal, 2007). Furthermore, ability (Griliches, 1977) or quality of schooling (Hanushek and Woessmann, 2008; Behrman and Birdsall, 1983) are correlated with attaining higher levels of education but are often omitted and may bias estimates; measurement error in years of education can lead to underestimates of returns and the measure may be an imperfect proxy for skills (Hanushek and Woessmann, 2008); and private returns do not account for externalities (Temple and Reynolds, 2007; McMahon, 2001).

and Manyong, 2006), but this tends not to account for unobservable factors, like ability or determination, that influence both educational attainment and productivity. Although the Ethiopian government uses this argument to justify spending on universal primary education, evidence for a link between education and productivity in traditional agriculture is relatively weak.

Given the costs of education, the relatively low returns, the low likelihood that Ethiopian children will complete primary school, and the evidence that returns only set in once children have completed some education, it is somewhat surprising that households are so likely to enrol children in the early grades of primary school. This is particularly true given the poor quality of education in many developing countries (Pritchett, 2001). Schultz (1981, 2-5), cited in the introduction to this chapter, sought to explain why poor households might be rational not to invest in education: the current puzzle is why they continue to invest so substantially.

D. Beliefs about opportunities and returns to education affect investment

Human capital models predict that household investment in human capital responds not only to variations in the direct and indirect cost of schooling, as it would if education were simply a good, but also to variations in returns in future earnings to additional years of schooling. Earlier research, in line with the models' predictions, finds households are conscious of and make decisions according to knowledge about the returns to education. More educated farmers were much more able to take advantage of new technologies during the Green Revolution in India, so returns to education increased, particularly where a high degree of technological progress took place. Families increased investment in education more in those areas (Foster and Rosenzweig, 1996). Households may realise, accurately, that better education increases the likelihood that rural workers will be in higher-paying non-farm employment, while those with no education tend to work in agricultural employment, at least in rapidly growing economies like Vietnam, China and India (de Brauw and Harigaya, 2007; Du, Park, and Wang, 2005; Kajisa and Palanichamy, 2006). They may make investments by calculating whether education will enable children to migrate to urban areas to access higher-return non-farm employment. Rural Indian parents are aware that the rate of return to education beyond middle school is higher in urban than rural areas, and appear to make decisions based on urban returns (Kochar, 2004). Rural Filipino households invest heavily in educating children, most of whom later engage in rural non-farm jobs or migrate to cities and send

remittances home (Takahashi and Keijiro Otsuka, 2009; Quisumbing, Estudillo, and Otsuka, 2004).

However, another strand of research suggests that parents and children have limited information about the returns to education. When parents and children receive better information about the returns to education, they alter investment in education. In the Dominican Republic, the perceived returns to secondary school are extremely low, despite high measured returns. Students at randomly selected schools who were given information on returns completed 0.20–0.35 more years of school over the next four years than those without this information (Jensen, 2010). Another study randomly selected Indian villages for recruitment visits publicising opportunities in call centres. Parents in these villages were more likely to enrol girls in school, feed them better, and expect them to get married later (Jensen, 2012). Young women increased their labour market participation and enrolment in relevant courses, delayed marriage and first children, and reported higher career aspirations. Yet another study in India found that one new call centre increased school enrolment in the surrounding district by 5.7 per cent, which the authors attribute to children and families in the immediate area having information that economic changes were occurring that had altered the return to education (Oster and Millett, 2011).

Households may also respond to information about opportunities and costs of education. In Chile, children and families were shown a DVD showcasing the higher education experiences of thirteen adults who grew up in poor families in urban Chile but financed their post-secondary education by working hard at school and becoming eligible for financial aid (Dinkelman and Martínez, 2014). In the US, providing information on school test scores to lower-income families in a public school choice plan increased the proportion of parents who chose a high-performing school, which in turn increases student test scores (Hastings and Weinstein, 2008).

Households also respond to better information about their children's abilities. Malawian parents who received information via an easily understood report card about their children's relative achievement reallocated resources towards their higher achievers (Dizon-Ross, 2014). Similarly, in Mexico City, poor students often incorrectly anticipated their scores on a centralised admissions test and even high performers chose non-academic tracks for high school. Providing some students with their scores on a mock version of the admission achievement

test resulted in better alignment between students' performance and their choice of school track (Bobba and Frisancho, 2014).

These interventions are usually interpreted as addressing the fact that information may be incomplete, imperfect, asymmetrically distributed among parties in a market, or costly to acquire, and that addressing information failures changes behaviour (Akerlof, 1970). Research on imperfect information relaxes the standard neoclassical assumption that all parties in a market have perfect information, although it still assumes that agents form correct beliefs based on available information and maximise utility. Information failures may be particularly prevalent in developing countries (Stiglitz, 1986); and this theoretical framework is also used outside education to understand responses to information about national employment programmes in India, delivered through a movie (Dutta et al., 2014), precautions against HIV infection (Dupas, 2011) and use of contraception (Munshi and Myaux, 2006).

However, the seeming disagreement between earlier and later findings may occur because it is not entirely clear that interventions to provide information to households are simply pure information interventions and that results can be attributed solely to the provision of previously unavailable information. It is very difficult to think of a pure information intervention that generates exogenous variation in the information available without also generating the other effects mentioned above.

The next chapter will discuss alternative theoretical frameworks which could be used to interpret these results, suggesting that other psychological mechanisms, rather than simply the presence or absence of information, may be at work. For example, from a behavioural economics perspective, interventions may make information more salient (Hanna, Mullainathan, and Schwartzstein, 2012) or memorable (Tversky and Kahneman, 1974), or alter the way decisions are framed (Ariely, Loewenstein, and Prelec, 2003). Even very basic, objectively provided information, like the simple statements read to students in the Dominican Republic, may prime respondents to behave in particular ways, particularly when combined with a survey asking about education outcomes and behaviours. Alternatively, psychologists who emphasise the importance of identity and stereotypes have often used exposure to inspirational role models, like those in the studies of Chilean financial aid videos or the families who lived near call centres, to break people's perceptions that they do not suit particular roles or activities, or to enhance people's aspirations (Lockwood and Kunda, 1997; Asgari, Dasgupta, and Cote, 2010; Greene, Sullivan, and Beyard-Tyler, 1982).

III. Conclusion

Human capital theory about household decisions to invest in children's schooling explains many patterns in behaviour. However, more detailed examination of empirical results from developing countries suggests the need to supplement these models. Although economists of education in developing countries still tend to refer largely to standard human capital models, they increasingly draw on strands of literature that weaken the assumptions about beliefs and preferences in the standard neoclassical model, as will be discussed in the next chapter.

Firstly, investing in education has costs, so if the costs of education decrease or households become less cash- or credit-constrained, investment in education increases. Policies to reduce schooling costs improve household investment of children's time in education. Interventions which reduce costs or opportunity cost of attending school – such as school-building programmes, cash transfers, the removal of school fees, school meals and provision of inputs like textbooks and uniforms – all improve participation in school.

Improvements in access to education in Ethiopia have been some of the world's most dramatic. The Ethiopian government has implemented many of the recommendations of standard human capital models, in particular by cutting the costs of attending school by eliminating fees and building schools. The majority of Ethiopian children now attend school, although completion rates for primary school remain a concern. Schools continue to use other fees and payments to pass costs on to families, so sending children to school continues to require substantial investment by households.

However, the findings on household reactions to changes in the costs of schooling are not entirely predicted by standard models. Providing school meals and removing user fees do not have conclusively positive effects on enrolment and attendance. Cash transfers are most effective in improving school participation when conditions are attached, although if costs were the only barrier to schooling participation, there would be no difference between conditional and unconditional transfers. Interventions to provide free inputs generate larger changes in enrolment than would be predicted by standard human capital models, unless many households are right on the margin of sending children to school.

Secondly, children whose peers outside the household are attending school are themselves more likely to attend school. Again, this is not predicted by the standard model. If some children go to school, labour supply among close substitutes should increase rather than decrease. However, the opposite occurs.

Thirdly, human capital models also predict that investments in human capital improve individuals' market and non-market productivity and earnings over the life cycle, and this holds in developed country labour markets and in more urban, formal sector labour markets in developing countries. In Ethiopia, the returns to education in government and the formal private sector are high. However, returns to education on average for the whole population in developing countries are likely to be much lower and there may be threshold effects. Education may assist farmers to cope with technological advances or shocks, but in traditional agricultural settings, schooling has very limited effects on productivity. Given the costs of education, the relatively low returns, the low likelihood that Ethiopian children will complete primary school, the low quality of schooling and the evidence that returns only set in once children have completed some education, it is somewhat surprising that households are so likely to enrol children in the early grades of primary school.

Finally, human capital models predict that household investment responds to changes in the market returns to education. Earlier research does find that households make decisions according to knowledge about the returns to education. However, another strand of research suggests parents and children have limited information about the returns to education. When parents and children receive better information about the returns to education, they alter investment in education. The seeming disagreement between earlier and later findings may occur because interventions to provide information to households are not simply pure information interventions, but may be explained by other psychological mechanisms. These are explored in the next chapter.

3

The relationship between economics and psychology

Standard neoclassical economic models are based on a series of assumptions about the behaviour of human actors' beliefs and preferences. The microeconomic part of human capital theory, discussed in the previous chapter, applies a standard neoclassical model to household decisions about investments in education, health and nutrition. The third, more conceptual research question in this thesis asks whether findings from applying concepts of self-belief, aspirations, and identity imply that these assumptions have to be substantially modified or even rejected. By way of preparation, it is necessary to achieve some order in the way economists, variously and sometimes contradictorily, have invoked psychological concepts.

In the first section of the chapter, I discuss the evolution of the relationships between psychology and economics. After close interaction in the late nineteenth century, modern neoclassical economists rejected use of concepts based on unobservable mental states, culminating in the revealed preference school of thought.

Neoclassical economists have become more tolerant of such concepts after problems with the idea of revealed preference became clear, and research on imperfect information highlighted the importance of beliefs. However, they remain somewhat apprehensive of concepts which are difficult to measure objectively, such as beliefs and preferences, and make relatively rigid assumptions about how they behave.

There is a tendency to lump together all research in economics drawing on any part of psychology as part of behavioural economics (Angner and Loewenstein, 2012). However, I argue that different schools of thought in psychology have distinct orientations to core problems relevant to economics (Yang and Chiu, 2009). Different areas of research in economics have drawn on different parts of psychology, often somewhat haphazardly, and may reflect perspectives that are only one side of a debate as conclusive findings.

In the second section, I distinguish two paths subsequently taken by economists in drawing upon psychology. On the first path, work on non-cognitive skills slightly broadens the neoclassical ideas of skills, an agent's endowment of ability, to include non-cognitive skills. This area of economics draws on part of social psychology which studies personality and focuses on how internal dispositions shape behaviour. Other areas of social psychology emphasise the role of situational, cultural and historical factors, but research on non-cognitive skills does not really acknowledge this. Research on non-cognitive skills also draws on cognitive developmental psychology, which examines the physical and neural influences on the development of cognition and skills in children, rather than social developmental psychology, which emphasises the importance of social interactions in children's development.

Partly as a result of these influences, non-cognitive skills in economics are viewed as relatively stable across multiple contexts, because they are shaped by one's underlying disposition, and relatively fixed by adulthood, because of how the brain develops. However, this is one particular conception of non-cognitive skills, which may obscure some of the nuance of psychological debates.

Given these two characteristics, including these skills in economic models does not really require modifications to the assumptions made in the standard model about the characteristics of beliefs, preferences and skills. Non-cognitive skills can be viewed as part of an individual's endowment.

The second path is that taken by behavioural economists. Behavioural economics draws mostly on cognitive psychology, in particular Behavioural Decision Research (BDR), which highlights that properties of our cognitive processes explain many of our judgements and decisions. Behavioural economists continue to use the ideas of belief and preference, but have been more radical in requiring a number of substantial modifications to assumptions made in the standard model about them. People do not process all information or make decisions in a rational, deliberative fashion: they make snap judgements based on heuristics or schemas. Thus, they satisfice rather than optimise. Instead of being self-interested, they can be self-interested or altruistic. Beliefs, preferences and to some extent skills are not fixed but "frame-dependent" (Hoff and Pandey, 2014, 120) and vary in different contexts as well as over time. Relatedly, behavioural economists aim to examine agents' processes of deliberation and decision-making empirically, rather than abstract from them. There is variation within behavioural economics, but most work includes at least some of these modifications.

In the subsequent four chapters, I apply concepts of self-belief, aspirations and identity in empirical work. In the conclusion, I assess which of these two paths is most appropriate for integrating these concepts into economics. Furthermore, I highlight that social psychology's emphasis on the role that human agency and the creation of meaning play is a valuable addition to the set of psychological ideas used in economics.

I. Modern neoclassical economics and psychology: separation at birth

Before Vilfredo Pareto, both economics and psychology were relatively young disciplines and boundaries between them were not sharply defined. Many early neoclassical economists – William Jevons, Jeremy Bentham, Francis Edgeworth – based their assumptions about human behaviour on results from experimental psychology. For example, Edgeworth cites two early psychologists to support his assumption of diminishing marginal utility (Bruni and Sugden, 2007, 151-3).[1] They were Gustav Fechner and Wilhelm Wundt, two late nineteenth century hedonic psychologists, who aimed to identify and classify the basic elements or structures of psychological experience, measuring mental phenomena like pleasure and pain by exploring their relationships with material stimuli (Nolen-Hoeksema et al., 2014).

A. The Paretian turn in economics

Vilfredo Pareto's theory of choice began the so-called "Paretian turn" in economics in the early twentieth century, in which "every psychological analysis is eliminated" (Pareto, 1900, 214). Pareto proposes "that economic theories should be deduced from firmly-established propositions about choice rather than about sensation" (Bruni and Sugden, 2007, 155), or indeed tastes and preferences more broadly (Hausman, 2012, 47). He never quantifies the utility that an agent derives from a bundle of goods in terms of the pleasure or pain derived from consuming the bundle: the theory only relies on knowing agents' ranking of different bundles and between which bundles agents are indifferent.[2] His "division of labour" between

[1] John Stuart Mill, on the other hand, argues economics should focus on explaining "economic man", a fictional creature driven only by self-interested economic motives, so he makes more abstract and less empirically grounded assumptions about what will cause pleasure (Mill, 1836).

[2] Pareto, ironically, spent the latter part of his life arguing that much personal action is designed to give spurious justification to non-rational actions and that social structures are important drivers of human action (Pareto, 1935). He was an important inspiration for Talcott Parsons, one of the founders of modern sociology.

economics and psychology persisted for the first half of the twentieth century (Hausman, 1992a).

At the time of Pareto and his modern neoclassical successors, logical positivism dominated philosophy of science. It "eschewed entities...that could not be readily observed and reliably measured" and was sceptical of the scientific value of explanations based on unobservable, unverifiable mental states (Gardner, 1985, 15). Behaviourism was on the ascendancy in psychology. Behaviourists aim to move away from constructs such as beliefs, desires, plans and intentions, which refer to mental states but have no directly observable correlates. (Gardner, 1985, 11). They argue that only stimuli, i.e. events in the environment, and behavioural responses to them are publicly observable and objective phenomena, so psychology ought to be a science of behaviour and focus on how biological drives and other stimuli cause human behaviours. In their view, human behaviour is shaped and controlled automatically by environmental stimuli (Watson, 1930; Skinner, 1957, 1972). Most behaviour is argued to be the result of conditioning and reinforcement. Although they posit that some internal mental conduit links these two, they argue it does not exert any influence on behaviour (Hunt, 1993).

In this environment, it is no wonder that modern neoclassical economists seized upon, extended and popularised Pareto's choice theory (Robbins, 1932; Hicks and Allen, 1934). They were scornful of the lack of predictive power of early neoclassical economics, based on methods of introspection (Hutchison, 1938). They distanced their discipline from its previous roots in hedonic psychology. They favoured Pareto's work in part because, as in behaviourist psychology, it avoids engaging with unobservable mental states (Ross, 2005; Angner and Loewenstein, 2012; Hausman, 2012).

At one extreme, revealed preference theorists (Arrow, 1959; Richter, 1966; Samuelson, 1955) assume that if an agent's choices are observable and consistent, one can impute a set of preferences that fulfils the axioms of ordinal utility theory and implies the choice the agent has made (Gul and Pesendorfer, 2008). Preferences are implicit in choices and revealed when choices are made. Arguably, revealed preference theory arose in part because "Economists do not always feel comfortable when mental states are invoked in explaining phenomena of interest" (Reiss, 2013, 32). Little (1949, 97) asserts, for example, that "the new formulation is scientifically more respectable [since] if an individual's behaviour is consistent, then it must be possible to explain that behaviour without reference to anything other than behaviour".

Revealed preference theory is now less widely used, in part because of the increasing prominence of the extensive literature on imperfect information. Imputing preferences from choices requires one to make the assumption that agents have perfect information. If beliefs or expectations are incorrect or change, one cannot infer preferences from choices (Hausman, 2011, Chapter 3). Another limitation is that preferences are limited to alternatives among which agents actually choose (Hausman, 2005; Reynolds and Paris, 1979, 356). However, agents may have preferences about alternatives that are not in their choice set, which explain or predict economic behaviour. Indeed, game theorists explain players' choices of strategy with assumptions about their preferences over all possible outcomes, including choices which agents are never in a position to select (Rubenstein and Salant, 2008; Reiss, 2013, 62-3). However, the neoclassical apprehension of psychological concepts is still relatively widely prevalent (Angner and Loewenstein, 2012; Hausman, 2012).

B. Fixed preferences and beliefs; agents optimise

Neoclassical economic models are based on the axioms of ordinal utility theory, which define the characteristics preferences must have to be considered rational, as well as a series of auxiliary assumptions (Hausman, 2014, 2012, Chapter 2). Preferences are assumed to be transitive; complete (people either rank all possible alternatives or are indifferent between them); and choice-determining (among the alternatives available, people will choose the one they most prefer) (Varian, 1984; Mas-Colell, Whinston, and Green, 1995, 115).[3] They are also assumed to be consistent, in that they are both context-independent and stable over time (McClennen, 1990). Agents are further assumed to optimise and act to maximise their own utility.

Unlike in revealed preference theory, preferences cannot simply be inferred from choices: the axioms do require some characterisation of preferences. The axioms are usually argued both to define rationality and to approximate actual preferences, so they are part of a positive theory that explains and predicts actual choices (Varian, 1984; Mas-Colell, Whinston, and Green, 1995). But they are not based on empirical findings about how preferences and beliefs behave – they lack "psychological foundations" (Mandler, 1999).

Neoclassical economists assume that people have perfect information, so that people's beliefs coincide with what is actually the case. However, if agents can form mistaken beliefs,

[3]If completeness, transitivity and some technical conditions (such as reflexivity and continuity) hold, people's preferences can be represented by continuous utility functions (Debreu, 1959).

lack all the facts, or interpret them incorrectly, they might fail to choose their most preferred alternative because they think it is not feasible. To account for this, one might revise the axiom of choice determination to be "Among the alternatives that they correctly believe to be feasible, agents choose one at the top of their preference ranking" (Hausman, 2014, 4). Thus agents' beliefs and preferences determine their choices.

C. Self-interest

Philosophers of economics disagree about whether non-self-interested preferences fit within rational choice theory. In the seminal paper "Rational Fools", Sen (1977) distinguishes between three motivations: narrow self-interest, sympathy and commitment. Actions motivated by narrow self-interest aim at maximising one's own welfare, which is determined only by the goods one enjoys. Sen (1977) argues that economists usually assume that this is people's primary, if not only, motivation.

Sympathy refers to when "one person's welfare is being affected by the position of others" (Sen, 1977, 326). Importantly, actions motivated by sympathy are not altruistic: agents act in their own self-interest, but their self-interest is affected by what happens to others.

However, people are also motivated by commitments, considerations which do not affect their own welfare, however broadly it is defined. "If knowledge of torture of others makes you sick, it is a case of sympathy; if it does not make you feel personally worse off, but you think it is wrong and you are ready to do something to stop it, it is a case of commitment" (Sen, 1977, 326). Sen (1977; 1985; 2007) argues that the notion of commitment can capture both moral commitments and other concerns like culture, social norms or rules of behaviour. However, in his view preferring to behave to comply with a commitment does not fit with economists' account of preferences. Thus economists should develop non-preference-based theories of choice to allow for motivations other than self-interest.

Other philosophers of economics argue that, strictly speaking, the axioms do not actually state that each individual is motivated only by self-interest (Hausman, 2014, 2012). Preferences and utility are not defined by any substantive content: they simply indicate where alternatives are located in the agent's ranking. Although some agree with this account of preferences (Baigent, 1995, 91) and many reviewers agree that it is philosophically coherent, most also argue that it is not widespread among economists (Pattanaik, 2013; Lehtinen, 2013; Sugden,

2012; Teschl, 2013; Davis, 2011). Neoclassical economists do cling to ideas of preferences as being self-interested (Teschl, 2013).

D. Deliberative processing

Standard models assume both that agents have all available information and that they process it fully, so that beliefs capture the actual state of affairs. Agents in standard models are assumed to have extensive information-processing capabilities and to spend considerable time in reflection.

In addition, for agents to have complete preferences over all alternatives, they have done considerable work and reflection (Mas-Colell, Whinston, and Green, 1995). Preferences are comparative judgements: one can desire two alternatives, but not prefer them both. Preferences are also subjective: only the agent making choices knows what they take to be the alternatives they face and what they believe the consequences of their choices will be (Hausman, 2012, 2007).

However, modern neoclassical theory does not come with an empirically grounded account of the formation of preferences and beliefs (Angner and Loewenstein, 2012, 650). Neoclassical economists prefer to leave it to other social scientists to study the intricate, context-specific processes through which agents form and modify their preferences given their values, motivations, aspirations and beliefs (Hausman, 1992a). Economists then take agents' preference rankings as given, and study the consequences of choices based on them. Robbins (1932, 86), one of the first modern neoclassical economists, stresses: "Why the human animal attaches particular values in this sense to particular things, is a question which we do not discuss. That is quite properly a question for psychologists or perhaps even physiologists." Later neoclassical economists vigorously agree:

> On the traditional view, an explanation of economic phenomena that reaches a difference in tastes between people or times is the terminus of the argument: the problem is abandoned at this point to whoever studies and explains tastes (psychologists? anthropologists? phrenologists? sociobiologists?) (Stigler and Becker, 1977, 76).

Generalisations about people's preference rankings from other disciplines are spurned, because they are assumed to be "ad hoc and have only narrow scope" (Hausman, 1992a, 274), unlike the universally applicable axioms of standard models. As Akerlof (1984, 2) puts it:

> Economic theorists, like French chefs in regard to food, have developed stylised models whose ingredients are limited by some unwritten rules. Just as traditional French cooking does not use seaweed or raw fish, so neoclassical models do not make assumptions derived from psychology, anthropology, or sociology.

II. Economic research drawing on psychology

In this section, I distinguish two paths taken by economists in drawing upon psychology in two different areas of research, non-cognitive skills and behavioural economics. I highlight which areas of psychology each group of economists has drawn on. I also highlight major debates in psychology which each group of economics perhaps skates over, in favouring the particular area of psychology they have adopted. And, finally, I discuss the extent to which economists have modified the assumptions of standard models so that they can incorporate psychological concepts.

A. Non-cognitive skills

Neoclassical economists have long acknowledged that individuals have different endowments of skills. Cognitive skills do not fully explain variation in people's labour market outcomes and wages (Bowles and Gintis, 1976). Employer behaviour also suggests that they value skills other than cognitive skills: for example, employers often use personal interviews and letters of recommendation (Jencks, 1979). Economists began to consider personality traits and "non-cognitive skills" as part of the endowment of capabilities that affect productivity.[4]

Psychologists mainly use the term "non-cognitive skills" to refer to capabilities that are controlled by "executive functions", a set of neural building blocks largely in the temporal and frontal lobes of the brain which enable people to set goals, plan and execute them in an organised way (Hughes and Graham, 2002). Economists define non-cognitive skills more broadly than psychologists, which may be because the concept was originally designed to capture any non-cognitive characteristic that is correlated with productivity (Camfield, 2014, 2). James Heckman (2008, 298), a leading economist in the field, defines non-cognitive skills as "motivation, socio-emotional regulation, time preference, personality factors and the ability to work with others". In other papers, he excludes some of these characteristics but includes

[4]It is difficult in practice to draw a clean distinction between cognitive and non-cognitive skills, as few aspects of human behaviour are devoid of cognition (Borghans et al., 2008).

perseverance, adaptability, task persistence, self-discipline and thinking ahead (Heckman and Rubenstein, 2001); risk aversion, attention, socio-emotional skills and self-confidence (Heckman, Stixrud, and Urzua, 2006); or self-esteem, self-control and patience (Cunha and Heckman, 2008). More recently, he even argues that character should not be considered a set of traits, but a skill, because considering character as a skill emphasises its malleable nature throughout the life cycle (Heckman and Kautz, 2013).

However, this characterisation may have become so broad that it is not particularly meaningful. Correlations between different non-cognitive skills are low relative to correlations between tests of cognitive ability (Borghans et al., 2008) and different measures of non-cognitive skills do not cluster as expected (Heckman, Stixrud, and Urzua, 2006). As a result, many economists are focusing on personality as a "distinct and measurable subset of non-cognitive skills" (Camfield, 2014, 5) (see Almlund et al., 2011; Borghans et al., 2011; Brunello and Schlotter, 2011).

A.1 Cognitive developmental psychology and stable, context-independent non-cognitive skills

Research on non-cognitive skills draws on the work of cognitive developmental psychologists such as Jean Piaget, who examine how children develop cognitive skills such as perception, thought processes, memory, attention and language (Piaget and Inhelder, 1969). More recently, researchers have examined development of executive functions. They include cognitive flexibility, which enables people to respond appropriately to changing situations; planning, which helps people to work out the steps necessary to achieve their goals; and working memory, which supports holding many things in mind while pursuing a goal; (Morgan and Lilienfeld, 2000). Some capacities controlled by executive functions include motivation (in this context, the desire to accomplish and do well on a task, which energises behaviour and gives it direction (Deci and Ryan, 1985)), self-control (the ability to control one's emotions and regulate one's behaviour in pursuit of a goal (Bandura, 1977, 128-9)) and perseverance (a willingness to master skills and complete tasks (Duckworth et al., 2007)).

Most work emphasises that the ideal time to develop these skills may be age 13 to 20, when the brain is relatively plastic, but not so plastic that the effects of the training are displaced by subsequent experiences. There is not a clear cut-off point, as with cognitive skills, when development ends: the prefrontal cortex which governs emotion and self-regulation continues

to develop until the early twenties and potentially as late as the thirties (Heckhausen and Singer, 2001). Researchers highlight that there is a relatively long window of opportunity to improve some non-cognitive skills (Heckman et al., 2010; Cunha and Heckman, 2008; Heckman, Pinto, and Savelyev, 2013) compared to cognitive skills, which primarily develop before age three (Grantham-McGregor et al., 2007). However, most work on non-cognitive skills emphasises that these skills are relatively fixed for an individual by the time they reach adulthood.

Economists have not really drawn on the perspectives of social developmental psychologists. This school of psychology focuses more on the development of self-concept and interpersonal relationships, emphasising that children learn most skills through social interaction and observational learning, and the skills are often specific to their environment and cultural norms (Rogoff, 2000; Vygotsky, 1934). As I will outline in subsequent chapters, social psychologists emphasise that motivation, perseverance and confidence are influenced by people's executive functions, but also by their beliefs about themselves and their capabilities (Bandura, 1977). These are perceptions and interpretations of the world and oneself, subjective realities which individuals actively construct, which may be relatively stable across contexts but are likely to evolve over time, because they are less constrained by brain plasticity. They also respond to diverse influences, not only direct experience, but also learning from watching others or persuasion (Bandura, 1997).

A.2 Personality psychology and stable, context-independent personalities

More recent economic research on non-cognitive skills which focuses on personality also draws on part of social psychology which studies how personality and internal dispositions shape behaviour. Personality is defined as "the distinctive and characteristic patterns of thought, emotion, and behaviour that make up an individual's personal style of interacting with the physical and social environment" (Nolen-Hoeksema et al., 2014, 462). Researchers "stress that individuals differ systematically in their reactions to a given situation", attributing these differences to "internal factors that predispose certain responses", such as personality traits and social attitudes (Ajzen, 2012, 367).

Economists in this area thus contend that personality is relatively fixed across contexts:

> People tend to behave in the same fashion across a wide range of situations. Evidence from genetics and neuroscience provides a biological basis for the ex-

istence of such traits, suggesting that something tied to the person, not just the situation, affects behaviour (Heckman and Kautz, 2012, 452).

Other areas of social psychology emphasise ""emphasise the power of the situation to direct human social behaviour" (Ajzen, 2012, 367). At one extreme, constraints and incentives in particular situations are argued to entirely determine behaviour (Mischel, 1973). More moderately, social learning theory, an approach which bridges cognitive theory and social psychology, holds that internal cognitive processes and the environment combine to influence behaviour and have reciprocal effects on each other (Bandura, 1986, 2001). Thus culture, nurture, personal experience and observation of others all play a role in how personality develops and influences behaviour in different situations.

Other researchers emphasise that people's perceptions are a function of culture and history as well as individual traits (Wilson, Huttly, and Fenn, 2006, 1251). People's behaviour is strongly influenced by social norms – perceptions, feelings, thoughts or behaviours that are shared by group members and thought by them to be acceptable – and the desire to identify with a reference group (Asch, 1952; Cialdini, 1993). Emotions, beliefs and behaviours are affected by the presence of others, real or imagined, explaining behaviours like conformity (Nolen-Hoeksema et al., 2014).

Although economists exhibit some awareness of these debates (Heckman and Kautz, 2012), they cite recent evidence that personality is stable across multiple tasks and situations, and often stable over time, so that people act in ways which can be strongly predicted by their personality (Donnellan, Lucas, and Fleeson, 2009). They argue that evidence from evolutionary psychology shows that personality traits are substantially determined by genetics (Bouchard and Loehlin, 2001), and regions of the brain which remain relatively stable in adulthood are associated with different traits (Canli, 2006; DeYoung et al., 2010). Some traits, such as extraversion and conscientiousness, even become stable in childhood (Shiner, Masten, and Roberts, 2003).

A.3 Integration of non-cognitive skills into standard models

Because of their focus on personality psychology and cognitive developmental psychology, researchers on non-cognitive skills believe these skills to be relatively stable. Stability has two dimensions. Firstly, non-cognitive skills are viewed as shaped by one's underlying disposition

and relatively stable across multiple contexts. Secondly, they are relatively fixed and stable by adulthood, because of how the brain develops.

If non-cognitive skills are relatively context-independent, just like preferences and beliefs (Heckman and Kautz, 2012, 452), and apply to and function in virtually all aspects of life, they can be integrated relatively easily into standard economic models as part of an individual's endowment of skills. Their perspective does not challenge any of the other characterisations of beliefs and preferences in standard economic models.

B. Behavioural economics

Some behavioural economists argue that they effectively work within the neoclassical approach, based on the ideas of equilibrium, efficiency and utility maximisation, and modify only one or two assumptions, often those which are not core (Camerer and Loewenstein, 2003). However, in comparison to the non-cognitive approach described above, behavioural economists propose more radical alterations to the assumptions of standard economic models. Behavioural economics has been comprehensively surveyed elsewhere (Rabin, 1998; Camerer and Loewenstein, 2003; Kahneman and Tversky, 1984; Kahneman and Thaler, 2006; DellaVigna, 2009). Here, I simply briefly describe the areas of psychology on which it draws and the modifications behavioural economists suggest to standard economic models.

B.1 Cognitive psychology

In the 1960s, George Miller, Noam Chomsky, Jerome Bruner, and Roger Brown led the "cognitive revolution", which rejected behaviourism and aimed to bring the mind back into psychology (Green, 1996; Miller, 2003). They argue that "Thinking can best be understood in terms of representational structures in the mind and computational procedures that operate on those structures" (Thagard, 1996, 10). Cognitive psychologists aim to unpack the black box between stimulus and response (Miller, 2003). They still study specific behaviours, but interpret them in terms of underlying mental processes. And, following on from Herbert Simon's (1955) demonstrations of how psychological phenomena could be simulated by computers, researchers argue that even complex aspects of human thinking, including judgement and decision-making, can be captured with computational models, according to which we "compare, combine, and record...mental representations" (Hastie and Dawes, 2001, 9).

Behavioural economics has largely drawn inspiration from one strand of cognitive psychology, behavioural decision research (BDR) (Angner and Loewenstein, 2012). The first work in behavioural economics was by two cognitive psychologists, Daniel Kahnemann and Amos Tversky (1979). BDR focuses on how the properties of our cognitive processes explain many of our judgements and decisions, and in particular many departures from optimal decisions due to our limited working memory (Hastie and Dawes, 2001, 10-12). A key emphasis is on common patterns in cognition across culture, time and individuals:

> We have a common set of cognitive skills that are reflected in similar decision habits. But we also bring with us a common set of limitations on our thinking skills that can make our choices far from optimal (Hastie and Dawes, 2001, 2).

Cognition is conceptualised in terms of mental structures – symbols, rules, images, heuristics – and neural processes such as perception, thinking, memory, problem-solving and judgement which operate on those structures to perceive, process and retrieve information and make choices (Nolen-Hoeksema et al., 2014). More recent work in cognitive psychology on mental models, schemata and categories is much more amenable to allowing that cognitive processes differ to some extent between individuals and are also shaped by culture, history and personality. "[S]timuli are mentally construed, interpreted and understood (or misunderstood)...behaviour is directed not toward actual states of the world, but toward mental representations of those states" (Shafir, 2013, 2).

B.2 More radical alterations to the assumptions of the standard model

Behavioural economists acknowledge that predictions based on standard models are often accurate for markets, but argue that they are often inaccurate descriptions of the behaviour of human beings (Camerer and Loewenstein, 2003). They point to evidence accumulating from laboratory experiments and increasingly from field environments that decision-making behaviour is less well described by standard theory than by psychologically based theories. They thus aim to replace the assumptions about human decision-making processes and preferences on which neoclassical models are based with more "psychologically plausible" assumptions, by which they mean "consistent with the best available psychology" (Angner and Loewenstein, 2012, 652).

Some behavioural economists argue that "Behavioural economics simply rekindles an interest in psychology that was put aside when economics was formalised in the latter part of

the neoclassical revolution" (Camerer and Loewenstein, 2003, 44). They point to parallels with the cognitive revolution in psychology, where cognitive psychology developed in contrast to behaviourism. They argue that behavioural economics is as necessary a part of the evolution of economics as the cognitive revolution was: "Both fields are based on a repudiation of the positivist methodological strictures that were in place at their founding and a belief in the legitimacy of making reference to unobservable entities such as emotions and heuristics" (Angner and Loewenstein, 2012, 680).

Early work in behavioural economics involved identifying anomalies in behaviour which did not accord with standard models (Kahneman, Knetsch, and Thaler, 1991; Tversky and Thaler, 1990). Later work incorporates behavioural assumptions into models of economic phenomena (Rabin, 2002). I highlight four key modifications which behavioural economists suggest to the core assumptions of standard economic models.

B.3 Two types of processing: Automatic and deliberative

In some situations, people do deliberate carefully and consciously between alternatives by calculating costs and benefits and forming preferences, as the standard model suggests. However, the "biases and heuristics" strand of research of Kahneman and Tversky (Tversky and Kahneman, 1974; Kahneman, 2003, 2011) highlights that acquiring and processing information consumes energy and time. Individuals have limited attention and information-processing capacity. Thus people make many decisions almost automatically using mental shortcuts – heuristics, mental models or rules of thumb – "which reduce the complex tasks of assessing probabilities and predicting values to simpler judgmental operations" (Tversky and Kahneman, 1974, 1124). The shortcuts are used to filter, categorise and interpret information.

A number of heuristics have been identified. One is availability: people tend to focus on readily available information in memory (Tversky and Kahneman, 1974) and place too much weight on information that stands out, is novel, or seems relevant, but is not necessarily more relevant or better quality (Hanna, Mullainathan, and Schwartzstein, 2012). Another is representativeness: people apply lessons from familiar situations to unfamiliar situations that may, or may not, resemble them (Kahneman and Frederick, 2002). Yet another is present-biasedness: people often overemphasise the present when weighing long-term and uncertain benefits against immediate desires (Laibson, 1997; Kahneman, Knetsch, and Thaler, 1991; Samuelson and Zeckhauser, 1988), and may struggle to consider the welfare of their future

selves at all (Hershfield et al., 2011). Heuristics or mental models are a key concept in one theory of identity I later discuss, namely "frame-dependent" identity (Hoff and Pandey, 2014).

More recent work examines the role of affective states, emotions, moods and feelings, drawing more on developments in social psychology (Zajonc, 1984; Epstein et al., 1992; Sloman, 1996; Wilson, Lindsey, and Schooler, 2000). Rather than objectively weighing alternatives, people make subjective assessments of situations and are moved more by feelings, often automatically generated, than by evaluations of situations against a thoroughly thought-through ranking (Loewenstein et al., 2001; Finucane et al., 2000). Indeed, many biases which were earlier viewed in cognitive terms may actually reflect emotional factors, such as nonlinear probability weighting (Loewenstein et al., 2001) or hyperbolic time discounting (Loewenstein, 1996).

B.4 Context-dependent preferences and beliefs

Kahnemann and Tversky (1979; 1981) also developed prospect theory, one of the earliest behavioural economics models, which assumes that utility is not absolute, but is dependent on one's reference point. Judgements of value are often relative: people anchor their evaluations of expected utility relative to a reference point (often the status quo). Prospect theory accounts for this in terms of framing effects. In lab experiments, people are highly susceptible to how decisions are framed (Ariely, Loewenstein, and Prelec, 2003). Indeed, it is hard to view preferences as known, or even as known but perceived and reported with error: they are "commonly construed in the process of elicitation" and are thus highly context-dependent (Tversky and Thaler, 1990). Prospect theory also posits anchoring effects, when people form different preferences or valuations of alternatives depending on the initial reference points they are given. Where the standard model assumes people's preferences are context-independent and stable, prospect theory shows they are context-dependent and often highly unstable. One theory of aspirations as reference points (Dalton, Ghosal, and Mani, 2015; Genicot and Ray, 2014), described in later chapters, draws on ideas of reference points in prospect theory.

Prospect theory also explains why people are usually also loss-averse and make different choices when faced with the same decision framed as a gain or a loss compared to the status quo (Slovic and Lichtenstein, 1968; Lichtenstein and Slovic, 1973). People are also biased in favour of the status quo (Kahneman, Knetsch, and Thaler, 1991; Samuelson and Zeckhauser, 1988). As a result, people's choices are heavily influenced by which choice is framed as the

default or standard option (Johnson and Goldstein, 2003). They adapt to a wide range of experiences, so that the objective characteristics of an experience and reactions to it are very different (Frederick and Loewenstein, 1999).

B.5 Agents satisfice

Although many behavioural economists argue that they work within standard models, some argue that they take a more disruptive approach, arguing that people satisfice, rather than optimise. Given that decision-making has costs, people decide how good an alternative they should find and stop searching when they find it, even if they could find a better option (Simon, 1955).

There is debate on whether decisions made in this way are optimal. Some psychologists argue that such "fast and frugal" decision-making is rational and produces optimal results (implying that standard neoclassical behaviour is irrational), once we account for the costs of processing information and the constraints imposed by our limited cognitive processing ability (Gigerenzer and Selten, 2002; Gigerenzer and Goldstein, 1996).

But most argue that, although people's information-processing capacities are "good enough" to make sense of the world and to make decent decisions, they are far from optimal (Fiske, 2003). Such behavioural economists tend to agree with the cognitive psychologists that, "In general, these heuristics are quite useful, but sometimes they lead to severe and systematic error" (Tversky and Kahneman, 1974, 1124).

B.6 Self-interest

Another key departure from standard models is that behavioural economists try to model people as having "other-regarding" or "pro-social" preferences (Fischbacher, Gächter, and Fehr, 2001). People often take altruistic actions (Andreoni, 1990, 1995). People value and adhere to norms of trust and fairness (Rabin, 1993; Fehr, Kirchsteiger, and Riedl, 1993; Bohnet et al., 2008) and incur personal losses to impose them on others (Fehr and Gächter, 2000). People reciprocate both unkind and kind behaviour (Rabin, 1993). And people value social approval and adherence to social norms (Gintis et al., 2005; Henrich et al., 2004). Some behavioural economists agree with the perspective that preferences are not self-interested by definition:

> Because most of these models work by allowing agents to derive utility from the pleasure (or pain) of others...they are strictly speaking consistent with neoclassical economics (which...makes no assumption about the arguments of the utility function). In this sense, then, there is not necessarily anything characteristically behavioural about models of other-regarding preference (Angner and Loewenstein, 2012, 665).

In Sen's (1977) terms, these agents are neither narrowly self-interested nor committed – they are sympathetic, in the sense that they are acting in their own self-interest, but their self-interest is affected by what happens to others.

III. Conclusion

After a long hiatus, neoclassical economists have become more tolerant of concepts which posit the existence of unobserved mental states and widely use concepts of belief and preference. In this chapter, I have reviewed new areas of research which apply insights and empirical evidence from psychology in economics. Economists in these two areas of research take two relatively different approaches to integrating psychological concepts into their disciplines.

Economists working on non-cognitive skills take one path. They draw on cognitive developmental psychology and personality psychology, which suggest that non-cognitive skills and personality are relatively stable across contexts and in adulthood. Thus, both executive functions and personality can be defined as skills, part of an agent's endowment of ability. Because these concepts are argued to be stable, it is possible to keep assumptions about beliefs, preferences, and skills broadly intact while adding dimensions to how these concepts are measured and conceptualised.

The second path is taken by behavioural economics, drawing largely on Behavioural Decision Research (BDR) within cognitive psychology, but also increasingly drawing on aspects of social psychology to cover the role of emotion in decisions. Behavioural economists argue they are working within the neoclassical approach, based on the ideas of equilibrium, efficiency and utility maximisation, and modifying one or two assumptions about beliefs or preferences, often those which are not core:

> [T]here is nothing in core neoclassical theory that specifies that people should not care about fairness, that they should weight risky outcomes in a linear fashion,

> or that they must discount the future exponentially at a constant rate...Other assumptions simply acknowledge human limits on computational power, willpower, and self-interest (Camerer and Loewenstein, 2003, 4).

But they do suggest some material modifications to those assumptions that specify the characteristics of people's beliefs and preferences. As we saw, people often decide automatically rather than deliberatively; satisfice rather than optimise; they can be altruistic as well as self-interested; and beliefs, preferences and even some skills are not fixed but "frame-dependent" (Hoff and Pandey, 2014, 120).

Relatedly, behavioural economists aim to describe agents' processes of deliberation and decision-making rather than make simplifying assumptions about them. They demonstrate that the assumptions are often incorrect and seek to uncover empirical regularities in how preferences and beliefs behave. Most work in behavioural economics includes at least some of these modifications.

In the ensuing four empirical chapters, I apply ideas of self-belief, aspirations and identity in qualitative and quantitative data. I compare different theoretical perspectives on these concepts in psychology and examine economic models which attempt to incorporate these psychological concepts into economics. Throughout, I discuss which of these two paths is the most appropriate approach for using concepts of self-belief, aspirations and identity in economics.

4

Altering locus of control and aspirations through exposure to role models

I. Introduction

I argued in the Introduction that people may underinvest in high-return opportunities and in Chapter 2 that people may over-invest in sending children to school. In collaboration with psychologists, economists are interrogating new explanations for such behaviour and reassessing the simple "rational" view of human decision-making.

In this chapter, I examine the role played by people's beliefs about themselves and their capabilities in future-oriented investment decisions. I examine whether it is possible to alter three measures of people's self-belief – locus of control, self-esteem and aspirations – using an experimental design. The first two measures have been widely used by economists: locus of control differentiates individuals who think outcomes are contingent on individual behaviour from individuals who are fatalistic or believe that powerful social forces or individuals shape their lives in ways beyond their control (Levenson, 1981). Self-esteem is an individual's evaluation of their own worth or goodness (Rosenberg, 1965). I define aspirations as forward-looking goals or targets (Locke and Latham, 1990).

Measures of locus of control, self-esteem and aspirations have been used by different sub-fields of economics, as outlined in Chapter 3. Human capital theorists consider self-esteem and locus of control as examples of personality traits or "non-cognitive skills", part of the endowment of capabilities that affect an individual's productivity, life trajectory and propensity to make long-term investments (Heckman, Stixrud, and Urzua, 2006; Heckman and Kautz, 2012, 2013; Borghans et al., 2011; Lindqvist and Vestman, 2011; Almlund et al., 2011). I

use standard measures of these concepts commonly used by other economists. Behavioural economists have considered aspirations as one type of reference point which frames decisions, rather than as beliefs or preferences (Selten, 1998; Dalton, Ghosal, and Mani, 2015). I draw on theory about self-beliefs and aspirations from social learning theory in social psychology (Bandura, 1977, 1997), which I discuss in Section II. This perspective has not been widely used in economics (but see Beaman et al. (2012) and Krishnan and Krutikova (2013)).

With a team of co-authors, I conducted an experiment in a poor, remote part of rural Ethiopia which showed short documentaries about people from similar backgrounds to the audience. The documentary subjects improved their socio-economic position from being poor or of average socio-economic position to being relatively successful through their efforts in agriculture (for example, saving to purchase an irrigation pump) or in small business (for example, starting a business selling flour).

The documentaries are motivational narrations of the process through which people from poor backgrounds set, work towards, and achieve goals and improve their economic circumstances. The documentaries were designed to provide a "vicarious experience" of another person's life story which might cause audience members to re-evaluate their beliefs about their own capabilities to change their outcomes through their own actions. They emphasise the importance of working hard and persisting despite obstacles. They praise role models for their determination and strength. They highlight that progress takes time and that success is incremental. They also showcase stories of people who succeeded using commonly available resources and in widely prevalent activities, suggesting that the audience could also succeed in this way. The documentaries did not provide specific relevant information on untapped opportunities, costs or returns. All the economic activities depicted were commonly undertaken in the target area. The specific information was unlikely to be new for viewers, although the documentaries may have made existing information more salient. Because of the experimental design, there are unlikely to be any other changes in opportunities or constraints specifically among the treatment group.

This chapter reports on the effects of the intervention on psychological outcomes. The intervention increased the extent to which individuals had an internal locus of control and believed that they, rather than chance or fate, controlled their outcomes. Watching the documentaries also increased parents' aspirations and expectations for children's education six months after the screening, by between 0.18 and 0.33 years of schooling, which is robust

to multiple testing corrections, and increased indices of aspirations and expectations by 0.7 and 0.1 standard deviations respectively, although only the latter result is robust to multiple testing corrections. There were some small but significant effects on self-esteem and some measures of risk preference (the latter are not robust to multiple testing). There were also some effects on individuals' beliefs about what income and education other individuals in the person's village could achieve, a measure which I argue captures both objective beliefs about opportunities and constraints and participants' optimism or pessimism about available opportunities. These are largely not robust to multiple testing correction. In the next chapter, I report on effects on future-oriented behaviour, in particular spending on children's education and enrolment of the children of treated individuals, as well as savings and use of credit. Evaluations of exposure to role models have not to date focused on economic outcomes.

In a traditional economic model, exposure to these videos, or any other exhortations to work hard, set goals, plan and persist despite sacrifices, would be unlikely to change beliefs, preferences or behaviour. Non-cognitive skills are argued to be relatively stable and fixed over time, and would not respond to such a light-touch intervention. As I will outline, aspirations as conceived of by some behavioural economists change only in response to changes in past performance, not in response to others' experiences. Nonetheless, there are small but significant changes in behaviour among people who saw the documentary.

Given that there are changes in self-beliefs and their expression in goals and aspirations, and in beliefs about available opportunities, and no other changes in constraints or opportunities, I argue that there is suggestive evidence that changes in self-beliefs and aspirations, as conceived of in social learning theory, cause changes in behaviour. The experiment suggests that adding self-beliefs and aspirations "to rational models of decision-making increases their explanatory and predictive power" (Bandura and Locke, 2003, 92).

Section II reviews the literature on which the intervention design builds while Section III describes the intervention content. Section IV describes the experimental design, while Section V describes the psychological measures used. Section VI presents the empirical strategy and results. Section VII discusses the implications of these findings for economic theory: do they imply modifications to the standard model's assumptions about beliefs, preferences and skills?

II. Literature review

A. Self-beliefs in social learning theory

In the 1970s, psychologists were puzzled by findings that beliefs about the probability of various outcomes and subjective evaluations of possible outcomes (similar to notions of preferences), were rarely correlated with actual behaviour or performance (Chapman, Skinner, and Baltes, 1990; Danehower, 1988). They are, however, often correlated with measures of attitudes towards behaviours (Albarracín et al., 2001; Armitage and Conner, 2001). Social learning theorists argue that people's beliefs about their own capabilities and effectiveness also affect their choices and should be considered as an additional variable affecting choices (Bandura, 1997; Lent, Brown, and Hackett, 1994).

Social learning theorists argue that people have an underlying self-concept: a "basic, fundamental appraisal of one's worthiness, effectiveness and capabilities as a person" (Judge et al., 2003, 304). A core element of self-concept is self-efficacy, people's beliefs about their "capabilities to exercise control over their own level of functioning and over events that affect their lives" (Bandura, 1990, 257). When choosing between courses of action, people apply beliefs about their capabilities to perform specific actions or tasks, informed by their underlying self-beliefs (Schunk, 1991, 207). People may have different conceptions of their worth and ability in broad domains of life: they may not feel effective at work but feel effective in their personal relationships (Bandura, 2001). But these underlying appraisals of themselves are argued to be relatively stable, or context-independent, across different daily situations.

Secondly, an individual's self-beliefs can change over time. Concrete past successes and failures can alter self-beliefs. But, unlike skills, which are learned through direct experience and practice, people's self-beliefs are also influenced by observational learning (Bandura, 2002, 2004). People often learn through "vicarious experiences", from the experience of another person in a similar situation, rather than from direct experience. Laboratory work has shown that varying exposure to experiences alters self-beliefs and predicts performance of tasks, even if skill levels are not altered at all (Wood and Bandura, 1989; Bandura and Cervone, 1986; Bouffard-Bouchard, 1990; Boyer et al., 2000; Stajkovic and Luthans, 1998; Sadri and Robertson, 1993). Verbal assessments and feedback about one's ability from other people are also powerful determinants of self-beliefs (Schunk, 1981, 1983, 1987; Zeldin and Pajares, 2000).

Thirdly, self-beliefs are construals – subjective interpretations (Ross and Nisbett, 1991) – of a person's skills. People's assessment of their own capabilities and capacity to achieve certain

outcomes is not an accurate reflection of their actual effectiveness, nor of actual constraints (Bandura, Adams, and Beyer, 1977; Bandura et al., 1999; Schunk, 1984; Zimmerman, Bandura, and Martinez-Pons, 1992). There are often correlations between self-beliefs, cognitive skills, executive functions or ability on specific tasks: self-perceptions of capability "are critical determinants of how well knowledge and skill are acquired in the first place" (Pajares, 1997, 1), and both self-beliefs and skills determine performance and outcomes. However, people with strong abilities may well have low beliefs about their ability, or vice versa. Indeed, in experiments, the same performance attainment results in widely different revisions to self-beliefs and aspirations among different people (Bandura, Reese, and Adams, 1982). For example, failing makes some people less sure of their capabilities so they lower their goals; others stay confident and persist or raise their goals.

B. Aspirations in social learning theory

Social learning theory suggests that people's self-beliefs shape the choices and options they consider. People may exclude entire classes of options without going so far as to analyse costs and benefits, because they do not believe they are able to act to achieve them (Bandura, 1997; Ajzen and Madden, 1986). Self-beliefs thus "set the slate of options for consideration" (Bandura et al., 2001, 201). Within this "slate", or opportunity set, individuals select goals and aspirations.

Because people construe or interpret their past performance through the lens of their current self-beliefs, their revision of self-beliefs – and thus their aspirations – based on performance may not be objective (Bandura, Adams, and Beyer, 1977; Bandura et al., 1999; Schunk, 1984; Zimmerman, Bandura, and Martinez-Pons, 1992). "A given performance attainment can give rise to diverse self-set goals depending on level of perceived self-efficacy. How people behave is both mediated by their beliefs about their capabilities and can often be better predicted by these beliefs than by the results of their previous performances" (Bandura and Locke, 2003, 93-94). People's aspirations may exclude options from consideration that they are capable of achieving, or may include options they do not have the capabilities to attain.

Aspirations capture people's preferences among the set of options they believe to be attainable. They are "rooted in a value system and a sense of personal identity" and "set personal standards" (Bandura, 2001, 8). Although aspirations can be altered by new direct or

vicarious experiences, they tend to be relatively stable: they are forward-looking expressions of people's values and beliefs about what they could achieve. They need not be revised downward (or upward) in relation to every setback or success. Social learning theorists argue that,

> if actions were performed only on behalf of anticipated external rewards and punishments, people would behave like weather vanes, constantly shifting direction to conform to whatever influence happened to impinge upon them at the moment. In actuality, people display considerable self-direction in the face of competing influences after they adopt personal standards (Bandura, 2001, 7-8).

Yet another, less socially oriented, school of psychology has a more backward-oriented view of goal-setting, where "goal level may be a function of past level achieved rather than a representation of a desired state to which a person is striving" (Vancouver, Thompson, and Williams, 2001, 605).

C. Aspirations in behavioural economics

Much current theory in economics which examines aspirations does not draw on social psychology at all, but takes a behavioural economics perspective more in line with this backward-oriented view of goal setting. In prospect theory, discussed in Chapter 3, there is extensive empirical evidence of anchoring effects, when people form different preferences or valuations of alternatives depending on the initial reference points they are given (Kahneman, Knetsch, and Thaler, 1991; Samuelson and Zeckhauser, 1988).

Some behavioural economists draw on prospect theory and conceive of aspirations as reference points which frame decisions. People set aspirations and use them to regulate effort (Selten, 1998, 2008; Selten, Pittnauer, and Hohnisch, 2012). The aspirations are based on past experience of their performance, which acts as a reference point. People do not optimise, as agents in standard models would; they rather satisfice (Simon, 1955). For example, if they meet their aspiration for a particular period, they may be satisfied and stop exerting effort, even if they could achieve higher returns. If people fail once, they may revise their aspirations downward, even if objectively they could achieve the same or a higher goal.

Dalton, Ghosal, and Mani (2015) extend this model to argue that individuals are subject to a bias in how they process information: they underestimate how their aspirations evolve over their lifetime as a consequence of their effort. Poor individuals may continually fail to

meet their aspirations because they live in difficult conditions and continually lower their aspirations in ways which perpetuate poverty.

Others argue that the reference points are socially determined reference points: people form aspirations largely by observing the outcomes of other individuals similar to them in their immediate environment (Bogliacino and Ortoleva, 2011; Stark, 2006; Ray, 2006; Genicot and Ray, 2014). Aspirations may capture elements of collective beliefs or social conventions developed over generations (Bisin and Verdier, 2001). This situation can also lead to poor individuals who are surrounded by other poor individuals with very similar life trajectories forming low aspirations (Appadurai, 2004; Genicot and Ray, 2014).

D. Role model interventions

Narrative communication interventions in public health (Hinyard and Kreuter, 2007) and education entertainment in psychology (Singhal et al., 2004) often use videos depicting life stories of people similar to the audience. In health research, randomised trials compare videos of people similar to the audience who have changed behaviour to video messages from experts or neutral parties providing factual information. Those who watched narrative videos were more likely to quit smoking after two weeks (Williams et al., 2011), have lower hypertension after six months (Houston et al., 2011) and reduce behaviours which increase skin cancer risk (Lemal and Van den Bulck, 2010).

Deliberate educational messaging in soap operas alters multiple dimensions of attitudes and behaviour in field settings, including in gambling (Berg and Zia, 2013, South Africa), condom use (Vaughan and Rogers, 2000, Tanzania), adoption of agricultural practices (Heong et al., 2008, Vietnam), use of oral rehydration therapy (Abdulla, 2004, Egypt), and co-operative behaviours after conflict (Paluck, 2009, Rwanda). However, only the Rwanda and South Africa studies were experimental.

Non-experimental research in economics also examines the effect of exposure to role models. Exposure to soap operas which depict strong female role models with smaller families reduces fertility and increases divorce in Brazil (Chong and La Ferrara, 2009; Chong, Duryea, and La Ferrara, 2012), and reduces fertility and domestic violence and alters beliefs about women's autonomy in rural India (Jensen and Oster, 2009). "Negative" role models have a similar effect: exposure to a reality TV show about teen mothers reduces teen pregnancy in the United States (Kearney and Levine, 2014).

Psychologists suggest a number of theoretical mechanisms for role model effects, drawing on social learning theory about vicarious experiences and observational learning (Moyer-Gusé, 2008). Stories create a sense of identification between story-teller and viewer (Kreuter et al., 2008). A viewer imagines "being that character and replaces his or her personal identity and role as audience member with the identity and role of the character" (Cohen, 2001, 251); or, alternatively, may desire to emulate the character (Bandura, 1986). If people are "transported" or swept up into a resonant and engaging narrative, this in turn facilitates acceptance of the information provided (Green and Brock, 2000; Slater and Rouner, 2002). Indeed, randomised trials find viewers who watch life stories are more engaged, more likely to identify with characters, experience stronger emotions, and are more likely to have improved knowledge and changed attitudes (McQueen et al., 2011; Murphy et al., 2014; Williams et al., 2011).

Role model effects are also powerful in real life. When some Indian villages were randomly assigned to reserve a seat on the village council for a woman, girls in these villages had female role models in leadership positions (Beaman et al., 2012). The gender gap in girls' and parents' occupational aspirations declined, the gender gap in adolescent educational attainment disappeared and girls spent less time on household chores. Even after the policy ended, women in these villages were twice as likely to stand for and win positions (Beaman et al., 2009). There is suggestive evidence that the role model effect was important. However, female leadership may have affected outcomes through public good provision or other policies that changed opportunities and constraints for girls or women. By using an experimental design, I offer cleaner identification of a link between exposure to the documentary and changes in psychological outcomes and behaviour, because opportunities and constraints are unlikely to change only for the treatment group within villages. Through measurement of psychological outcomes, I suggest mechanisms behind behaviour change. My design also includes a placebo group, who watched a popular Ethiopian entertainment show of short comedy sketches. I can thus test if impacts are just based on exposure to TV in a remote area, rather than on documentary content.

Relatedly, an extensive literature in psychology documents that exposure to role models alters self-efficacy, aspirations and behaviour, particularly for groups who have few role models. Girls may be less likely to aspire to become scientists as there are few female scientists and they believe they cannot fulfil these roles (Gardiner et al., 2007; Nauta, Epperson, and Kahn, 1998; Richma, van Dellen, and Wood, 2011). Female science students randomly shown

biographies of female engineers or exposed to female instructors had higher self-efficacy in science subjects, and identification with and motivation to pursue science careers (measured both explicitly and through explicit association tests), compared to students exposed to male instructors. The students also attempted more problems on computerised tests and were more confident in assessments of their abilities, although final grades were not affected (Stout et al., 2011). Girls who were shown photographs or videos of female role models in traditionally male occupations are more likely to rate jobs that are not traditionally filled by women as suitable for both men and women (Greene, Sullivan, and Beyard-Tyler, 1982). The experiment I conducted brings the experimental approach into the field with adults in a developing country setting, and resurveys individuals after six months to examine the short-term persistence of changes in psychological outcomes.

Finally, light-touch interventions which provide directive advice or specific relevant information on the returns to schooling in general or on specific work opportunities also affect behaviour, as described in Chapter 2. Social learning theorists emphasise that it is very difficult to isolate whether exposure to a role model changes expectations about the likelihood of various outcomes, changes self-beliefs by improving an individual's confidence in their ability to enact the behaviour, or changes both (Bandura, 2002, 2004). Even if individuals already have relevant information, the role model's experience may make it more salient.

Some interventions, like that of Jensen (2010), provide only a script with narrow, specific, quantitative information and probably act simply by addressing information asymmetries. Others present information through relevant role models similar to those used in this experiment to make messaging more salient, and often include inspiration and encouragement (Dinkelman and Martínez, 2014; Ravallion et al., 2013). This intervention takes a third path and provides as little specific relevant information as possible. As in Chong, Duryea, and La Ferrara (2012), Chong and La Ferrara (2009) and Jensen and Oster (2009), any changes are more likely to be linked to a "vicarious experience" (Bandura, 1977) of how a similar individual to the audience managed to improve their life through hard work and persistence, rather than linked to the updating of beliefs about the probability of various outcomes due to new information. However, despite attempts to minimise the concrete information provided, it is not possible to argue that individuals derive no new information from the videos.

III. Treatment and experimental design

A. *Intervention content*

To make the documentaries used in this experiment, development agents and NGO staff in rural areas were invited to submit descriptions of the life stories of ordinary individuals who had improved their socio-economic well-being despite adverse initial conditions. Ten individuals were selected to have short documentaries made about their lives by Next Studios, an Ethiopian production company. Four were selected for the experiment, two about men and two about women. Each documentary is 15 minutes long and in Oromiffa, the local language in the study site.[1] Three documentary subjects were from other districts in the region and one was from a neighbouring region. It was thus almost impossible that respondents would know anyone in the videos and there is no evidence that this happened.

The documentaries have common themes. Firstly, all the subjects are ordinary rural residents who were either poorer than those around them or of similar socio-economic status, so the audience can relate to them. The psychology literature highlights that role models should be of similar backgrounds to audience members. Similarly, the economic literature on aspirations highlights that people may form unrealistically high aspirations if they lack role models who come from similar backgrounds to them (Bogliacino and Ortoleva, 2011; Stark, 2006; Ray, 2006; Genicot and Ray, 2014). When those who saw the documentary were asked at endline about the story they found the most relevant to them, 52 per cent of audience members thought the documentary subjects had initially been worse off than they currently were (11 per cent said the subject was initially the same as them, while 36 per cent said the subject was better off than them). However, 73 per cent of the audience said that by the end of the documentary the documentary subjects were better off than they were currently (21 per cent said the subject was worse off and 5 per cent said the subject was the same as them). The subjects of the documentaries start from a similar position to the audience but achieve a better life through hard work and perseverance.

Secondly, all the subjects take slightly different courses of action to those around them: starting a small business, diversifying their source of income, improving their farming practices, or acting outside cultural norms by adopting non-traditional divisions of household

[1]The documentaries, with English subtitles, and one of four placebo segments are at https://www.youtube.com/channel/UCqfoNjCzt8YPjTRWQaMQfAg. The Appendix summarises two documentaries and one placebo segment.

responsibility between spouses. They all undertook activities that were relatively normal for the villages where documentaries were screened, such as installing irrigation, or starting a shop. No specific information on costs or returns was provided. The life stories were not specifically about particular actions that ought to be followed and did not promote taking specific actions.

Thirdly, the documentaries are filmed in a motivational and inspirational style and describe the emotional and mental processes of setting, working towards and achieving a goal. The role models emphasise the importance of hard work. For example, Teyiba Abdella, whose story is summarised in Appendix A, describes the start of her flour-trading business by saying "In those days I used to work like a donkey...carrying heavy loads on my back." She says "The point I would like to make is about the life-transforming value of hard work. All of us should co-operatively and persistently work hard towards a better life". Beshir Malim Yisak, whose story is also summarised in Appendix A, is also physically relieved of the burden of carrying water to irrigate his fields when he saves enough to purchase a water pump. A village elder says of him, "I advise all youngsters to follow Beshir's example and to be persevering hard workers like him."

They describe how they set goals, received support and were encouraged by mentors and family members. They also built relationships with people people outside their families and network, such as the miller who provides Teyiba with increasing amounts of flour on credit because she repays it on time. The role models emphasise that progress is slow and they had to persevere through setbacks. Beshir describes how he made detailed plans over three years to save for and purchase his water pump. Both he and Teyiba describe future goals they are working towards, such as building a house or moving to the nearby town.

Spouses, mentors and narrators highlight the personal qualities of the subjects, such as determination, reliability and kindness. Teyiba's husband says, "She is such a kind woman, by far stronger than me...she is everything to me and a role model to her fellow villagers". A village agricultural extension officer says Beshir is a "fast learner" and has "innovative ideas".

Finally, individuals succeed largely through their own efforts and, in some cases, by drawing on assistance from community members and resources available in all villages, such as agricultural extension agents. Beshir is proud of being "independent". They do not receive interventions from government or NGOs. For example, although Beshir asks for the help of

the local extension agent in purchasing a pump from Addis Ababa, he saves and pays for it himself.

B. Study site

The field experiment took place in Doba, an administrative district in Oromia region, 380 kilometres east of Addis Ababa (Ethiopia's capital city). The documentaries feature rural inhabitants in grain-growing areas. Doba is also rural and grain-growing: 1.5 per cent of Doba's population live in urban areas and 89.5 per cent are subsistence farmers growing sorghum and maize (Central Statistical Agency of Ethiopia, 2007). Doba was one of the first districts selected in 2005 for the Productive Safety Net Program (PSNP), which is targeted at the most chronically food-insecure districts in Ethiopia.

Doba is remote: most surveyed villages were accessible only by 4x4 vehicle and some required camel transportation. There is limited exposure to television: at baseline only 10 per cent of respondents watched TV once a week or more, 29 per cent watched at least once a month and 61 per cent watched about once a year or never. The intervention tests for the existence of a psychological mechanism, so the site was chosen to provide a setting where even a relatively light-touch and inexpensive intervention might have psychological effects. Doba residents would be likely to find a television show memorable and to pay attention to the screening content. The point estimates of the effect of the intervention may not be replicated in less remote areas where participants have more exposure to media, and I do not argue for their external validity. In less remote areas, a more intense intervention might be needed.

The sample of 64 villages was randomly selected from Central Statistical Agency's list of rural villages of between 50 to 100 households in the district. The baseline survey and screening took place between September and November 2010. One team of 35 enumerators went from village to village to ensure homogeneity in the screenings.

C. Experimental design

In each village, the enumerators compiled a list of all households and randomly selected 18 households. In each household, the household head completed a baseline household interview (on issues like household composition, assets and children's schooling) and a baseline individual interview (on psychological variables) at their home. Their spouse, if they

had a spouse, also completed a baseline individual interview at their home. Enumerators took great care to ensure respondent privacy. Spouses were always interviewed separately, usually by interviewing them at the same time (one inside and one outside near the house) or by scheduling appointments so one spouse was away (for example, while husbands were in the field). The endline interviews were conducted using exactly the same protocol.

In each village, six households were randomly assigned to the treatment group and received an invitation to watch a screening of the four 15-minute documentaries. Another six households, the control group, were surveyed but did not receive invitations. To capture the effect of being invited to a screening, the experiment includes a placebo design (like other studies on exposure to information (Card, Kluve, and Weber, 2010; Berg and Zia, 2013)). Another six households received an invitation to watch four 15-minute segments of an Ethiopian comedy TV show about rural life.

At the end of their individual baseline interview, the household head and spouse in treatment and placebo households received tickets for a screening session in a few days' time. Randomisation is at household level, so household heads and their spouses have the same treatment status. Respondents were told that the screening was an entertainment show, tickets were non-transferable, each respondent would receive a bag of sugar after the screening, and they could only attend the screening at the time and place on the ticket.

The 64 villages were grouped into 16 screening sites with four neighbouring villages in each site. Farmers' training centres or schools are often in between villages to ensure maximum accessibility. Screenings were conducted at one central school or farmers' training centre. On average, people walked 29 minutes travel time to the screening venue, so people not invited to the screenings were unlikely to walk to the centre.

Enumerators conducted the baseline in all four villages and then conducted the treatment and placebo screenings on one day at different times. The screenings used a projector and speakers connected to a power generator. Tickets had the name and survey identifier of each respondent on them. Ticketing was tightly controlled at the screening to ensure compliance with treatment assignment and to ensure people who were not selected for treatment were not treated.

The design also generates exogenous variation in the extent to which an individual's network was exposed to the treatment to explore the role social interactions play in changes in psychological variables and behaviour in response to the treatment. In each screening site of

four villages, two villages were randomly assigned to be "intense treatment" villages, where 18 additional households (roughly 36 individuals, or 1,152 individuals in total) were randomly selected to be invited to the documentary but were not surveyed. The other two villages were "intense placebo" villages, where 18 additional households were randomly selected to be invited to the placebo session. The results of the peer-level treatment are presented in the next chapter.

D. Compliance and attrition

If all sampled households had a head and a spouse, there would have been 769 individuals in the treatment group, 768 in the placebo group and 767 in the control group (the first row of Table 4.1). However, 95 individuals were single, widowed or divorced, so the household was given only one ticket. A further 94 individuals were not interviewed in the baseline or given tickets because they were away, ill or had just given birth. The remaining individuals (in the row "Given Tickets") were interviewed and given tickets. Compliance among those allocated tickets is high: 94 per cent of surveyed individuals and 92 per cent of non-surveyed individuals invited to screenings were recorded arriving at the correct screening.[2] There are no significant differences in compliance between groups.

The main results use data from baseline and follow-up interviews six months after the screening. Attrition between baseline and follow-up is very low: only 48 individuals (2.3 per cent of the sample) were lost to the second round. There are no significant differences in attrition rates between groups. The rest of the analysis examines the 2,063 individuals who appeared in both rounds (see first column of Table 4.2).

Straight after the screening, in the screening venue, a large team of enumerators interviewed both household members to measure aspirations and expectations. Household heads and spouses were interviewed separately, by different enumerators, often at the same time but in different parts of the venue to ensure privacy. Households were interviewed straight away at the screening site to avoid spillover effects from discussion between respondents.

On the screening day, some enumerators interviewed control households at their home to capture follow-up measures of aspirations and expectations from control households and prevent control households from attending the screening. Control households also received a

[2]An individual is coded as compliant if enumerators recorded the time that they arrived at the correct screening. For 151 individuals, the time of arrival at the screening venue was not recorded, but the individuals were also not marked as absent. If these individuals are included, compliance rates rise to 96 and 97 per cent for surveyed and non-surveyed households. Only 20 respondents attended the incorrect screening.

Table 4.1: Compliance and attrition among sampled individuals

	Surveyed				Not surveyed[1]		
	Treated	Placebo	Control	Total	Treated	Placebo	Total
Total sample	769	768	767	2,304	1,154	1,151	2,305
One spouse dead/divorced	38	27	30	95	56	62	118
Not given ticket (sick or away)	40	24	30	94	8	3	11
Given tickets	691	717	707	2,115	1,090	1,086	2,176
Attrition							
Missed Round 2	15	15	19	48			
Missing questionnnaire	1	0	3	4			
Attrition rate[2]	0.023	0.021	0.031	0.025			
Sample[3]	675	702	685	2,063	1,090	1,086	2,176
Missed follow-up interview	43	37	24	104			
Attrition rate[4]	0.064	0.053	0.035	0.050			
Sample with 3 measures	632	665	662	1,988			
Non-compliers							
Missed screening	14	8	0	24	43	28	71
Attended incorrect screening	3	11	6	20	0	0	0
Compliers							
Not recorded at correct screening[5]	10	9	25	44	45	62	107
Recorded at correct screening[6]	648	674	654	1976	1,002	996	1,998
Compliance rate[7]	0.938	0.940	0.925	0.934	0.919	0.917	0.918

[1] These columns describe the additional households who received invitations to screenings but were not surveyed. We recorded which invitation they received and whether they arrived at the designated screening session.
[2] The proportion of those surveyed in Round 1 and given tickets who were not found in the subsequent round.
[3] The sample in this paper consists of individuals given tickets minus those who could not be found in Round 2 and those missing a questionnaire. All individuals in the sample have a baseline and endline questionnaire.
[4] The proportion of those surveyed in Round 1 and Round 2 who were not surveyed straight after screening.
[5] Some individuals were not recorded as arriving at the correct screening, but were also not recorded as absent or attending the wrong screening.
[6] For treatment and placebo groups, these are individuals who were recorded arriving at the correct screening. For control groups, these are individuals who completed a follow-up interview during the screenings for their village.
[7] The compliance rate is the proportion of those given tickets who were recorded arriving at the correct screening.

Table 4.2: Sample of individuals surveyed and given tickets

		All villages	Intense treatment vill.	Intense placebo vill.
		64	32	32
Number surveyed		2,063	1,033	1,029
	of which:			
	Treatment individuals	675	337	338
	Placebo individuals	702	354	348
	Control individuals	685	343	342

This table reports on the sample of individuals (n=2,063) used in all subsequent analysis, which excludes 52 individuals who were not surveyed in Round 2 or were missing a questionnaire.

bag of sugar at this follow-up interview. This interview method was chosen so that control households never experienced the effect of gathering in one place. However, comparisons between the control group and treatment and placebo groups on measures straight after screening are interpreted with caution as survey methods differ. In addition, of the 2,063 respondents surveyed in Round 1 and Round 2, only 1,959 respondents have measures at baseline, straight after screening, and endline.

E. Experimental integrity

Table 4.3 reports tests of balance for both treatment and placebo experiments on individual and household-level variables. There are no significant differences across groups in gender or age of the individuals, nor in the frequency with which they watch TV, listen to the radio, travel outside the district or have lived outside the district, variables which capture their exposure to the opportunities of individuals outside their village. The asset measure, answered by the household head, captured which of a list of durable goods – tools, furniture, electrical goods and carts or bicycles – the household owned and their estimated resale value. Land and houses were not included.

Each set of tests for balance reported below involves a large number of comparisons: for example, testing if nine demographic variables are balanced across the treatment, placebo and control groups results in 27 comparisons alone. I thus correct p-values for multiple testing using the Benjamini and Hochberg (1995) procedure. I run the procedure separately for all p-values where I compare treatment and control, all p-values where I compare placebo and control, and all p-values where I compare treatment and control.

Table 4.3 shows that there are some small imbalances between some groups without correcting for multiple testing in the proportion of people who are single, widowed or divorced, the highest grade attained and household assets. However, none of these imbalances

Table 4.3: Balance tests: demographic variables at baseline

	Mean	Treatment	Placebo	Control	T–C	P–C	T–P
Individual-level variables (n=2,063)							
Age	36.82	36.93	36.94	36.57	0.32	0.30	0.02
	(12.43)	(11.44)	(13.17)	(12.61)	(0.78)	(0.82)	(0.80)
Male	0.49	0.49	0.49	0.49	0.00	−0.01	0.01
	(0.50)	(0.50)	(0.50)	(0.50)	(0.01)	(0.01)	(0.01)
Highest grade	1.34	1.49	1.29	1.24	0.25**	0.06	0.19
	(2.31)	(2.43)	(2.28)	(2.23)	(0.13)	(0.13)	(0.13)
Single	0.06	0.07	0.05	0.05	0.03**	0.00	0.02*
	(0.23)	(0.26)	(0.22)	(0.21)	(0.01)	(0.01)	(0.01)
Watches TV	0.10	0.11	0.09	0.11	0.00	−0.01	0.01
(> monthly)	(0.3)	(0.31)	(0.29)	(0.31)	(0.02)	(0.02)	(0.02)
Listens to radio	0.61	0.62	0.59	0.63	−0.01	−0.04	0.03
(> monthly)	(0.49)	(0.49)	(0.49)	(0.48)	(0.03)	(0.03)	(0.03)
Travels outside district	0.14	0.15	0.12	0.14	0.01	−0.02	0.03
(> monthly)	(0.34)	(0.36)	(0.33)	(0.35)	(0.02)	(0.02)	(0.02)
Lived outside district	0.1	0.11	0.08	0.10	0.01	−0.02	0.03*
	(0.30)	(0.31)	(0.27)	(0.30)	(0.02)	(0.02)	(0.02)
Household-level variables (n=1,138)							
Assets (ETB)	6,567.63	7,292.46	5,816.16	6,596.24	701.07	−763.27	1,464.34***
	(8,519.83)	(10,451.07)	(6,764.19)	(7,883.1)	(603.94)	(603.37)	(603.52)

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors (clustered at household level for individual outcomes) are in parentheses. The T–C difference and P–C difference are the treatment and placebo dummies run on the baseline data. The T–P difference is the difference between the treatment and placebo dummies. If p-values are corrected for multiple testing using the Benjamini and Hochberg (1995) procedure, no imbalances are statistically significant.

are significant once I control for multiple testing. In addition, Appendix C shows that results are robust to controls for marital status, assets and education, which are imbalanced at baseline, as well as standard controls like gender and age.

IV. Psychological concepts and measurement

A. Self-concept: Self-efficacy, locus of control and self-esteem

Many psychologists argue "core self-evaluation" (Judge et al., 1998, 2002b,a) is a core underlying concept and that many different measures of aspects of this concept – locus of control, self-esteem, self-efficacy, and aspects of neuroticism and emotional stability (captured in the Big Five personality measure (John and Srivastava, 1999)) – are simply indicators of core self-evaluation. Indeed, these measures demonstrate high convergent validity, poor discriminant validity, and poor incremental predictive validity in a number of samples – they appear to measure the same underlying concept (Judge and Bono, 2001; Judge et al., 2002a,b). Other work has focussed on self-concept and self-efficacy measures in particular and also suggests

"the traditional distinction between self-concept and self-efficacy may have been overstated in the literature" (Bong and Skaalvik, 2003, 8) (see also Bandalos, Yates, and Thorndike-Christ (1995)). Others argue that the concepts are distinct but likely to be very highly correlated. Self-perceptions of competence are components of an individual's self-concept, so self-efficacy may be one of the judgements necessary to form self-concept (Shavelson and Bolus, 1982) along with judgements of confidence, self-esteem and stability (Rosenberg and Kapland, 1982).

Of course, some psychologists disagree and argue that these concepts are distinct (Gardner and Pierce, 2009; Johnson, Rosen, and Levy, 2008; Chen, 2012). This experiment is not designed to draw fine distinctions between these concepts: the experiment focuses on whether these closely correlated indicators can be altered through exposure to role models and whether exposure to role models results in any behavioural changes. I therefore select measures which are most widely used and recognised in economics, self-esteem and locus of control.

A.1 Locus of control

I use the concept of locus of control, defined formally as "a generalised expectancy pertaining to the connection between personal characteristics and/or actions and experienced outcomes" (Lefcourt, 1991, 414). I use the Internal, Powerful Others and Chance (IPC) scale (Levenson, 1981). The scale builds on the measure by Rotter, Chance, and Phares (1972), which is widely used in economics (Heckman, Stixrud, and Urzua, 2006; Heckman and Kautz, 2012; Heineck and Anger, 2010; Cobb-Clark and Tan, 2011). Rotter's original scale categorises people as either internal or external (believing that outcomes are not determined by personal efforts) and assumes these dimensions are inversely related (Rotter, Chance, and Phares, 1972).

Levenson's scales capture three independent dimensions of the construct of control. The Internal scale captures if people see outcomes as contingent on their behaviour, with items such as "When I make plans, I am almost certain/guaranteed/sure to make them work" and "When I get what I want, it is usually because I worked hard for it". The Chance scale captures whether individuals think chance, luck or fate affects their outcomes, with items such as "It is not always wise for me to plan too far ahead because many things turn out to be a matter of good or bad fortune". The Powerful Others scale examines beliefs about whether other people control events in their lives: items include "I feel like what happens in my life is mostly determined by powerful people". This allows, for example, that some people believe in the

Table 4.4: Balance tests: psychological outcomes at baseline

	Mean	Treatment	Placebo	Control	T–C	P–C	T–P	N
Locus of control								
Internal	15.54	15.6	15.47	15.56	0.02	−0.10	0.12	2,052
	(2.52)	(2.58)	(2.49)	(2.51)	(0.16)	(0.16)	(0.15)	
Others	11.82	11.89	11.83	11.74	0.16	0.07	0.09	2,045
	(2.91)	(2.90)	(2.98)	(2.83)	(0.15)	(0.15)	(0.15)	
Chance	12.37	12.42	12.41	12.28	0.15	0.11	0.04	2,030
	(2.73)	(2.63)	(2.81)	(2.75)	(0.16)	(0.17)	(0.16)	
Perceptions of causes of poverty								
Individual	11.19	11.24	11.09	11.24	0.01	−0.15	0.16	2,052
	(2.59)	(2.53)	(2.64)	(2.60)	(0.15)	(0.16)	(0.16)	
Structural	10.20	10.05	10.24	10.30	−0.25*	−0.08	−0.17	2,049
	(2.42)	(2.36)	(2.5)	(2.39)	(0.14)	(0.15)	(0.15)	
Fate	9.28	9.39	9.29	9.15	0.25	0.12	0.13	2,049
	(3.33)	(3.38)	(3.37)	(3.23)	(0.20)	(0.20)	(0.20)	
Self-esteem	16.92	17.05	16.85	16.86	0.22**	0.01	0.21*	2,043
	(1.97)	(1.92)	(1.99)	(2.01)	(0.11)	(0.11)	(0.11)	
Life satisfaction								
Best life	4.24	4.33	4.10	4.29	0.04	−0.16	0.2*	2,055
	(1.83)	(1.81)	(1.79)	(1.88)	(0.11)	(0.11)	(0.11)	
Happiest life	6.61	6.64	6.53	6.65	−0.05	−0.16	0.11	2,037
	(2.34)	(2.34)	(2.38)	(2.29)	(0.13)	(0.13)	(0.14)	

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors (clustered at household level for individual outcomes) are in parentheses. The T–C difference and P–C difference are the treatment and placebo dummies run on the baseline data. The T–P difference is the difference between the treatment and placebo dummies. If p-values are corrected for multiple testing using the Benjamini and Hochberg (1995) procedure, no imbalances are statistically significant.

potency of luck and the efficacy of effort and others might share beliefs about effort but not about luck. There is extensive empirical work showing this structure is preferable (Lefcourt, 1991, 416).

I used a selection of items from each of Levenson's three scales, omitting those which were not appropriate to the rural Ethiopian context. Responses could be from 1 on an item if respondents "Strongly disagree" to 4 if they "Strongly agree", with no neutral option. Scores on each scale range from 5 to 20. For example, higher values on the Internal scale indicate that respondents see outcomes as contingent on individual behaviour.

At baseline, on average, participants have higher scores on the internal locus of control (a mean of 15.54) than on the chance (12.37) or others (11.82) sub-scales. The Internal scores are almost identical to the American samples in Levenson (1981). However, the Chance scores are much higher (the mean was 61 per cent of the total possible score, compared to 37 per cent in the studies in Levenson (1981)), as were the Powerful Others scores (the mean was 59 per cent of the possible total, compared to 39 per cent in Levenson (1981)). Unsurprisingly, poor

people in an isolated, highly religious area with limited or no education are more likely to believe that fate or chance and other powerful people control their outcomes. Other samples in Africa behave similarly to our sample (Cheng et al., 2013; Rossier, Dahourou, and McCrae, 2005; Reimanis and Posen, 1980; Van Haaften and van de Vijver, 1999). The Cronbach's alpha for scales is reported in Appendix Table B.1 and is above or close to the threshold of 0.7 recommended by psychologists for the Internal and Powerful Others scales, although it is only 0.676 for the Chance scale, indicating that items are slightly less strongly correlated than they should be. There are no imbalances between groups on any of the measures, both with and without corrections for multiple testing.

A.2 Perceptions of the causes of poverty

From sociology and political science, I use the Attributions for Poverty scale (Feagin, 1972, 1975) to measure people's perceptions of the causes of poverty among people in general, rather than only in their own lives. I use a version adapted for China (Shek, 2003) (a shorter but less accurate version is included in the World Values Survey (Abramson and Inglehart, 1995)). The scale assesses the extent to which respondents agree with each of three types – Individualistic, Structural and Fatalistic – of explanations for poverty. An example of an Individualistic explanation is "They [poor people] lack the ability to manage money or other assets". A Fatalistic explanation would be "They lack luck". A Structural explanation is "They lack opportunities due to the fact that they come from poor families". These echo the groups used in the IPC scale and the measure behaves similarly to the locus of control measure, with people agreeing more with individual than other explanations for poverty.

I used the same four point scale as for locus of control.[3] Scores are relatively similar to those when the scale was administered in Nigeria (Ige and Nekhwevha, 2012) and South Africa (Nasser, Abouchedid, and Khashan, 2002). Ethiopians and other Africans are more likely to attribute poverty to structural or fatalistic explanations than people in Western countries (Hunt, 2004). The Cronbach's alpha is above 0.7, except for the Structural scale, when it is 0.626. Results on this scale should be treated with more caution.

[3]The item "They are born with less talent/they are less gifted" was dropped from the Fate scale because it was poorly translated and did not cluster closely with the other three items in factor analysis. Aggregate scores were multiplied by $\frac{4}{3}$ so the scale also has a maximum of 16. One item was also modified in the original Individual scale, which refers to welfare, to refer to food aid.

A.3 Self-esteem

I also examine a common measure of self-esteem, one's overall sense of worthiness as a person (Rosenberg, 1965). I used six of ten items from the Rosenberg (1965) scale, the most widely used measure: a recent test in 53 countries found the factor structure is similar across countries and it is uniformly related to the same personality traits in most countries, suggesting the measure has sound psychometric properties (Schmitt and Allik, 2005).

Here, I use the same four point Likert scale as for locus of control. Scores can range from 6 (strongly disagree with all six items) to 24 (strongly agree with all six items). Some items are negatively phrased and were reverse-scored, as shown in Appendix Table B.1. My sample has a mean of 16.92 out of a possible 24 (70 per cent of the maximum). The scale was previously administered in Ethiopia in English with a mean of 29.24 out of a maximum possible of 40 (73 per cent of the maximum) (Schmitt and Allik, 2005), so the measure behaves correctly. However, the Cronbach's alpha in this sample is only 0.572, indicating that results on this item should be treated with more caution. There is also an imbalance between the treatment and control group (at 5 per cent level) and the treatment and placebo group (at 10 per cent level) at baseline on this variable, but these are not significant once I correct for multiple testing.

A.4 Life satisfaction

Life evaluation or life satisfaction measures capture individuals' perspectives on their lives (emotional well-being measures, in contrast, capture emotions at a point in time) (Diener, Kahneman, and Schwarz, 2003; Kahneman and Deaton, 2010). The intervention might be expected to increase life satisfaction if it increases positive self-concept: locus of control is an important predictor of life satisfaction (Judge et al., 2002b,a) and individuals with an internal locus of control and positive self-esteem are more likely to set and pursue goals or aspirations, which makes them happier and more satisfied (Elliot, Sheldon, and Church, 1997). However, the relationship may also go in the opposite direction: people who set and achieve high goals may adapt to having achieved those goals, be less satisfied and set even higher goals. This argument is used to explain why people in industrialised countries do not become more satisfied with life over time, despite improvements in living standards (Easterlin, 2001).

To measure life satisfaction, I used measures from the European Social Survey (Helliwell et al., 2010), which build on a common measure of life satisfaction (Cantril, 1966). Respondents were shown a picture of a ladder with ten steps. They were told the top of the ladder represents

the best possible life for them and the bottom step represents the worst possible and were asked "Where on the ladder do you feel you personally stand at present?". The question was repeated with the top and bottom steps representing the happiest and most miserable possible life. Using both measures provides some insurance against poor translation of one of the concepts. In this sample, respondents score higher on the "happiest life" question than the "best life" question, as in other studies (Helliwell et al., 2010). There is one imbalance between treatment and placebo groups at the 10 per cent level, but this is not significant once I control for multiple testing.

B. Aspirations and expectations for oneself and beliefs about others' opportunities

Social psychologists define aspirations as forward-looking goals or targets (Locke and Latham, 2002; Fishbach and Ferguson, 2007). Planning for the future and setting goals are core features of human behaviour (Feather, 1982; Locke and Latham, 2002). Although future events are not real, the process of imagining and planning for future events makes them concrete in people's minds and guides how people act to shape and regulate the future.

Goal-setting theorists distinguish between distal goals or aspirations, which "set the general course of pursuits" (Bandura, 2001, 8), and proximate goals, which are specific, challenging and temporally proximate. Proximate goals provide effective guides for present action, but distal goals are not specific enough for this purpose (Bandura, 2001; Locke and Latham, 2002). Both distal and proximate goals provide motivation and regulation for current behaviours and create incentives to sustain effort. "Goals, rooted in a value system and a sense of personal identity, invest activities with meaning and purpose" and "set personal standards" (Bandura, 2001, 8). When achieved, they give people "self-satisfaction and a sense of pride and self-worth" (Bandura, 2001, 8).

Psychologists from the social learning theory tradition argue that more positive self-beliefs result in people setting higher aspirations. There are strong correlations between levels of goals and aspirations and self-efficacy (Bandura, 2001; Locke and Latham, 2002; Wood and Bandura, 1989), self-concept (Shah and Higgins, 2001; Elliot, Sheldon, and Church, 1997) and an internal locus of control (Levenson, 1974; Strickland, 1965). Higher aspirations can be defined as higher levels on one dimension, or as considering a broader range of options (Betz and Hackett, 1997; Lent, Brown, and Hackett, 1994). However, goals and aspirations also reflect individuals' preferences. Among options which people consider attainable according

to their self-beliefs and their beliefs about objective constraints and available resources, people aspire to achieve only some outcomes, those which they prefer sufficiently to other outcomes.

There is a rich qualitative literature on aspirations and expectations (see Camfield et al. (2012) for a review in developing countries) but few quantitative measures. Economists mainly use indirect measures of income and wealth aspirations. Stutzer (2003, 6) defines aspirations as "the income that is required to reach mean expected welfare", which he captures using an individual welfare function, which asks respondents what amount of income they would consider very bad, very good and gradations in between. Knight and Gunatilaka (2012) use a similar measure of minimum income need. These measures do not capture that people's aspirations may reflect personal preferences about one's living standards as well as objective assessments of what is considered necessary.

I measure respondents' aspirations, expectations and beliefs about others' opportunities in four dimensions. Annual income is the amount of cash income the household earns from all agricultural and non-agricultural activities in a year. Wealth focuses on durable wealth (including housing, vehicles, furniture and other valuable durables). To capture education aspirations and expectations, I asked about the years of schooling the respondent wants their eldest child to complete. I include codes for different types of post-school education, so completing a three-year university degree was 15 years of education. Social status was measured as the percentage of community members who would ask for the respondent's advice at times of important decisions.

Andres et al. (1999) distinguish between aspirations, desired or ideal ambitions and goals, and expectations, a more realistic assessment of an individual's beliefs about what they will achieve. Glewwe, Ross, and Wydick (2015) also distinguish between hopes and expectations. I thus asked two questions about goals on each dimension. For income, for example, respondents were asked "What is the level of income that you would like to achieve?" (to capture aspirations) and "What is the level of income that you think you will reach within ten years?" (to capture expectations). The measures have been tested before in Ethiopia and correlated with demographic variables as expected (Bernard and Taffesse, 2014).

Table 4.5: Balance tests: Expectations, aspirations and beliefs for wealth and income at baseline

	Mean	Treatment	Placebo	Control	T–C	P–C	T–P	N
Expectations								
Income	33,081.82	27,197.16	43,592.16	28,066.71	−585.29	16,000.13	−16,585.42	2,045
	(231,346.45)	(55,606.32)	(386,524.06)	(674,56.83)	(4,387.94)	(14,384.87)	(14,410.55)	
Wealth	28,073.07	27,547.24	29,295.88	27,339.65	394.10	2,230.79	−1,836.69	2,049
	(70,490.7)	(57,320.66)	(80,858.2)	(70,948.62)	(3,754.88)	(4,463.08)	(4,037.75)	
Aspirations								
Income	146,056.98	154,779.67	154,403.66	128,907.80	26,703.27	25,944.75	758.52	2,047
	(609,111.25)	(723,021)	(671,263.56)	(375,094.53)	(29,874.1)	(27,772.89)	(36,161.07)	
Wealth	152,577.14	59,837.65	205,533.14	189,945.66	−134,641.47	13,297.15	−147,938.62	2,049
	(2,841,719.5)	(161,201.12)	(3,789,935.25)	(3,097,617.75)	(121,416.45)	(181,454.27)	(141,856.33)	
Beliefs about the minimum others can attain								
Income	1,437.70	1,522.56	1,334.03	1,460.58	60.74	−108.98	169.72*	2,003
	(1,256.06)	(1,347.92)	(1,125.86)	(1,283.21)	(75.03)	(67.23)	(72.33)	
Wealth	1,097.20	1,086.47	1,103.05	1,101.81	4.67	20.29	−15.62	1,993
	(1,151.8)	(1,130.88)	(1,162.64)	(1,162.76)	(65.74)	(66.3)	(63.56)	
Beliefs about the maximum others can attain								
Income	15,941.40	17,180.66	15,273.10	15,408.28	1,792.79	−75.63	1,868.43	2,041
	(25,928.38)	(40,523.6)	(13,431.01)	(14,928.34)	(1,561.53)	(850.13)	(1,533.16)	
Wealth	21,829.43	21,033.45	23,722.75	20,679.50	567.72	3,116.30	−2,548.58	2,039
	(38,091.82)	(27,546.74)	(53,402.32)	(26,455.76)	(1,560.76)	(2,324.89)	(2,340.55)	

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors (clustered at household level for individual outcomes) are in parentheses. The T–C difference and P–C difference are the treatment and placebo dummies run on the baseline data. The T–P difference is the difference between the treatment and placebo dummies. If p-values are corrected for multiple testing using the Benjamini and Hochberg (1995) procedure, no imbalances are statistically significant.

Table 4.5 reports mean expectations and aspirations at baseline.[4] Current household income levels at baseline are 6,243 ETB (about 367 USD at exchange rates at the time of the survey, 1 ETB=17 USD). Households report durable wealth of around 7,420 ETB (436 USD). At baseline, respondents expect to do rather well in ten years' time, expecting on average 5.3 times their current income and 3.8 times their current wealth. The mean remains affected by outliers. Median income and wealth expectations for the bulk of respondents are relatively low. The median respondent has income and wealth of 4,000 ETB (237 USD) at baseline. They expect income of 12,000 ETB (706 USD) and wealth of 14,000 ETB (824 USD respectively) in ten years.

Aspirations are much higher. The mean for income aspirations is roughly 23 times the mean for current income (although, given the poverty level they start from, and with a household size on average of 5.81, this is still only in the order of per capita annual income of about 1,480 USD). Similarly, the mean for wealth aspirations is 21 times the mean for current wealth.

These means are particularly affected by outliers.[5] Median income and wealth aspirations are much lower and the gaps between expectations and aspirations are much smaller. The median household aspires to income and wealth of 20,000 ETB (1,176 USD, per capita roughly 202 USD for an average sized household).

Economists would tend to think of my measure of expectations as measures of respondents' objective beliefs about what will happen to them in ten years. However, I argue that the psychological evidence on goals would suggest that expectations, asked as in this survey, express hopes or goals and are strongly shaped by people's beliefs in their own capabilities: as argued in the Introduction, self-beliefs "set the slate of options for consideration" (Bandura et al., 2001, 201), among which individuals select goals and aspirations.

To measure respondents' beliefs about opportunities and constraints currently facing people like them, I also ask about respondents' beliefs about what others in the village can currently achieve. For income, for example, the questions are "What is the maximum level of

[4]I am conservative in trimming the sample for continuous outcome variables. Individuals who report values on the outcome variable which are four standard deviations or more above or below the sample mean have that outcome variable replaced as missing. In the main sample, this applies to 26 observations on the expectations measure (1 per cent of the sample of individuals) and 52 observations on the measure of aspirations (2 per cent of the sample). An additional nine (three) outliers occur on the aspirations (expectations) data collected straight after screening.

[5]For example, there are three very large outliers on wealth aspirations, so the sample standard deviation is relatively large and the final sample includes high levels of wealth aspirations in placebo and control groups at baseline even after trimming.

income that a person can have in your village?" and "What is the minimum level of income that a person can have in your village?"[6] They may capture changes in objective information, changes in respondents' evaluations of the probabilities of various outcomes, or general changes in participants' optimism or pessimism. These are more akin to how economists conceptualise beliefs. They are unlikely to be influenced by people's self-beliefs.

On average over the sample, respondents report that the minimum income possible is 1,437 ETB (85 USD), while the maximum is 15,941 (937 USD). The medians are 1,000 and 10,000 ETB. People believe the minimum wealth possible to be similar to minimum income, at 1,097 ETB (64 USD). The maximum wealth possible is 21,829 ETB (1,284 USD).[7]

Table 4.6 gives baseline descriptive statistics for expectations, aspirations and beliefs about social status and education. Many longitudinal studies of youth use direct measures to capture the aspirations of young people and their parents for the amount of education children will complete and their future occupation, both in the developed (Strand and Winston, 2008; Strand, 2007; Willitts et al., 2005) and developing world (Boyden, 2013; Dercon and Singh, 2013; Oketch, Mutisya, and Sagwe, 2012).

Mean expectations are full secondary education (12.43 years), and aspirations are somewhat higher (12.91 years), including some college education for many. People believe the minimum education people in the village can attain is close to zero and the maximum people can attain is 11.37 years, short of a full course of preparatory school. This may reflect the difficulties households in remote areas face in accessing preparatory schools, which are usually located in towns, but households aspire and expect to overcome these barriers. High aspirations for education have been found elsewhere in Ethiopia: the Young Lives survey found parental aspirations for their children in poor communities across Ethiopia to be even higher than in this sample, with caregivers aspiring to 14.21 years of education for their children for the cohort of children aged eight in 2009 (Dercon and Singh, 2013).

Currently, about 44 per cent of individuals said they were consulted for important decisions in the village, the measure of social status. At baseline, they aspire to reach 75 per cent and expect to reach 71 per cent. They believe on average that the maximum possible for people in their village is 74 per cent, so they aspire to be as high status as the perceived highest-status

[6] In the main sample, I treat 8 observations on the maximum measure and 41 observations on the minimum measure as outliers. An additional 21 (26) outliers occur on the maximum (minimum) questions collected straight after screening.

[7] It is not possible to assess the accuracy of these beliefs compared to the actual minimum and maximum on these variables in each village, as we sample only 18 households per village, we may not have sampled households with the lowest or highest incomes in each village.

Table 4.6: Balance tests: Expectations, aspirations and beliefs for education and social status at baseline

	Mean	Treatment	Placebo	Control	T–C	P–C	T–P	N
Expectations								
Education	12.43	12.55	12.33	12.41	0.17	−0.10	0.26*	1,936
	(2.52)	(2.41)	(2.47)	(2.66)	(0.16)	(0.15)	(0.15)	
Social status	70.95	70.03	71.57	71.23	−1.62	−0.19	−1.43	2,040
	(29.3)	(30.42)	(28.16)	(29.35)	(1.5)	(1.5)	(1.46)	
Index	0.02	0.02	0.03	0.02	0.00	0.01	−0.01	2,055
	(0.56)	(0.44)	(0.65)	(0.56)	(0.03)	(0.03)	(0.03)	
Aspirations								
Education	12.91	12.96	12.89	12.87	0.09	0.02	0.07	2,015
	(1.71)	(1.65)	(1.53)	(1.93)	(0.10)	(0.10)	(0.09)	
Social status	75.01	73.7	75.71	75.57	−2.31	−0.64	−1.67	2,039
	(31.92)	(32.91)	(31.3)	(31.58)	(1.56)	(1.64)	(1.53)	
Index	0.03	0.02	0.04	0.02	0.00	0.01	−0.02	2,058
	(0.56)	(0.46)	(0.61)	(0.59)	(0.03)	(0.03)	(0.03)	
Beliefs about the minimum others can attain								
Education	0.13	0.12	0.16	0.11	0.02	0.06*	−0.04	1,989
	(0.88)	(0.91)	(0.89)	(0.84)	(0.05)	(0.05)	(0.05)	
Social status	15.38	14.11	16.58	15.37	−1.28	1.07	−2.34***	2,009
	(13.87)	(13.44)	(14.16)	(13.88)	(0.83)	(0.84)	(0.81)	
Beliefs about the maximum others can attain								
Education	11.37	11.55	11.25	11.31	0.24*	−0.07	0.31**	2,031
	(2.44)	(2.22)	(2.56)	(2.5)	(0.13)	(0.14)	(0.13)	
Social status	74.22	71.59	76.22	74.77	−3.61**	0.73	−4.34***	2,042
	(28.68)	(31.3)	(26.79)	(27.68)	(1.47)	(1.41)	(1.43)	

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors (clustered at household level for individual outcomes) are in parentheses. The T–C difference and P–C difference are the treatment and placebo dummies run on the baseline data. The T–P difference is the difference between the treatment and placebo dummies. If p-values are corrected for multiple testing using the Benjamini and Hochberg (1995) procedure, no imbalances are statistically significant.

individuals in their village. Once I correct for multiple testing, no imbalances between groups are statistically significant.

Respondents clearly understand expectations and aspirations differently. Appendix Table B.3 shows that aspirations and expectations at baseline are strongly correlated (the correlation coefficient is 0.33). But Appendix Figure B.1 shows that, except for a small percentage of the sample at low values of the status variable, measures of aspirations stochastically dominate measures of expectations: the percentage of respondents aspiring to a particular level is higher than the percentage expecting to reach that level. There is a larger tail of low expectations in education than in aspirations, and gaps between aspirations and expectations are smaller for education and social status. Nonetheless, I do not draw strong distinctions between expectations and aspirations. I argue both are distal goals over relatively long time horizons

which may not be specific enough to guide everyday choices but act as personal motivators and incentives for action.

The measure also asks respondents to give a ranking of the importance of each of the four dimensions. The enumerators gave each respondent twenty beans and a piece of paper divided into quadrants, explained which dimension of life each quadrant represented and asked respondents to allocate the beans according to the relative importance of each dimension. These weights are used to aggregate the standardised responses to each of the four dimensions into an aspirations index. If a_i^k is individual i's aspiration for dimension k, w_i^k is the weight that individual i assigned to this dimension, μ_i^k and σ_i^k are the sample mean and standard deviation at baseline on dimension k, and the aspiration index is $A_i = \sum_k \left(\frac{a_i^k - \mu_k}{\sigma_k}\right) w_i^k$. Beaman et al. (2012) and Glewwe, Ross, and Wydick (2015) also use indices: I add individual-specific weights. Appendix Table B.2 reports means of the weights. Average weights are relatively close to five beans for each dimension, although simple t-tests across dimensions show differences are statistically significant. In addition, differences in the weights given to each dimension can be quite large for each individual. Results are largely robust to removing the weights.

C. Subjective discount factor and risk aversion

I also measure standard economic measures to capture preferences, namely discount rates and risk aversion. I use survey-based measurement tools to elicit people's subjective discount factor and risk preferences. The subjective discount factor is the value to a person today of 1 ETB received in future (formally, $\beta = \frac{1}{1+\delta}$, where δ is the rate of time preference). I use a similar tool to Cole et al. (2013) in India and Hill, Hoddinott, and Kumar (2013) in Ethiopia and find a very similar distribution over categories to Hill, Hoddinott, and Kumar (2013).[8]

Table 4.7 shows that the mean subjective discount factor is 0.54 and there are no differences between groups at baseline with or without corrections for multiple testing. The relatively high mean subjective discount factor at baseline could reflect respondent suspicion about

[8]I ask if respondents would prefer receiving 100 ETB now or 125 ETB in one month. To those who chose 125 ETB, one ETB in one month is worth between 0.8 and 1 ETB today: they have a monthly discount factor between 1 and 0.8 and are assigned the mid-point of 0.9. If they chose 100 ETB, they then choose between 100 ETB now or 150 ETB in one month; those choosing 150 ETB have a monthly discount factor between 0.8 and 0.667 and are assigned the midpoint of 0.733. Those who have a discount factor lower than 0.667 are asked how much they would need to be given in one month to choose to wait. This measurement assumes a linear utility function, and will estimate a discount rate which is biased upwards (and a discount factor which is downward biased) if the function is actually concave. More complex measures are available to address this (Andersen et al., 2008; Andreoni and Sprenger, 2012), but I do not use these because of the very limited numeracy of our respondents. Results using the log of the discount factor are very similar to those displayed.

Table 4.7: Balance tests: Subjective discount factor and risk aversion at baseline

Panel 1: Balance tests

	Mean	Treatment	Placebo	Control	T–C	P–C	T–P	N
Discount factor	0.54	0.55	0.54	0.55	0.00	−0.01	0.01	2,039
	(0.32)	(0.32)	(0.31)	(0.31)	(0.02)	(0.02)	(0.02)	
Coefficient of partial risk aversion								
Coin toss	0.99	1.06	0.95	0.96	0.10*	−0.01	0.11**	2,037
	(1.00)	(1.02)	(1.00)	(0.99)	(0.06)	(0.05)	(0.06)	
Market	1.22	1.16	1.26	1.22	−0.06	0.03	−0.09	2,026
	(1.13)	(1.10)	(1.15)	(1.15)	(0.06)	(0.06)	(0.06)	

Panel 2: Distribution of sample over categories of risk preference

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

*p below 0.1, **p below 0.05, ***p below 0.01. In Panel 1, robust standard errors (clustered at household level for individual outcomes) are in parentheses. The T–C difference and P–C difference are the treatment and placebo dummies run on the baseline data. The T–P difference is the difference between the treatment and placebo dummies. In Panel 2, options 1 to 5 correspond to the choices in Appendix Table B.2. Using a chi-squared test, there are no significant differences in the distribution over categories between treatment, placebo and control group. For the difference between treatment and control, p=0.263 for coin and 0.106 for maize sale. For the difference between placebo and control, p=0.372 for coin and 0.593 for maize sale.

default on the promised future payment or measurement error because of the hypothetical nature of the question (Cole et al., 2013). Unlike Duflo, Kremer, and Robinson (2008), I find no significant difference between discount rates at baseline in the rainy, hungry season and endline at planting time (p=0.213).

I use survey-based instruments to calculate risk preferences (Binswanger, 1980). Individuals were presented with two hypothetical decisions. The first asked which of five hypothetical payouts they would choose if the payout was determined by a coin toss (Cole et al., 2013). The second asked about the amount of price risk individuals would choose when selling surplus grain output. The distribution of individuals across categories is similar to results from the same measure in the Ethiopian Rural Household Survey (Hill, Hoddinott, and Kumar, 2013). The two questions had the same payout structure, in which the lotteries offered increased in both mean and variance (so payouts were ordered from most to least risk averse), but the payouts in the grain sale measure were multiplied by 100. All payouts had the same, constant probability, as in a coin toss, which is simple to explain to respondents. I assume a constant partial risk aversion utility function (CPR) of the form $U = (1 - S)M^{1-S}$, where U is

utility, S is partial risk aversion (fixed regardless of the level of payoff), and M is the certainty equivalent of a given lottery (Binswanger, 1980).

Table 4.7 shows that the mean coefficient of partial risk aversion at baseline is respectively 1.22 (market) and 0.99 (coin toss). Respondents are significantly more risk averse when answering the question about maize prices fetched at market (p=0.000), as the payouts are larger and higher stakes are often associated with more risk-averse behaviour (Holt and Laury, 2002). There is no systematic difference between groups at baseline, except that the coefficient of partial risk aversion using the coin measure is higher in the treatment than control (at the 10 per cent level) and placebo (at the 5 per cent level), and this difference is not significant once I control for multiple testing. However, when I examine the variable in its original categorical form, the distribution of individuals across categories is balanced for both measures of risk preference using a chi-squared test.

D. Relationships between measures

Table B.3 in Appendix B shows correlations between the main psychological outcome variables. The locus of control and causes of poverty measures both have three scales: people believe outcomes are controlled by themselves, others or chance, and that poverty is due to individuals, others or chance. A high score on any of the locus of control scales is strongly positively correlated with a high score on the corresponding causes of poverty scale, suggesting the scales are behaving correctly. In addition, relationships work as expected between scales. Having a high score on the individual or internal measures is negatively correlated with a low score on the fate/chance and structural/others measures. If one believes people control their own outcomes, one is less likely to attribute their outcomes, or their poverty, to fate or chance.

As predicted by theory, a broad cluster of measures related to having a positive self-concept are positively correlated. Having an internal locus of control is correlated with higher self-esteem, greater life satisfaction, and higher aspirations and expectations. Relatedly, having an external locus of control (having high scores on the chance and others scales) is negatively correlated with these variables.

There is limited work on the relationships between psychological measures and standard economic measures of preferences. People with a higher self-concept (internal locus of control, higher aspirations and expectations) have a lower discount rate (i.e. they are more likely to weight the future similarly to the present) and are less risk-averse. Interestingly, this

differs slightly from findings in developed countries, where people who score highly on the neuroticism scale in the Big Five (in turn positively correlated with self-concept) tend to be more risk averse (Borghans et al., 2009).

Table B.6 in Appendix B reports correlates of the psychological variables at baseline. These behave largely as expected. Older people believe more strongly in the influence of fate and chance on poverty and people's outcomes. Men and women are relatively similar on most measures. Research in the developed world also finds women and men have a similar locus of control (Cobb-Clark and Tan, 2011). Wealthier individuals are more likely to have an internal locus of control and to attribute poverty to individuals, and less likely to agree that powerful others and chance influence people's outcomes, as found in developed world studies (Levenson, 1974; Hunt, 2004; Heckman, Stixrud, and Urzua, 2006). They also report higher life satisfaction. Similarly, individuals with more education are less likely to agree that chance controls outcomes and that fate or structural factors cause poverty, and more likely to have an internal locus of control. They also have higher life satisfaction on one measure, but this may be because they are wealthier.

Similarly, individuals with higher levels of education have higher wealth aspirations and expectations, higher educational aspirations and expectations for their children and higher status aspirations and expectations for themselves. Wealthier individuals (measured by their household assets) have higher aspirations for their own income and expectations for their own wealth. Women have significantly lower aspirations than men on all dimensions except wealth, where their aspirations are lower but not significantly so. This is in line with findings from the developed world (Lips, 2000).

Their expectations (except for status) are not significantly different from men's, however. Older individuals have higher status aspirations and expectations, but not higher wealth or income aspirations or expectations, than younger individuals.

There are fewer relationships between beliefs and demographic variables. If these reflect beliefs about opportunities and constraints for everyone in one's village rather than oneself, there should be no correlation with demographic variables. However, people are likely to be closer to others in the village more like them and may have more information on their levels of these variables. Indeed, being wealthier means one believes others can have slightly higher maximum income and wealth, although coefficients are much smaller (an extra 1,000 ETB in assets increases aspirations for wealth by 2,535 ETB, but only increases one's beliefs about

the maximum possible others can have by 388 ETB). Being more educated also means one believes others can achieve more wealth.

V. Empirical strategy and results

A. *Empirical strategy*

I examine effects on individuals of being invited to attend a documentary screening. Equation 4.1 is the most basic possible specification. y_{i2} measures the outcome variable in the endline survey, six months after the screening, T_i is an individual-level dummy variable equal to one if the individual was invited to a documentary screening, so δ_1 is the direct effect of being invited to a documentary, including both the fact that a screening occurred and the content of the screening. This is the most policy-relevant parameter. P_i is an individual-level dummy variable equal to one if an individual was invited to a placebo screening, so ρ_1 is the effect of being invited to the placebo, which captures potential effects arising simply from the event of a screening and exposure to television in a remote area:

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

η_i is an individual-level error term. I account for village fixed effects using village-level dummies, μ_v, which absorb village-level shocks, village characteristics and unobserved differences between screening sessions. Within villages, random allocation to treatment was at household level, but many outcomes are measured at individual level. For outcomes measured at individual level, I correct standard errors for clustering at household level.

I also examine $\delta_1 - \rho_1$, the difference between the treatment and placebo groups, which identifies the effect of the content of the screening, independent of the effect of the event of attending any screening. This difference is less likely to be significant, because being invited to a screening and a gathering is likely to have some effects on outcomes in some cases.

Importantly, these estimates are only unbiased in the absence of spillovers between groups. In the next chapter, I examine the additional effects of the number of an individual's peers who were treated, which is independent of their own treatment status. I show that there is little evidence of spillovers from treatment, so spillovers are unlikely to bias results.

These estimates provide the Intention-to-Treat (ITT) effect of the intervention. Given the high rates of compliance, these effects are unlikely to differ substantially from the Average

Treatment Effects on the Treated (ATT), so I use the simpler specification. I use the entire sample of respondents who were given tickets, including those non-compliers who missed the screening or attended the incorrect screening. I drop only the 52 individuals for whom we do not have two rounds of data.

In Appendix C, I show robustness to two sets of controls. Equation 4.2 includes y_{i1}, the baseline value of the dependent variable, to control for imbalances in the outcome variable at baseline, without imposing any structure on the relationship between the outcome at baseline and endline. This ANCOVA regression model may be more efficient than either difference-in-difference estimators or simple post-treatment estimation when the outcome variable is measured with noise (Bruhn and McKenzie, 2009):

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

In Equation 4.3, I include a vector of additional controls X_{i1}, measured at baseline: the highest grade the respondent completed at school, whether the respondent is single, and household assets, which were imbalanced at baseline, as well as age and gender, using the indicators described in Section D.:

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

B. Treatment effect on self-beliefs and life satisfaction

All regression tables show the specification in Equation 4.1, including only the treatment and placebo dummies and village fixed effects. In the tables in Appendix C, Column 1 shows the specification in Equation 4.1, Column 2 the specification in Equation 4.2, and Column 3 the specification in Equation 4.3. There is little difference between these three specifications for most outcome variables.

Table 4.8 gives treatment effects on psychological measures. For locus of control, those who saw the documentary score 0.30 points higher than the control group on the Internal scale, which captures if people see outcomes as contingent on individual behaviour (a 1.9 per cent increase, p=0.04, from the baseline mean). This is δ_1 in Equation 4.1. They also score 0.38 points higher than the placebo group (a 2.4 per cent increase, significant at the 1 per cent level, from the baseline mean). This is $\delta_1 - \rho_1$ in Equation 4.1. There is no significant difference on the Internal scale between those invited to a placebo session and those in the control group

Table 4.8: Treatment effects on locus of control, causes of poverty, life satisfaction and self-esteem

	Locus of control			Causes of poverty			Self-	Life satisfaction	
	Chance	Others	Internal	Fate	Struct.	Indiv.	esteem	Best	Happiest
Treated	−0.05	−0.07	0.30**	−0.43**	−0.00	0.41**	0.17	0.06	0.24**
	(0.17)	(0.18)	(0.15)	(0.20)	(0.15)	(0.16)	(0.12)	(0.12)	(0.12)
Placebo	−0.00	0.04	−0.08	0.01	0.27*	0.31**	−0.08	0.03	−0.02
	(0.16)	(0.17)	(0.15)	(0.20)	(0.15)	(0.16)	(0.12)	(0.12)	(0.13)
Treated-	−0.05	−0.12	0.38***	−0.44**	−0.28*	0.10	0.25**	0.03	0.26**
Placebo	(0.16)	(0.17)	(0.15)	(0.20)	(0.15)	(0.16)	(0.12)	(0.12)	(0.12)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	2,039	2,052	2,060	2,063	2,063	2,063	2,058	2,063	2,063
Control mean	13.35	12.62	16.05	9.85	10.36	11.47	17.03	4.85	6.86

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parentheses.

(ρ_1 in Equation 4.1). I interpret this as indicating that the effect arises because of the content of the documentary rather than the event of attending the screening. Similarly, on the causes of poverty measure, the members of the treatment group are significantly more likely than the control group to agree with items that offer individualistic explanations for poverty. The placebo also had a positive effect on the Individual causes of poverty scale (at the 10 per cent level).

Results are more mixed on scales capturing the role of chance and structural factors. There are no significant differences in the Chance locus of control scale. However, those who saw the documentary are 10 per cent less likely than both the placebo and control group to agree with fatalistic explanations that attribute poverty to luck and fate (both differences are significant at the 5 per cent level). The Chance locus of control scale has a lower Cronbach's alpha than is recommended, so answers to individual statements were less closely correlated, and the scale may fail to pick up changes.

There are no treatment effects on the Powerful Others or Structural scales (although the placebo has an effect on the Structural scale at the 5 per cent level). The documentaries clearly focused on one's own behaviour playing a key role in outcomes, but they also highlight the role played by powerful others with more resources, such as people who are able to extend credit or assist with purchasing agricultural resources. Effects are robust in magnitude and significance to the inclusion of the lagged dependent variable and individual controls in Appendix C.

I find a small but significant positive treatment effect compared to the placebo group of 0.25 on self-esteem. This might be expected, given the strong correlations in the literature

between self-esteem and an internal locus of control. The difference between the treatment and control group is of similar magnitude, 0.17, but not significant (p=0.12). There is also significant positive difference of 0.24 (p=0.05) between the treatment group and the control group and of 0.26 between the treatment group and the placebo group (p=0.04) on where participants place themselves on a ladder measuring happiness (the average score on this measure at baseline was 6.86). However, there are no differences on the other measure of life satisfaction. The measure may capture differences in personality traits, specifically the propensity to be satisfied with life (Schimmack, Diener, and Oishi, 2002), but the results are also robust to specifications controlling for the person's answer at baseline, suggesting there is a real improvement in life satisfaction.

C. Treatment effect on aspirations, expectations and beliefs

In Table 4.9, I report the treatment effect of the intervention on measures of aspirations and expectations. The second panel of Table 4.9 reports on aspirations and expectations after six months ($t = 2$). Each column reports a separate estimate of Equation 4.1 for the composite index and income, wealth, education and social status aspirations and expectations respectively.

Being invited to a documentary screening has a positive and significant effect of 0.06 on expectations (significant at the 1 per cent level), compared to being assigned to the control group – or about 10 per cent of a standard deviation. There is also a positive difference of 0.03 between treatment and placebo, although this is not significant (p=0.11). Being invited to a documentary also has a positive effect of 0.04 on the aspirations index compared to the control (at the 10 per cent level), about 7 per cent of a standard deviation, although the difference between the treatment and placebo is not significant.

Treatment effects on aspirations and expectations are largely driven by strong and positive effects on aspirations and expectations for children's education. The treatment results in an increase in the expected grade that the respondent's oldest child will complete of 0.33 years compared to the control (p=0.01) and 0.22 years compared to the placebo (p=0.09). Again, effects on aspirations are smaller and less significant: the treatment effect is 0.18 years of education compared to the control (p=0.09) and the difference between treatment and placebo is not significant.

This effect on parents' aspirations for children's education is particularly plausible in the Ethiopian context, given rapid improvements in access to education. In recent years primary schooling in Ethiopia has become relatively accessible to most families as a means to improve their future opportunities. Access to education has dramatically improved: in 1992, nearly four out of five primary school age children were not in school; by 2009, this was below one in five (Engel, 2010, 7). Even in this remote and relatively hilly district, households in our sample were now on average only 25 minutes' walk from the nearest primary school. Since 1995/6, costs of education have decreased considerably: no fees are charged for the first eight years of primary school, and textbooks are often, although not always, provided by the school. This has reduced barriers to enrolment, although parents still need to pay for stationery and uniforms (Orkin, 2012). High educational aspirations have also been found in other surveys in rural Ethiopia.

Although none of the four documentaries featured a character with significant formal education, many of the subjects were assisted by mentors or community members with education. For example, Beshir Malim Yisak, whose story is summarised in Appendix A, collaborates with a local NGO worker to purchase a pump from Addis Ababa using money he has saved. The documentaries may have indicated to respondents that people with education are able to take action to improve other people's lives, and thus that getting an education is valuable for their children.

Watching a documentary also increased participants' expectations of social status in ten years by three percentage points (p=0.02). There is no difference between treatment and placebo, however, in part because the placebo also improves expectations of social status (p=0.08).

The first panel of Table 4.9 reports on aspirations directly after the screening ($t = 1$). These results should be interpreted with some caution. There may be some persistence between the first and second measures of aspirations and expectations, as there was only between four days and a week between the first and second time that participants did the aspirations questionnaire (although any persistence would be uncorrelated with treatment). In addition, while the methods for interviewing all three groups were identical for baseline and endline ($t = 2$), the treatment and placebo groups were interviewed at the screening venue just after screening ($t = 1$).

Table 4.9: Treatment effects on aspirations and expectations straight after screening and after six months

	Expectations					Aspirations				
	Index	Income	Wealth	Education	Status	Index	Income	Wealth	Education	Status
Panel 1: After screening										
Treated	0.12**	43,649.20*	−6,015.59	0.49***	1.38	0.13**	211,165.61	165,594.07	0.11	−0.81
	(0.06)	(23,775.28)	(9,735.80)	(0.16)	(1.35)	(0.07)	(133,331.30)	(162,204.07)	(0.09)	(1.45)
Placebo	0.02	−2,804.60	−10,120.33	0.44***	0.17	0.00	−27,989.61	19,959.24	0.08	−1.63
	(0.04)	(6,090.47)	(9,333.04)	(0.15)	(1.31)	(0.03)	(47,471.96)	(44,243.20)	(0.08)	(1.44)
Treated -	0.10**	46,453.80*	4,104.75	0.05	1.21**	0.13*	239,155.22	145,634.83	0.02	0.81
Placebo	(0.05)	(23,549.34)	(3,603.29)	(0.12)	(1.33)	(0.06)	(130,035.53)	(156,463.31)	(0.08)	(1.39)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	1,959	1,954	1,956	1,849	1,956	1,959	1,956	1,955	1,930	1,952
Control mean	0.02	31,510.57	35,471.56	12.27	72.15	0.03	168,007.25	82,603.81	12.96	76.46
Panel 2: After six months										
	Expectations					Aspirations				
Treated	0.06***	−3,866.67	1,654.69	0.33***	2.95**	0.04*	4,425.14	−2,806.54	0.18*	0.62
	(0.02)	(5,243.19)	(1,770.50)	(0.13)	(1.29)	(0.02)	(12,196.14)	(5,040.27)	(0.10)	(1.43)
Placebo	0.02	−2,970.60	−992.94	0.11	2.30*	0.03	11,342.53	−2,061.02	0.08	0.97
	(0.02)	(5,473.04)	(1,640.43)	(0.13)	(1.30)	(0.02)	(12,247.84)	(5,132.16)	(0.10)	(1.44)
Treated -	0.03	−896.07	2,647.63	0.22*	0.65	0.01	−6,917.39	−745.52	0.10	−0.35
Placebo	(0.02)	(4,456.28)	(1,760.72)	(0.13)	(1.29)	(0.02)	(12,752.68)	(4,923.55)	(0.10)	(1.45)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	2,062	2,056	2,030	1,890	2,053	2,063	2,052	2,019	1,973	2,059
Control mean	-0.01	36,153.51	24,532.19	12.47	67.14	0.03	110,079.21	55,265.97	12.89	80.39
Difference in post-screening and six-month treatment effects										
P: Treated$_{t=1}$ = Treated$_{t=2}$	0.293	0.052*	0.330	0.482	0.446	0.181	0.148	0.302	0.354	0.697
P: T–P$_{t=1}$ =T–P$_{t=2}$	0.336	0.052*	0.584	0.050**	0.621	0.078*	0.081*	0.387	0.378	0.390

*p below 0.1, **p below 0.05, ***p below 0.01. The indices are in standard deviations. Robust standard errors clustered at household level are in parentheses.The second last row is the p value of a test of whether the difference between the treatment and control is significantly different when measured straight after screening or after six months ($\delta_{1,\,t=1} = \delta_{1,\,t=2}$). The last row is the same test, but for the difference between the treatment and placebo ($\delta_1 - \rho_{1,\ t=1} = \delta_1 - \rho_{1,\ t=2}$). I conduct a seemingly unrelated estimation to account for likely correlations in the error term between the two equations testing aspirations for the same individuals at two points in time.

I thus focus on the comparison between the treatment and placebo groups for these measures. These groups were interviewed straight after the screening at the screening venue using the same methods, whereas the control group were interviewed at their homes. Being invited to a documentary screening has a positive and significant effect (δ_1 in Equation 4.3) of 0.10 (expectations, p=0.03) and 0.13 (aspirations, p=0.04) straight after the screening, compared to being invited to the placebo session. There are also significant positive effects compared to being in the control group of 0.12 (expectations) and 0.13 (aspirations), both significant at the 5 per cent level.

I test whether the differences between treatment effects at $t = 1$ and $t = 2$ are significant by testing differences in coefficients after conducting a seemingly unrelated estimation to account for likely correlations in the error term between the two equations testing aspirations for the same individuals at two points in time. These tests are shown in the last two rows of Table 4.9. These should also be interpreted with caution, because measurement for the control group is not exactly comparable in the two time periods. In some cases, the tests show that the treatment effect does decline over time. In other cases, I cannot reject that the treatment effect straight after screening is the same as the effect after six months, but this is likely because the test is underpowered.[9]

D. Treatment effect on beliefs

Table 4.10 shows treatment effects on beliefs about the minimum and maximum others in the village can currently attain. After six months, the treatment group's beliefs about the minimum level of education others can attain has increased by 0.16 years compared to the control group (p=0.06) and 0.18 years compared to the placebo group (p=0.03). There is no change in beliefs about the maximum level of education. The treatment group's beliefs about the maximum level of income possible for others in the village also increases after six months compared to both the placebo and the control (p=0.01 for both comparisons). This is particularly striking as participants' aspirations and expectations for their own income do not change. There is also a small increase in beliefs about the minimum level of income possible among both the treatment and placebo groups.

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

Table 4.10: Treatment effect on beliefs straight after screening and after six months

	Panel 1: After screening							
	Current minimum others can achieve				Current maximum others can achieve			
	Income	Wealth	Education	Status	Income	Wealth	Education	Status
Treated	42.83	38.27	0.02	-1.52^{*}	$1,078.67^{*}$	1,784.25	0.26^{**}	-0.35
	(67.41)	(49.33)	(0.06)	(0.80)	(644.47)	(1,471.43)	(0.11)	(1.28)
Placebo	-4.48	-33.06	0.01	-1.72^{**}	818.58	2,107.13	0.23^{**}	-0.50
	(67.73)	(48.89)	(0.06)	(0.79)	(656.93)	(1,790.77)	(0.11)	(1.32)
Treated -	47.32	71.34	0.01	0.20	260.09	-322.87	0.03	0.15
Placebo	(71.38)	(48.96)	(0.06)	(0.80)	(687.92)	(1,883.00)	(0.11)	(1.25)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	1,949	1,941	1,956	1,953	1,932	1,954	1,952	1,953
Control mean	1,470.97	984.78	0.17	16.97	14,394.50	19,080.45	11.38	75.81
	Panel 2: After six months							
	Current minimum others can achieve				Current maximum others can achieve			
	Income	Wealth	Education	Status	Income	Wealth	Education	Status
Treated	116.81^{*}	58.40	0.16^{*}	0.21	$2,441.26^{***}$	643.19	0.11	-2.03
	(64.75)	(38.96)	(0.08)	(0.83)	(790.54)	(1,647.70)	(0.11)	(1.26)
Placebo	145.02^{**}	18.57	-0.02	-0.15	470.45	356.03	0.09	0.08
	(62.74)	(37.84)	(0.07)	(0.83)	(742.82)	(1,611.50)	(0.10)	(1.28)
Treated -	-28.21	39.83	0.18^{**}	0.35	$1,970.81^{***}$	287.16	0.02	-2.11
Placebo	(64.60)	(37.87)	(0.08)	(0.83)	(787.29)	(1,528.78)	(0.10)	(1.29)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	2,025	2,036	2,035	2,048	2,026	2,032	2,049	2,043
Control mean	1,538.77	813.58	0.27	15.33	17,703.42	21,260.75	11.47	77.23
	Difference in post-screening and six-month treatment effects							
P: Treated$_{t=1}$ = Treated$_{t=2}$	0.203	0.883	0.162	0.183	0.114	0.657	0.498	0.119
P: T–P$_{t=1}$ =T–P$_{t=2}$	0.973	0.581	0.107	0.735	0.046^{**}	0.586	0.674	0.038^{**}

*p below 0.1, **p below 0.05, ***p below 0.01. The indices are in standard deviations. Robust standard errors clustered at household level are in parentheses. The second last row is the p value of a test of whether the difference between the treatment and control is significantly different when measured straight after screening or after six months ($\delta_{1,\,t=1} = \delta_{1,\,t=2}$). The last row is the same test, but for the difference between the treatment and placebo ($\delta_1 - \rho_{1,\,t=1} = \delta_1 - \rho_{1,\,t=2}$). I conducted a seemingly unrelated estimation to account for likely correlations in the error term between the two equations testing aspirations for the same individuals at two points in time.

Table 4.11: Treatment effects on subjective discount factor and risk aversion

	Subjective discount factor	Risk aversion	
		Coin	Market
Treated	−0.01	−0.11*	−0.05
	(0.02)	(0.06)	(0.06)
	[0.56]	[0.08]	[0.43]
Placebo	−0.03	0.01	0.07
	(0.02)	(0.06)	(0.06)
	[0.12]	[0.85]	[0.21]
Treated-Placebo	0.02	−0.12**	−0.12**
	(0.02)	(0.06)	(0.06)
Village F.E.	*Yes*	*Yes*	*Yes*
Respondents	2,061	2,061	2,061
Control mean	0.54	1.26	1.25

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parentheses.

However, interestingly, these changes in beliefs arise only after six months. Straight after screening, there are few significant effects on beliefs, and those which arise (on the maximum education others can attain) do not persist. Cautiously, I suggest that changes in beliefs about others' opportunities may arise because self-beliefs change: participants may become more positive about their own capabilities and this improves their assessment of other people's opportunities.[10]

E. Treatment effect on subjective discount factor and risk aversion

The documentaries depict individuals who are goal-oriented, plan and focus on the future. Watching the documentaries may have highlighted the value of these behaviours and caused respondents to think more about the future or to be more patient. However, Table 4.11 shows there are no effects on the respondents' discount rate. Changes in behaviour cannot be explained by treated individuals becoming more patient.

Similarly, documentary subjects sometimes took actions that the audience might have perceived as risky, such as investing in new technologies or farming methods or diversifying their economic activities, and watching the documentaries may have encouraged respondents to be less risk-averse. Interestingly, there are some treatment effects on risk aversion. On the coin toss measure, with smaller payouts, the coefficient of partial risk aversion decreases by

[10]Furthermore, questions about beliefs were asked after the questions about aspirations and expectations. There may be some framing effects in the articulation of these beliefs. Ideally, I would have asked aspirations and expectations questions to some of the sample and beliefs questions to the rest, but power considerations made this impossible.

0.11 among the treatment group compared to the control group (p=0.08) and 0.12 compared to the placebo group (p=0.05). The treatment group had a coefficient of 1.06 at baseline, so this is roughly a 10 per cent change. On the market measure, there is no significant difference between the treatment and control groups (p=0.43) but a decline in risk aversion of 0.12 in the treatment group compared to the placebo group (p=0.04). The baseline treatment group mean was 1.16, so this is again roughly a 10 per cent change. At baseline, people with a higher self-concept were less risk-averse, so it is not surprising that an increase in self-concept is related to a decrease in risk aversion.

F. Corrections for multiple testing

As a final robustness check I correct p values for the fact that I test multiple outcomes, which increases the probability of false discoveries. I use the Benjamini and Hochberg (1995) procedure over all outcomes tested in this chapter and the next chapter, but separately over all treatment effects (compared to the control), all placebo effects (compared to the control) and all treatment effects (compared to the placebo). This procedure is less conservative than familywise error rate control procedures, which assess the probability that at least one false discovery is made and tend to be used when a false discovery has large consequences (Anderson, 2008). This study is more exploratory, so I use the slightly less conservative procedure. I also do not use summary index tests, which are suitable for assessing the overall impact of an intervention but give little consideration to each particular outcome. Results for different outcomes have different theoretical implications.

For each outcome variable, Table 4.12 reports the naive p-value from running each estimate independently already discussed in earlier sections of the chapter and the q-value that corrects the p-value for multiple testing. Those p-values that remain significant are in bold, and those that lose significance are in italics.

Some effects remain strongly significant. Treatment effects compared to the control on education expectations and aspirations largely remain significant, as do effects on the expectations index after six months and effects on whether people believe that poverty is determined by individuals.

However, the aspirations index after screening is no longer significant, nor are effects on the internal locus of control, life satisfaction, risk aversion, or whether poverty is determined by fate. All treatment effects on beliefs are no longer significant. The differences between

Table 4.12: Multiple testing corrections: psychological outcomes

		Treatment		Placebo		Treat - Plac	
		p val	q val	p val	q val	p val	q val
Expectations	Index	*0.031*	0.170	0.557	0.987	*0.029*	0.213
t=1	Income	*0.067*	0.179	0.645	0.987	0.049	0.213
	Wealth	0.537	0.702	0.278	0.967	0.255	0.579
	Children's education	**0.002**	0.041	0.004	0.243	0.703	0.914
	Social status	0.308	0.506	0.895	0.987	0.362	0.648
Aspirations	Index	*0.051*	0.170	0.970	0.987	*0.042*	0.213
t=1	Income	0.114	0.240	0.556	0.987	*0.066*	0.244
	Wealth	0.308	0.506	0.652	0.987	0.352	0.648
	Children's education	0.234	0.419	0.323	0.987	0.777	0.914
	Social status	0.574	0.702	0.258	0.967	0.559	0.892
Expectations	Index	**0.009**	0.087	0.272	0.967	0.111	0.344
t=2	Income	0.461	0.648	0.587	0.987	0.841	0.914
	Wealth	0.350	0.545	0.545	0.987	0.133	0.357
	Children's education	**0.009**	0.087	0.409	0.987	0.089	0.308
	Social status	*0.022*	0.145	0.078	0.655	0.617	0.911
Aspirations	Index	*0.062*	0.174	0.161	0.949	0.602	0.911
t=2	Income	0.717	0.784	0.355	0.987	0.588	0.911
	Wealth	0.578	0.702	0.688	0.987	0.880	0.914
	Children's education	*0.073*	0.179	0.434	0.987	0.299	0.609
	Social status	0.664	0.769	0.499	0.987	0.809	0.914
Minimum	Income	0.525	0.702	0.947	0.987	0.508	0.853
t=1	Wealth	0.438	0.631	0.499	0.987	0.145	0.374
	Children's education	0.796	0.819	0.911	0.987	0.883	0.914
	Social status	*0.057*	0.170	*0.030*	0.444	0.799	0.914
Maximum	Income	*0.094*	0.215	0.213	0.967	0.705	0.914
t=1	Wealth	0.226	0.416	0.240	0.967	0.864	0.914
	Children's education	*0.019*	0.141	0.027	0.444	0.790	0.914
	Social status	0.782	0.819	0.704	0.987	0.906	0.922
Minimum	Income	*0.072*	0.179	0.021	0.444	0.662	0.914
t=2	Wealth	0.134	0.264	0.624	0.987	0.293	0.609
	Children's education	*0.054*	0.170	0.782	0.987	0.029	0.213
	Social status	0.805	0.819	0.860	0.987	0.673	0.914
Maximum	Income	**0.002**	0.041	0.527	0.987	*0.012*	0.213
t=2	Wealth	0.696	0.776	0.825	0.987	0.851	0.914
	Children's education	0.309	0.506	0.359	0.987	0.859	0.914
	Social status	0.107	0.233	0.951	0.987	0.101	0.333
Locus of control	Chance	0.761	0.817	0.994	0.995	0.766	0.914
	Others	0.684	0.776	0.794	0.987	0.497	0.853
	Internal	*0.045*	0.170	0.533	0.987	*0.010*	0.213
Poverty causes	Fate	*0.036*	0.170	0.957	0.987	*0.031*	0.213
	Structural	0.985	0.985	*0.067*	0.655	*0.063*	0.244
	Individual	**0.010**	0.087	*0.052*	0.609	0.520	0.853
Self-esteem		0.123	0.251	0.460	0.987	*0.022*	0.213
Life satisfaction	Best life	0.641	0.757	0.790	0.987	0.836	0.914
	Happiest life	*0.051*	0.170	0.898	0.987	*0.039*	0.213
Discount factor		0.563	0.702	0.122	0.880	0.322	0.614
Risk aversion	Coin toss	*0.076*	0.180	0.854	0.987	*0.050*	0.213
	Market	0.434	0.631	0.215	0.967	*0.044*	0.213

q-values adjusted for multiple testing are calculated using the Benjamini and Hochberg (1995) procedure. p values in italics lose significance after multiple testing corrections; p values in bold remain significant.

treatment and placebo are no longer significant on any variables; nor are any placebo effects compared to the control. Many results remain close to significance, so I interpret this as an issue of statistical power. Our power calculations did not account for multiple testing corrections, so only results which were significant at the 1 per cent level without multiple testing corrections remain significant when I correct for multiple testing. Unfortunately, this was an exploratory study with a small amount of funding, which limited the size of the sample.

VI. Implications for theory

Although my thesis is largely empirical, I also ask whether the fact that aspirations, self-efficacy and locus of control have empirical relevance in turn requires that assumptions in standard models of decision-making in economics need to be modified or even rejected. I argue that many of these ideas can largely be integrated into standard models using existing concepts and maintaining most assumptions. However, my empirical results suggest that the ways in which aspirations, self-efficacy and locus of control are currently integrated into economic models requires some adjustment.

A. Are self-beliefs fixed or frame-dependent?

A key question, highlighted in Chapter 3, is whether psychological concepts are fixed, stable and consistent or whether they are frame-dependent. My results suggest that self-beliefs are underlying, fundamental appraisals of oneself which are relatively stable across different contexts in people's lives. This finding is in opposition to the frame-dependent notion of self-beliefs, which suggests that people's self-beliefs differ in different contexts: people may have high self-beliefs and aspirations for themselves in economic activities, but not in personal relationships, for example.

In this data, people's beliefs about themselves appear to influence a wide variety of other beliefs they hold. People's underlying self-concept is correlated with many of their other beliefs, such as beliefs about the factors causing poverty and beliefs about others' opportunities. Furthermore, there is suggestive evidence that changes in beliefs about other people's opportunities are driven by changes in underlying self-beliefs, because changes in beliefs about others' opportunities only arise after six months, after changes in self-beliefs have occurred. The next chapter will also demonstrate that viewing the documentary and the

underlying changes in self-beliefs and beliefs about opportunities which result are linked to changes in quite different domains of behaviour, such as savings, use of credit and investment in children's education, suggesting that changes in these beliefs may result in changes in many aspects of behaviour across many different contexts in people's lives.

However, this study suggests beliefs about oneself are not necessarily stable over time. Even a light-touch intervention can have an effect on individual locus of control (and more marginal effects on self-esteem) over a six month period, so such concepts are not entirely stable in adulthood but can change over time, as in Cobb-Clark and Schurer (2013) and Gottschalk (2005). Unlike personality traits or other non-cognitive skills, which are argued to be relatively fixed in adulthood, as discussed in Chapter 3, self-beliefs evolve over time and can be altered by relatively light-touch interventions. This has important implications for researchers in economics on non-cognitive skills, who often use estimation strategies which rely on these concepts being stable over time. This may undermine arguments for causal links rather than correlations in the empirical literature demonstrating that locus of control, self-esteem and self-efficacy are linked to improved economic outcomes in longitudinal data.

B. Are locus of control, self-esteem and self-efficacy skills or beliefs?

As outlined in Chapter 3, economists have begun to recognise problems with their very broad conceptualisation of non-cognitive skills and have begun to focus on the narrower concept of personality traits (Borghans et al., 2008). However, much economic work on the effects of non-cognitive skills on outcomes (described in the Introduction to the next chapter) uses measures of locus of control and self-esteem as measures of non-cognitive skills, in part because these measures are widely used in longitudinal studies.

I argue that locus of control and self-esteem should be conceived of as a type of belief, self-beliefs, rather than as non-cognitive skills. The key issue is how they are learned: skills are learned through direct experience and practice, but people's self-beliefs can be altered through direct experience or through vicarious experiences, including exposure to role models, or verbal persuasion (Bandura, 2002, 2004). In this experiment, people were not given any practical opportunities to practise new skills: they were simply exposed to a source of information, which could be construed (through the lens of social learning theory) as a vicarious experience, and which resulted in small but significant changes in locus of control. In contrast, it is difficult to conceive of someone acquiring a skill without any direct experience

or practice in using the skill, so participating in these types of experiment does not seem likely to alter a skill.

Secondly, considering these concepts as beliefs allows that self-beliefs are construals or interpretations of an individual's capabilities and are often inaccurate, because people interpret their past performance and update their self-beliefs through the lens of their current self-beliefs. Self-beliefs, like any other beliefs in economics, can be inaccurate: work on imperfect information (Akerlof, 1970; Greenwald and Stiglitz, 1986) and on decision-making under uncertainty (Tversky and Kahneman, 1974) highlights that beliefs can be incorrect and are often updated to accommodate new information. However, I cannot speak to the accuracy of self-beliefs either before or after the intervention.

Including ideas of construal and subjective interpretation addresses what some psychologists argue is a core deficit in more cognitively focused perspectives on self-beliefs. Jerome Bruner (1990), one of the instigators of psychology's cognitive revolution, argues that the emphasis on the processing of information rather than the construction of meaning has "dehumanised the very concept of mind" (Bruner, 1990, 1) that the cognitive revolution had sought to re-establish, making it difficult for psychologists to capture how the mind both constitutes and is constituted by culture. He believes cognitive science had become overly focused on technical advances in neurobiology and information processing and argues for more consideration of processes of construal and meaning-making, which are considered more by social psychologists. He argues for a return to what he perceives to be the original purpose of the cognitive revolution:

> ...an all-out effort . . . to discover and to describe formally the meanings that human beings created out of their encounters with the world, and then to propose hypotheses about what meaning-making processes were implicated. It focused on the symbolic activities that human beings employed in constructing and in making sense not only of the world, but of themselves (Bruner, 1990, 2).

C. Does studying aspirations require adopting a behavioural economics perspective?

Finally, I argue that the results of this experiment support a perspective on goals and aspirations which draws on social learning theory from social psychology rather than more

backward-looking theories of goal-setting in behavioural economics and other parts of psychology, both of which were outlined in the Introduction to this chapter.

There is little disagreement between the two perspectives on whether social comparisons and learning from relevant others are important determinants of aspirations: some behavioural economists and social learning theorists both allow that this is the case. Both perspectives also agree that goals or aspirations drive the amount of effort exerted and affect future performance and outcomes. In addition, they agree that aspirations and goals are affected by past performance.

There are three major differences between the two perspectives. The first is whether past performance is the only determinant of aspirations, as some behavioural economists argue (Selten, 1998, 2008; Selten, Pittnauer, and Hohnisch, 2012; Dalton, Ghosal, and Mani, 2015), or if self-beliefs also play a role. It is very difficult to establish the relative influence of past performance and self-belief on aspirations: past performance captures past self-efficacy and aspirations (Sternberg and Kolligian, 1990) and current self-efficacy and aspirations may capture past performance (Vancouver, Thompson, and Williams, 2001, 608). However, straight after the screening, the intervention had not altered performance or outcomes among individuals at all, but self-concept and aspirations had changed. This suggests that aspirations and self-belief are related, independently of performance. Aspirations may thus change even if people do not receive any new information about past performance, so it cannot be the only determinant of aspirations.

Secondly, behavioural economists do not examine how aspirations fit into the broader constellation of psychological concepts. Social learning theorists hypothesise that there is a relationship between self-belief and aspirations. I find support for this perspective. The variables are correlated at baseline and they are both affected by the same intervention.

Finally, social learning theorists emphasise that people take "agentic action": they are creative and proactive and explore and manipulate the environment to make desired goals occur (Diamond, 1988). Relatedly, consciousness plays a central role in instigating, motivating and regulating any actions taken in the environment, (Carlson, 1997). Thus, sensory, motor but also cerebral systems are used by people to accomplish tasks and goals which give them meaning and satisfaction (Bandura, 1997). Rather than taking sub-consciously biased decisions, they consciously set goals. However, I am not able to adjudicate between these perspectives, because the study did not discuss the process of setting and revising aspirations.

This perspective on goals and aspirations provides a theoretical framework for integrating these concepts into economics that does not rely on viewing people as satisficers. This has the important implication that one does not have to accept behavioural economics departures from core assumptions of neoclassical economics, in particular the assumption that people satisfice rather than optimise, to view aspirations as important. People may maximise or optimise, like rational agents in standard economic models, but may nonetheless exclude certain options because they do not believe they have the capability to attain them.

VII. Conclusion

In this experiment, some individuals from an isolated district in rural Ethiopia were randomly selected to be invited to watch four 15-minute documentaries. The videos were about how people from similar backgrounds to them had, through their efforts, improved their socio-economic position: from being poor or of average socio-economic position in their communities to being relatively successful. The documentaries did not provide specific information on returns to particular behaviours or specific advice on behavioural change. There were no other changes in opportunities or constraints for the treatment group.

When individuals were followed up six months after the screening, there were significant changes in psychological measures of people's perception of whether they are able to shape their lives and future (namely their locus of control from psychology and perceptions of the causes of poverty from sociology) among individuals who saw the documentary. The intervention increases the extent to which individuals think their outcomes are contingent on their own actions and decreases the extent to which they think that poverty is caused by fate or chance. However, only the effect on the perception that poverty is caused by individual actions is robust to multiple testing. These results add support to existing research suggesting that measures of self-belief are not stable over time in adult populations, as has been commonly assumed. Unlike skills, they can be altered through interventions which expose people to vicarious experiences of the lives of role models, but without individuals having any new experiences themselves.

I find a significant increase in the aspirations of treated individuals, measured both immediately after the screening and six months later. Indices of aspirations and expectations increase by 0.07 and 0.1 standard deviations respectively after six months, although only the latter result is robust to multiple testing corrections. The intervention also increased

parents' aspirations and expectations for children's education six months after the screening, by between 0.18 and 0.33 years of schooling, which is robust to multiple testing corrections. Psychologists have found extensive evidence of correlation between aspirations and an internal locus of control, so it is not surprising that the intervention alters aspirations.

I also find some small increases in self-esteem and well-being, and decreases in some measures of risk aversion. Self-esteem and well-being are strongly correlated with locus of control in other research, and at baseline in this data, so these results are not surprising, but they are small in magnitude and not robust to multiple testing. My results on risk aversion are the opposite from those predicted by the limited literature on correlations between risk aversion and personality: people with a higher self-concept in the developed world tend to be more, not less, risk-averse. However, these effects are small and not robust to multiple testing. The intervention had no effect on discount rates.

The intervention alters participants' beliefs about the opportunities facing people like them, in particular the minimum education level and the maximum income level they can attain. These effects also arise mainly after six months. Straight after screening, there are few significant effects on beliefs, and those which arise (on the maximum education others can attain) do not persist. This measure is an imperfect measure of objective beliefs about opportunities and constraints: it may also capture subjective evaluations of the probabilities of various outcomes, and participants' optimism or pessimism about available opportunities. Given that the measure is imperfect and results are largely not robust to multiple testing correction, these results are not of major concern. However, although the documentaries did not provide specific information on returns to particular behaviours or specific advice on behavioural change, it is not entirely possible to rule out that individuals derive new information from the videos or that existing information becomes more salient.

The experiment provides some suggestive evidence that people's underlying self-concept is relatively stable across contexts: self-beliefs, aspirations and beliefs about others' opportunities are all correlated. Furthermore, there is suggestive evidence that these changes in beliefs about other people's opportunities are driven by changes in self-beliefs, because changes in beliefs about others' opportunities only arise after six months, after changes in self-beliefs have occurred. Participants may become more positive about their own capabilities and this improves their assessment of other people's opportunities. This suggests that the primary psychological effect is a change in self-beliefs and aspirations. The next chapter will also show

that watching the documentary has effects on behaviour in diverse domains over a six-month period.

5

The future in mind: Exposure to role models and forward-looking behaviour

I. Introduction

This chapter examines whether exposure to the documentaries discussed in the previous chapter changes behaviour six months after the screening. The experiment only robustly identifies causal links between watching the documentary and changes in psychological outcomes and between watching the documentary and changes in future-oriented behaviour. However, I argue that the experiment also provides some suggestive evidence on whether changes in self-beliefs cause changes in future-oriented behaviour, as social learning theory in psychology would suggest. Households within each village were randomly allocated to treatment, so it is unlikely that any other opportunities or constraints changed solely for these households in the six months between baseline and follow-up. I argue that any changes in behaviour did not occur because the treatment group received specific relevant information on untapped opportunities, because the documentaries contained little information to which participants did not already have access (although the documentaries may have made existing information more salient). Effects on psychological variables appear to arise because of the content of the documentary, not simply because people attend a screening and gathering in a remote rural area, because attending a placebo documentary has no effects on beliefs and, in most cases, the treated group's self-beliefs and aspirations are different from both the placebo and the treatment group.

Psychologists in social learning theory argue that self-beliefs and their expression in goals and aspirations are important determinants of future-oriented behaviour, but proving

the existence of a causal relationship is difficult. Economists studying non-cognitive skills have accumulated extensive evidence from longitudinal studies that self-beliefs, particularly locus of control and self-esteem, at earlier stages of life are strongly correlated with life outcomes. Most studies find a more internal locus of control is correlated with higher wages (Dunifon and Duncan, 1998; Andrisanni, 1977; Andrisanni and Nestel, 1976; Semykina and Linz, 2007), although some work finds no effects (Goldsmith, Veum, and Darity, 1997; Duncan and Morgan, 1981). Similar relationships are found for self-esteem (Drago, 2011; Murnane et al., 2001) and core self-evaluations (Judge and Hurst, 2007). The size of the effect is similar to the effect of cognitive ability (Heckman, Stixrud, and Urzua, 2006; Heckman and Kautz, 2012). A more internal locus of control is also correlated with more effective job search behaviour (Caliendo, Cobb-Clark, and Uhlendorff, 2015), higher-return occupational choices (Cobb-Clark and Tan, 2011), a lower probability of unemployment (Lindqvist and Vestman, 2011), a higher probability of graduating from high school (Cebi, 2007; Coleman and Deleire, 2003), and better ability to cope with unanticipated life events such as health shocks (Schurer, 2014) or unemployment (Caliendo, Cobb-Clark, and Uhlendorff, 2015). There is also some evidence of links between higher aspirations, occupational choice (Haller and Miller, 1963; Haller et al., 1974; Cook et al., 1996) and educational attainment (Quaglia and Cobb, 1962; Page, Garboua, and Montmarquette, 2007).

Because non-cognitive skills only stabilise in adulthood, using measures captured a long time before outcomes results in measurement error. But it then becomes difficult to establish the direction of the relationship between locus of control and economic, social and demographic events, which "poses enormous econometric challenges and renders much of the applied literature in this area rather unconvincing" (Cobb-Clark and Schurer, 2013, F363). For example, Gottschalk (2005) finds that an exogenous increase in work hours induced by an experimental tax credit increased internal control tendencies among welfare recipients, suggesting that employment affects psychological outcomes. Much work assumes the relationship runs in the opposite direction.

To attempt to establish causality, economists have conducted randomised controlled trials to evaluate education and policy interventions that may enhance self-beliefs. The famous Perry Preschool early childhood development programme, targeted at disadvantaged children, had no effects on IQ, but had large effects on self-esteem and locus of control. It improved achievement test scores, social skills, schooling attainment and employment outcomes among

the treated group and reduced risky behaviours (Heckman, Stixrud, and Urzua, 2006). Such interventions are also effective with older children: training to improve motivation, self-esteem, and study skills in Portugal decreased grade retention (Martins, 2010). A non-experimental evaluation found a long-term life skills programme in Bombay slums improved self-esteem, self-efficacy, life satisfaction, aspirations, exam results and initial labour market outcomes (Krishnan and Krutikova, 2013).

Among adults, a group mentorship programme among sex workers in India improved self-reported measures of self-esteem, agency, and happiness among members of the treatment group and improved workers' future-orientation: women who participated in the programme were more likely to choose a future-oriented savings product and visit a doctor, even though the training programme included no specific mention of health issues (Ghosal et al., 2013). However, the programme involved skills training, which may have had direct effects on economic outcomes and behaviour, so it is difficult to identify whether psychological changes are the primary mechanism behind changes in behaviour.

Another recent trial provided cognitive behavioural therapy and cash transfers to young Liberian men with high rates of crime and antisocial behaviour led to large, sustained decreases in involvement in crime and violence and modest long-term improvements in savings and homelessness. The cash transfer on its own had few benefits (Blattman, Jamison, and Sheridan, 2015), suggesting that psychological interventions cross-cut with existing, standard anti-poverty programmes may magnify the effects of standard programmes.

As is typical in economics, these experiments focus on bundled, long-term, intense interventions which may activate a number of psychological mechanisms and simultaneously affect both objective constraints and behaviour. There may be considerable within-programme heterogeneity in treatment administration and compliance in long programmes and unobservably different participants may be more likely to comply or to participate actively and gain more considerably. Such experiments have the advantage of testing the efficacy and cost-effectiveness of real-world programmes. But this can come at the cost of precisely identifying the theoretical mechanism behind any changes (Deaton, 2010, 2012; Barrett and Carter, 2010; Mookherjee, 2005): in this case, isolating whether changes in particular psychological variables cause changes in behaviour.

Laboratory experiments in psychology are at the opposite end of the methodological spectrum. They aim to target a specific underlying psychological process, trait, attitude

or belief, and identify the psychological mechanisms behind behaviour change (Walton, 2014, 74). They are usually lower-intensity to ensure that there is exact compliance with treatment protocols and the same treatment dose is received by all participants. Follow-ups are usually immediate. Experiments examine attitudes, intentions, or behaviour that can be measured by observation in the laboratory. For example, psychologists have experimentally manipulated self-efficacy beliefs by randomly assigning people positive or negative feedback on a task. More positive feedback induces higher self-efficacy. Those with higher perceived self-efficacy in turn set higher goals, perform better on academic and physical tasks in the laboratory, improve their problem-solving strategies and persevere more (Wood and Bandura, 1989; Bandura and Cervone, 1986; Bouffard-Bouchard, 1990; Boyer et al., 2000; Stajkovic and Luthans, 1998; Sadri and Robertson, 1993). More recently, psychologists have implemented similar psychologically precise, brief interventions in real-world settings, but still conduct follow-ups after a few weeks or months (see Walton, 2014; Cohen and Sherman, 2014, for reviews).

This experiment strikes a balance between these two traditions. The intervention is short and administered by the same team of enumerators across sites to ensure homogeneity in treatment and high compliance. It is targeted at particular psychological variables which theory suggests are linked to future-oriented behaviour. The intervention largely affects only this subset of beliefs, in one's own capabilities, although there are some, less robust, effects on risk aversion and beliefs in opportunities available to others. Follow-up is on a limited set of measures on aspirations, expectations and beliefs immediately and then on a full set of self-reported measures of behaviour after six months.

I find small but significant changes in behaviour among the treatment group compared to the control group and, in some cases, compared to the placebo group. Treated individuals have 68 per cent higher total savings than the control and placebo groups and have taken out 22 per cent more credit than the control group. There is also a positive treatment effect on a hypothetical demand for credit question. However, results are not robust to correction for multiple testing. There are no effects on individuals' time allocation to work or leisure. While small in absolute terms, individuals in this deprived area have limited stocks of savings and use of loans to begin with, so changes are large in relative terms, particularly because they occur six months after a one-hour intervention.

I also find strong effects on the number of children in the household of primary school age enrolled in school and in total spending on children's education. The number of children aged 7 to 15 enrolled in school was 0.28 larger in the treatment group compared to the control group (p=0.00) and 0.20 larger compared to the placebo group (p=0.02). The latter effect is a 21 per cent increase from the baseline average of 1.02 children enrolled in school across all groups and 17 per cent larger than the control group average of 1.23 at the time of the endline survey. Treated households also spent 31 per cent more than the control group on children's education. Results are robust to multiple testing corrections.

The experiment does not demonstrate that such a light-touch intervention alters behaviour in a sustained or economically significant way in the long term: the follow-up survey took place six months after baseline and changes are often small in absolute terms (although they are often large in relative terms, given participants' limited resources). Rather, I provide empirical evidence of the existence of a theoretical mechanism which suggests how psychological characteristics might be related to poverty and how psychological interventions might contribute to poverty alleviation. More intensive intervention targeted at the same variables might substantively alter outcomes.

Overall, despite a relatively light-touch intervention – a one-hour documentary screening – I find clear evidence of behavioural changes six months after treatment. Thus exposure to the documentary and to a gathering altered behaviour. In some cases, I can argue that effects arise solely from the content of the documentary because the treatment group is also significantly different from the placebo group; in other cases I can only argue that the combination of exposure to the documentary and to a gathering or screening had an effect. Although exposure to the placebo does not have any statistically significant effect on outcomes, being singled out to be invited to a gathering may plausibly have a slight effect on behaviour, so that it is not possible to detect differences between treatment and placebo.

Section II. describes the outcomes measured and their behaviour at baseline. Section III. gives results. Section IV. tests for the presence of spillovers from the treated group to others in the village. As discussed in the introduction to the previous chapter, observational learning from the experiences of other people is a key influence on the formation of self-concept, self-efficacy and aspirations. In addition, tests for spillovers are key to demonstrating that direct treatment effects are cleanly identified.

II. Outcomes and measurement

I measure behaviours that might suggest individuals are planning more for the future or making investments that might improve their future economic position. I could not investigate whether individuals altered their productive activities, such as by diversifying into non-agricultural activities, farming new crops or using different farming methods, as individuals would have required at least a full agricultural year to make such changes. I examine adult time allocation, savings and credit behaviour, and investment in children's education, both in children's enrolment and in spending on education. Table 5.1 reports descriptive statistics for these outcome variables at baseline.[1]

To capture time allocation to work and leisure, the household head reported the amount of time each member of the household spent on types of tasks on a typical day during March. The time allocation questions referred to March 2010 (baseline) and 2011 (endline) to avoid seasonal differences in time allocation, as the baseline and endline took place at different times of year. Any differences in measurement between rounds would be unrelated to treatment. I focus on time spent working on the farm or in business and in leisure (including eating, bathing and sleeping). The head may be more likely to report accurately on these measures than on time spent working in the home, which is difficult to measure. At baseline, individuals spent an average of 5.8 hours in farm work, and 12.3 hours in leisure on a typical day of March.

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

To measure credit, I ask about all loans larger than 15 ETB (just under 1 USD), including from co-operatives, banks or micro-finance institutions, money-lenders, savings and loan groups, *iqqub* and friends or family. Total credit is the total value of the loans larger than 15 ETB the respondent has taken out in the last six months, considering only the value of the

[1] I use the same procedure for trimming as in the previous chapter: on continuous variables, I drop an observation only for a particular outcome if it is above or below four standard deviations from the mean of the variable in that round. I lose 41 observations on individual-level variables (1.9 per cent of the sample) and 61 observations on household-level variables (5.3 per cent of the sample). Trimming does not affect the magnitude or significance of results.

Table 5.1: Experimental integrity: Future-oriented behaviour at baseline

	Mean	Treatment	Placebo	Control	T-C	P-C	T-P	N
Variables at individual level								
Daily time in work (minutes)	348.72	351.56	349.46	345.15	7.71	4.85	2.86	1,961
	(219.28)	(210.96)	(223.05)	(223.69)	(8.26)	(8.52)	(8.34)	
Daily time in leisure (minutes)	740.14	735.64	734.97	749.90	−15.21	−15.98	0.77	1,961
	(182.58)	(174.3)	(186.48)	(186.37)	(10.02)	(10.26)	(9.66)	
Any cash savings	0.22	0.25	0.21	0.21	0.04*	0.00	0.04*	2,063
	(0.42)	(0.43)	(0.41)	(0.41)	(0.02)	(0.02)	(0.02)	
Total savings (birr)	82.97	105.76	73.20	70.60	36.70	4.08	32.62	2,057
	(544.84)	(675.66)	(516.46)	(413.81)	(34.97)	(26.04)	(37.45)	
Taken out credit in last year	0.43	0.45	0.45	0.40	0.04	0.05*	−0.01	2,063
	(0.50)	(0.50)	(0.50)	(0.49)	(0.03)	(0.03)	(0.03)	
Amount of credit (ETB)	176.17	188.80	181.03	158.78	31.64*	22.03	9.61	2,063
	(330.40)	(354.40)	(320.36)	(315.46)	(18.29)	(17.49)	(18.23)	
Variables at household level								
	Hypothetical demand for credit (ETB) if loan is repayable in							
...1 year	5,867.38	5,683.29	5,550.67	6,363.85	−694.37	−837.41	143.04	1,131
	(7,375.2)	(6,498.55)	(7,227.83)	(8,278.18)	(524.83)	(525.44)	(526.13)	
...5 years	12,448.34	11,556.43	13,074.59	12,708.59	−1,023.30	366.04	−1,389.34	1,130
	(17,543.55)	(13,681.76)	(21,356.03)	(16,729.41)	(1,248.29)	(1,246.1)	(1,248.74)	
...10 years	22,210.22	19,117.69	24,566.58	22,905.87	−3,360.00	1,786.02	−5,146.02**	1,122
	(35,842.75)	(22,726.58)	(46,508.3)	(34,044.71)	(2,563.77)	(2,555.36)	(2,570.14)	
Children 7-15 enrolled	1.02	1.13	0.96	0.97	0.16**	−0.01	0.17**	1,138
	(1.11)	(1.14)	(1.06)	(1.10)	(0.08)	(0.08)	(0.08)	
Children 7-15 in household	1.88	2.06	1.85	1.73	0.33***	0.12	0.21**	1,138
	(1.51)	(1.53)	(1.49)	(1.49)	(0.11)	(0.11)	(0.11)	
Expenditure on schooling ETB)	184.68	217.68	182.66	153.70	64.23***	29.68	34.56*	1,138
	(280.91)	(316.76)	(279.18)	(238.33)	(19.49)	(19.47)	(19.47)	

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors (clustered at household level for individual outcomes) are in parentheses. The T–C difference and P–C difference are the treatment and placebo dummies run on the baseline data. The T–P difference is the difference between the treatment and placebo dummies. If p-values are corrected for multiple testing using the Benjamini and Hochberg (1995) procedure, only the imbalances in school expenditure and the number of children in the household between the treatment and control group remain statistically significant and these are only significant at the 10 per cent level.

loan when it was first taken out. At baseline, 43 per cent of individuals had taken out a loan of more than 15 ETB in the past six months, and including those who had not taken out any credit, individuals had taken out loans of a total of 176 ETB (10 USD) on average.

I also examine some questions asked only to the household head. Respondents were asked: "Someone from a micro-finance institution came to you and offered to lend you any amount of money you ask without charging interest or service charge. How much would you ask for if the loan is payable in 1 year?". The question was repeated for loans with a 5 and 10 year maturity. This is intended to capture demand for credit, which may not be captured by the actual loans people take out: there are credit market failures in any rural setting and the Ethiopian government imposes strict limitations on lending. The amounts individuals would borrow increase with the length of the repayment period, from 5,867 ETB if the loan is payable in 1 year, to 12,448 ETB if the loan is payable in 5 years, to 22,210 ETB if the loan is payable in 10 years. Loan size is usually responsive to changes in loan maturity (Karlan and Zinman, 2005).

Finally, I measure the number of children in the household between the ages of 6 and 15 who were enrolled in school at the beginning of the relevant school year, namely 2009/10 at baseline and 2010/11 at endline. The age range is when schooling is compulsory: children are supposed to enrol in Grade 1 when they have turned 7 and stay until Grade 8, when they would be about 14 or 15. I examine all households in the sample, including 107 households who have no children in this age group in both rounds, to ensure the sample is comparable with other results. At baseline, the average household had 1.88 children in this age group in the household and 1.02 children aged 7 to 15 enrolled in school, including households with no children in school. Households with at least 1 child in school at baseline had on average 1.81 children aged 7 to 15 enrolled in school. Since 24 per cent of households with children in this age group had no children in school, there is unlikely to be a ceiling effect on this variable.

I also examine annual spending on schooling for children in the household in the previous school year: the amount spent on uniforms, stationery and books, textbooks, and donations to the school. At baseline, households spent 186 ETB (11 USD) per year per household (including households with no spending on schooling). Among households with children aged 7 to 15 in school at baseline, the average expenditure on schooling expenses was 289 ETB (17 USD). An extensive literature search could not identify other estimates of the amount households spend on children's school expenses in Ethiopia. On average, across 13 African countries, households

spent an average of 4.6 per cent of the household's annual spending on primary schooling (Foko, Tiyab, and Husson, 2012). In this sample, households with children in this age group in school at baseline had annual cash income of 7,746 ETB (I do not have an expenditure measure), so schooling expenses are 3.73 per cent of household income.

There are no significant differences between treatment, placebo and control groups on time allocation, savings or credit variables. There are some imbalances in savings, demand for credit, and the number of children in the household enrolled in school, but these are not significant once I correct for multiple testing. Only two imbalances – between the treatment and control group on education expenditure (q=0.053) and the number of children in the household (q=0.053) – remain significant after multiple testing corrections. There are differences between the treatment and control groups and treatment and placebo groups on the number of children aged 7 to 15 in the household: the treatment group have 2.06 children in this age group on average, while the control group has 1.73 children. Treatment households also spend more on education than the control. As a precaution, I test robustness to the inclusion of the outcome variable at baseline as a control variable in the regressions in Appendix C.

III. Results

A. Savings and credit

I use the same specifications to examine whether exposure to the documentary causes changes in psychological variables and changes in forward-looking behaviour. I display the basic specification and two robustness checks. Column 1 shows the specification in Equation 4.1 in the previous chapter, which includes only the treatment and placebo dummies and village fixed effects. Column 2 shows the specification in Equation 4.2, which adds y_{i1}, the baseline value of the dependent variable, to control for imbalances in the outcome variable at baseline, without imposing any structure on the relationship between the outcome at baseline and endline. Column 3 shows the results from Equation 4.3, which includes a vector of additional controls X_{i1}, measured at baseline. Table 5.2 reports on all outcomes.

The documentaries emphasised that the subjects had worked hard to make progress, so one might expect an increase in hours in work among the treatment group. I find no evidence of an effect of watching the documentary or placebo on time allocation, although households

probably had some potential to increase hours worked, as on average they worked 5.8 hours a day on their farms.

I examine the effect on savings and credit, which are measured at individual level, so standard errors are clustered at household level, as in the previous chapter. I find some effect on the probability of holding any cash savings. In the sample as a whole, treated individuals are 5 percentage points more likely to have some savings (p=0.06) compared to the control group and 4 percentage points compared to the placebo group (although the latter difference is not statistically significant; p=0.12). However, effects on this variable are not robust to controlling for whether the individual had savings at baseline.

The stock of savings is on average 123 ETB higher in the treated group than in the control group (p=0.05) and 124 ETB higher in the treated group than in the placebo group (p=0.04). If one controls for the stock of savings at baseline and for demographic variables, the treatment effect drops to 97 ETB (compared to the control group) and 88 ETB (compared to the placebo group) and is only significant at the 10 per cent level.

In the simplest specification, the treatment group is 3 percentage points more likely than the control group to take a loan (p=0.19) and 4 percentage points more likely than the placebo group (p=0.12), but these differences are not statistically significant. However, with controls for whether the individual had taken out loans at baseline, treatment individuals are 4 percentage points more likely to take out credit than the placebo group (at the 10 per cent level).

The treatment group took out 22 ETB more in credit than the control group (a difference which is significant at the 10 per cent level). They took out 17 ETB more in credit than the placebo group, but differences are not statistically significant (p=0.18). This difference drops to 21 ETB when demographic controls and village fixed effects are added, but remains significant at the 10 per cent level.

Table 5.3 shows results on hypothetical demand for credit. I find positive significant effects of the intervention on the amounts treatment individuals would ask for in ten years compared to both the placebo and the control group. There is no significant effect on individuals who saw the placebo. Overall, results are consistent across the savings and credit variables, pointing to a direct effect of treatment on the use of financial instruments (whether savings or credit) for the treatment group and no effects for the placebo and control groups.

Table 5.2: Treatment effects on behaviour

	Time in farm work			Time in leisure		
Treated individual	7.16	1.83	5.61	5.82	9.99	5.10
	(7.82)	(8.68)	(7.36)	(10.59)	(10.82)	(10.58)
Placebo individual	−1.31	−8.83	−1.94	6.66	14.90	10.95
	(8.30)	(9.38)	(7.99)	(11.23)	(11.56)	(11.20)
Treated-Placebo	8.47	10.66	7.54	−0.85	−4.91	−5.85
	(7.99)	(8.91)	(7.65)	(10.99)	(11.27)	(11.00)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2052	1950	1950	2052	1950	1950
Control mean	310.54	310.54	310.54	798.91	798.91	798.91

	Has savings			Total savings		
Treated individual	0.05*	0.03	0.03	122.56**	106.19*	97.05*
	(0.03)	(0.02)	(0.02)	(61.64)	(54.42)	(52.12)
Placebo individual	0.01	0.01	0.01	−1.84	−13.85	9.02
	(0.02)	(0.02)	(0.02)	(49.38)	(44.85)	(41.48)
Treated-Placebo	0.04	0.02	0.02	124.41**	120.04***	88.02*
	(0.03)	(0.02)	(0.02)	(61.81)	(50.81)	(50.16)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2063	2063	2063	2053	2051	2051
Control mean	0.39	0.39	0.39	182.36	182.36	182.36

	Took out credit			Total credit		
Treated individual	0.03	0.03	0.03	22.35*	19.11	20.70*
	(0.03)	(0.02)	(0.02)	(11.75)	(11.61)	(11.59)
Placebo individual	−0.01	−0.02	−0.02	5.44	3.07	3.29
	(0.03)	(0.02)	(0.02)	(11.74)	(11.52)	(11.47)
Treated-Placebo	0.04	0.04*	0.05*	16.91	16.04	17.41
	(0.03)	(0.03)	(0.03)	(12.68)	(12.43)	(12.35)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2063	2063	2063	2044	2044	2044
Control mean	0.34	0.34	0.34	100.99	100.99	100.99

	Children 7-15 enrolled			Education spending		
Treated individual	0.28***	0.18***	0.21***	60.73***	34.81*	32.99*
	(0.09)	(0.07)	(0.06)	(21.02)	(19.37)	(18.76)
Placebo individual	0.08	0.09	0.10	31.72	18.00	20.20
	(0.09)	(0.07)	(0.07)	(21.16)	(19.56)	(19.17)
Treated-Placebo	0.20***	0.09	0.11*	29.01	16.81	12.78
	(0.09)	(0.07)	(0.07)	(22.39)	(20.61)	(20.73)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Households	1,137	1,137	1,113	1,128	1,128	1,104
Control mean	1.23	1.23	1.23	197.42	197.42	197.42

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

Table 5.3: Treatment effects on hypothetical demand for credit

	Hypothetical demand for credit if loan is repayable in								
	1 year			5 years			10 years		
Treated individual	868.15	1,179.20	506.32	2,127.78	2,419.65	1,296.50	6,211.66*	6,636.43**	5,538.00*
	(1,209.99)	(1,149.54)	(1,158.01)	(2,289.59)	(2,129.20)	(2,145.14)	(3,266.93)	(3,109.23)	(3,173.37)
Placebo individual	1,399.06	1,874.20	1,678.74	−174.15	−764.47	−884.49	−1,424.10	−2,415.88	−2,863.60
	(1,254.64)	(1,197.90)	(1,195.34)	(2,287.53)	(2,130.29)	(2,136.45)	(2,796.91)	(2,593.38)	(2,529.75)
Treated-Placebo	−530.91	−695.00	−1,172.42	2,301.93	3,184.13	2,180.99	7,635.77***	9,052.31***	8,401.60***
	(1,290.58)	(1,232.74)	(1,249.62)	(2,311.24)	(2,185.59)	(2,196.60)	(3,200.38)	(2,987.02)	(3,032.48)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Households	1,131	1,125	1,101	1,126	1,119	1,095	1,114	1,100	1,076
Control mean	10525.73	10525.73	10525.73	22604.28	22604.28	22604.28	33955.50	33955.50	33955.50

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

B. Investment in children's education

There are significant positive effects of the treatment on enrolment: the number of children between 7 and 15 who are enrolled increases by 0.28 in the treatment group compared to the control group (p=0.00) and 0.20 compared to the placebo group (p=0.02). The latter effect, which is the effect solely of the content of the documentary, is a 21 per cent increase from the baseline average of 1.02 children enrolled in school across all groups and 17 per cent larger than the control group average of 1.23 at the time of the endline survey. When the number of children in school at baseline and demographic controls are added, the effect drops to 0.21 compared to the control group (p=0.00) and 0.11 compared to the placebo group, and the difference between treatment and placebo groups is not significant (p=0.11).

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

I also examine spending on schooling for children of all ages still living in the household. In the most basic specification, treated households spend 61 ETB more (31 per cent more) on school expenses than the control group's spending of 197 ETB in the same period, a difference significant at the 1 per cent level. The difference between the treatment and the placebo group is not significant.

When demographic controls and controls for schooling expenditure at baseline are added, treated households spend 33 ETB more on school expenses than the control group did in the same period (17 per cent more), a difference which is significant at the 10 per cent level. There is no significant effect on the placebo group, but the difference between the treatment and placebo groups is not significant. Again, there is some potential for measurement error. Households were asked about their expenditure between September and December 2009 at baseline and the same period in 2010 at endline. So some households had limited time to increase school expenditure after the intervention if they wanted to do so.

These results are also in line with results that the documentaries have the strongest effects on aspirations for children's education. Behavioural measures examine investments in the education of all children in the household, while the aspirations measure asks specifically

about parents' aspirations for their eldest child's education. However, it is likely that an intervention that affects parents' aspirations for the eldest child will also improve aspirations for the education of other children.

There some limited evidence from the developed world that parents with higher self-concept have higher aspirations not only for themselves but for their children's education. Similarly, parents with a more internal locus of control are more likely to believe that they can influence their children's academic trajectories and have higher educational aspirations for them (Bandura et al., 2001). Higher parental aspirations in turn predict children's academic achievement (Bandura et al., 1996; Kao and Tienda, 1998), as does locus of control (Bandura et al., 1996; Coleman and Karraker, 1997; Gross, Fogg, and Tucker, 1995; Schneewind, 1995).

There are a number of possible channels through which these effects could occur. The literature to date has focused on parents' child-rearing practices and influence on their children's aspirations and self-beliefs. Educationally aspirant parents may be more pro-active and successful in cultivating their children's competencies (Bandura et al., 1996). Indeed, British mothers who had high internal locus of control when children were born have children with higher measures of cognitive and emotional development when they reach seven to nine years than mothers with an external locus of control, as they are more likely to be more involved with children and expose children to stimulating activities (Lekfuangfu et al., 2014). Children of mothers with a higher locus of control also have better cognitive development scores in the US (Cunha, Elo, and Culhane, 2013). Parents with high self-concept and aspirations may encourage development of social and self-management skills, so that parents' non-cognitive skills strongly predict their children's non-cognitive skills (Duncan, Boisjoly, and Harris, 2001; de Coulon, Meschi, and Vignoles, 2011).

Another channel is that parents' aspirations for children also influence children's education aspirations (Bandura et al., 1996; Zimmerman, Bandura, and Martinez-Pons, 1992) and occupational preferences (Bandura et al., 2001). There is evidence that children's self-efficacy and locus of control and their aspirations for their education and future occupation are strongly correlated with their subsequent educational and occupational outcomes, in both psychology (Lent and Hackett, 1987; Holland and Gottfredson, 1975; Bandura et al., 2001) and economics (Wydick, Glewwe, and Rutledge, 2013). Evidence from Young Lives across two rounds finds that parental aspirations for children's education are strongly correlated with

children's aspirations for their own education at a later age, and with children's outcomes, controlling for a wide variety of socio-economic indicators (Dercon and Singh, 2013).

These findings for school achievement tend to mirror child and parental aspirations. We observe a striking association be- tween parental and child aspirations, and between aspirations and outcomes, especially in the three countries where we find significant bias by the age of 15 in achievement. We collected parental aspirations when the children were aged 8 and 12 years. Parental aspirations at an early age are biased to- ward boys in India, while in Vietnam they are in favour of girls; by the age of 12 they are biased toward boys in Ethiopia and India. This pattern mirrors child aspirations: by the age of 12 and 15, they are significantly biased in the same direction as parents' aspirations at the age of 8 and 12, with clearly higher aspirations for boys in India and Ethiopia, and for girls in Vietnam. It is, however, also worth noting that the gap be- tween the educational aspirations of boys and girls in Andhra Pradesh, where this gap is widest in our data, is only half as wide as the gap in parental aspirations for their education at

However, another possible channel is that parents with higher self-concept and aspirations are more likely to make future-oriented investments in general, as shown by evidence that they have better educational and labour market outcomes themselves. They may be similarly more successful at making future-oriented investments in their children through their spending on children's education and their choices about whether children attend school. This experiment assesses the effects of changes in parents' aspirations for children and in their self-concept on concrete investments in children, one of the first such experiments to my knowledge in the developing world. In addition, the research mentioned above is based off longitudinal data, which makes it difficult to establish the direction of causal relationships. By altering parents' locus of control and aspirations experimentally, I can examine the effect of exogenous variation in these variables on investments in children's human capital and establish the direction of causation more clearly.

C. Correction for multiple testing

I implement the same multiple testing correction as in the previous chapter. I use the Benjamini and Hochberg (1995) procedure over all outcomes tested in this chapter and the previous chapter, but separately over all treatment effects (compared to the control), all placebo effects (compared to the control) and all treatment effects (compared to the placebo).

Table 5.4: Multiple testing corrections: behaviour

		Treatment		Placebo		Treat - Plac	
		p val	q val	p val	q val	p val	q val
Savings	Has savings	*0.057*	0.170	0.705	0.987	0.123	0.345
	Total savings	*0.047*	0.170	0.970	0.987	0.044	0.213
Credit	Has credit	0.185	0.353	0.777	0.987	0.117	0.345
	Total credit	*0.058*	0.170	0.643	0.987	0.183	0.449
Time allocation	Work time	0.360	0.545	0.875	0.987	0.290	0.609
	Leisure time	0.583	0.702	0.553	0.987	0.939	0.939
Hypothetical loan	1 year	0.473	0.650	0.265	0.967	0.681	0.914
	5 years	0.353	0.545	0.939	0.987	0.319	0.614
	10 years	*0.058*	0.170	0.611	0.987	*0.017*	0.213
Schooling	Children enrolled	**0.001**	0.041	0.325	0.987	*0.023*	0.213
	Ed. spending	**0.004**	0.059	0.134	0.880	0.195	0.462

q-values adjusted for multiple testing are calculated using the Benjamini and Hochberg (1995) procedure. p values in italics lose significance after multiple testing corrections; p values in bold remain significant.

For each outcome variable, Table 5.4 reports the naive p-value from running each estimate independently and the q-value that corrects the p-value for multiple testing. Those p-values that remain significant are in bold, and p-values that lose significance are in italics.

Only results which were significant at the 1 per cent level without multiple testing corrections remain significant when I correct for multiple testing. Thus differences between treatment and control groups on the number of children in the household in school and expenditure on schooling remain significant. However, results on savings and credit are no longer significant (p=0.17 for the amount in savings and in loans), nor are results on hypothetical demand for credit.

IV. Tests for spillovers

Finally, I test for the presence of spillovers within villages. As discussed in the introduction to the previous chapter, observational learning from the experiences of other people is a key influence on the formation of self-concept, self-efficacy and aspirations. Individuals in the treatment, placebo or control groups may be affected by changes in the psychological variables and behaviour of treated individuals.

It is difficult to identify how an individual's peer network affects their outcomes, just as it is difficult to identify the causal effect of any behaviour of a group on the outcomes of a group member. As Manski (1993) highlights, individuals in the same cluster or group may behave similarly because of sorting effects (where individuals form groups of similar peers with similar outcomes), correlated effects (where group members share similar characteristics

or face similar institutional environments or shocks), contextual effects (where behaviour reflects the exogenous characteristics of the group), or endogenous effects (where individuals are influenced by how others in the group behave). It is not possible to separate these effects using observational data.

In addition, tests for spillovers are key to demonstrating that direct treatment effects are cleanly identified. The design is "blocked": treatment, placebo and control status are assigned in equal proportions within each village to give the highest statistical power, but there is no pure control group in villages where no one receives the treatment. If there are no spillover effects or only contextual or correlated effects, this design is optimal, but if there are endogenous effects, the control group's outcomes will be contaminated by the effects of exposure to treated individuals.

A. Using variation in village-level treatment intensity

Previous research uses exogenous variations in group composition (Yang, 2007; Kling, Liebman, and Katz, 2007; Sacerdote, 2001) or partial population interventions that affect only some peers within a group (Bobonis and Finan, 2009; Duflo and Saez, 2003) to identify endogenous interaction effects. Here, I use a Randomised Saturated (RS) design (Baird et al., 2012), which nests a partial population approach. In the first stage, clusters were randomised to two treatment saturations: an additional 18 households (36 individuals) per village were invited to documentaries in half of the villages and an additional 18 households (36 individuals) per village were invited to placebo screenings in the other half. In the second stage, individuals in clusters were assigned treatment status. There are thus two levels of village treatment intensity: 6 households (12 individuals) were targeted in low-intensity villages, while 24 households (48 individuals) were targeted in high-intensity villages. The average village size was roughly 75 households. Only villages with 50-100 households were sampled to minimise effects of village size.

A.1 Specification

The specification used is in Equation 5.1. The dummy variable I_v is equal to one for villages where an additional 36 individuals were invited to a documentary session and zero for villages where the additional 36 individuals were invited to a placebo session. Screening site-level fixed effects μ_s account for site-specific characteristics (Equation 5.1 uses village fixed

effects, but here treatment is at village level). Standard errors are clustered at village level to account for non-independence of observations within village due to village-level treatment.[2]

$$y_{i2} = \alpha + \delta_2 T_i + \rho_2 P_i + \lambda_1 I_v + \lambda_2 I_v * T_i + \lambda_3 I_v * P_i + \mu_s + \eta_i \tag{5.1}$$

λ_1 tests for the presence of spillover effects if a number of assumptions are made (Baird et al., 2012). First, RS designs relax the stable unit treatment value assumption (SUTVA) within clusters, but maintain it across clusters. Such designs focus on spillovers between individuals in the same village. Villages are about 30 minutes' walk apart and individuals' networks of close friends were mostly in the same village, as discussed below, so spillovers between clusters are less likely. RS designs must be implemented in non-overlapping networks.

Second, if the experiment uses within-cluster controls to form the counter-factual, as in this experiment, and one wishes to identify spillovers, one must make a functional form assumption on the saturation distribution. In the ideal RS design, there are some "pure control" clusters where no individuals are treated (where the treatment saturation is zero). These clusters give the desired counter-factual outcome of untreated individuals who are not affected by spillovers.

However, in studies with no pure control group, the RS design can still test for the presence of spillovers. λ_1 captures variation in the control group's outcomes across the two saturations, low intensity treatment villages and high intensity treatment villages. If there is no variation in the control group's outcome across saturations $(\lambda_1 = 0)$, one can argue that there are no spillovers on the within-village control group at those saturations and that the within-village control group approximate pure controls. λ_2 and λ_3 allow that spillovers may differ by treatment group.[3] Finally, for this design to identify cleanly the individual treatment and placebo effects, one must assume that there are no spillover effects from watching the placebo.

[2]We do not use the usual Liang and Zeger (1986) clustered standard errors as these can be unreliable if there are fewer than about 100 clusters, and we have 64 villages. We therefore base inference on a t distribution with g – k degrees of freedom, where g is the number of groups, rather than on the standard normal distribution (Cameron, Gelbach, and Miller, 2008; Donald and Lang, 2007). Importantly, the coefficients δ_2 and ρ_2 do not have the same interpretation in these regressions as the coefficients δ_1 and ρ_1 in Equation 4.1. For example, δ_2 is the effect of being treated for individuals in intense-placebo villages. For treated individuals in intense-treatment villages, the treatment effect is $\delta_2 + \lambda_1 + \lambda_2$. The weighted average of these two coefficients is δ_1, the treatment effect in Equation 4.1.

[3] If there are spillovers, one can assume that the outcome variable is linear with respect to treatment saturation and that the spillover effect is continuous at zero and correct the ITT for these spillovers, but this is not necessary here.

A.2 Results

Detailed results are given in Appendix D. Table 5.5 summarises results. I give the sign of each coefficient, the naive p-value, and the q-value after accounting for multiple testing. I use the same Benjamini and Hochberg (1995) procedure over all outcomes, but run the procedure separately for each coefficient, i.e. for all treatment effects (compared to the control), all placebo effects (compared to the control), all treatment village effects, and so on. The p-values that lose significance are in italics. Although some individual coefficients measuring spillovers are significant, none survives corrections for multiple testing, so there is little evidence that there are spillovers when these are measured via variation in village-level treatment intensity.

B. Using variation in exposure of individuals' networks to treatment

Few spillovers within villages could mask the presence of stronger spillovers within social networks (Baird et al., 2012). Furthermore, in the presence of threshold effects, it is unclear village-level variations in treatment intensities lie on either side of a given threshold. If one treated individual in a village is enough to produce spillover effects, the village-level design would not identify them.

To address this limitation, the survey also captured variation in the exposure of a respondent's own network to treatment, as in Baird et al. (2012). At baseline, each surveyed individual listed their four closest friends (a small number of peers was chosen to avoid potential biases related to the size of one's social network). These were matched to the lists of invitees to capture how many of a respondent's friends were invited to the documentary and placebo. In fact, 99 per cent of respondents did cite four peers. Among them, 93 per cent cited four individuals who lived in the same village, which is unsurprising given the remoteness of these communities. This supports the assumption of the RS design that spillovers are unlikely to occur between clusters. Only 14 per cent of the respondents listed any siblings among the four individuals, so peer effects cannot be fully explained by family characteristics.

With imperfect correlation between one's village and one's social network, the RS design generates exogenous variation in treatment intensity within each individual's social network (Baird et al., 2012). Table 5.6 shows that, in intense-treatment villages, more of the respondent's peers had seen the documentary; in intense-placebo villages, more of the respondent's peers had seen the placebo. However, a person in an intense-placebo village may have four peers who are treated, or a person in an intense-treatment village may have none. The measure is

Table 5.5: Tests for spillovers using variation in village-level treatment intensities

	After 6 months		Locus of control			Causes of poverty				Life satisfaction			Risk aversion	
	Expectations	Aspirations	Chance	Others	Internal	Fate	Structural	Individual	Self-esteem	Best	Happiest	Discount	Coin	Market
Individual treatment					+			+++	++					
p-val	0.15	0.95	0.90	0.61	*0.08*	0.27	0.56	*0.00*	*0.04*	0.65	0.40	0.80	0.63	1.00
q-val	0.54	0.99	0.98	0.90	0.49	0.74	0.90	0.12	0.34	0.90	0.90	0.94	0.90	1.00
Individual placebo														
p-val	0.55	0.74	0.71	0.56	0.79	0.11	0.54	0.11	0.41	0.38	0.62	0.28	0.78	0.60
q-val	0.82	0.82	0.82	0.82	0.82	0.62	0.82	0.62	0.82	0.82	0.82	0.79	0.82	0.82
Village-level treatment									++					
p-val	0.73	0.63	0.68	0.74	0.97	0.18	0.95	0.92	*0.02*	0.51	0.91	0.83	0.92	0.89
q-val	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.43	0.98	0.98	0.98	0.98	0.98
Individual treatment*		+							−					
Village-level treatment														
p-val	0.49	*0.08*	0.66	0.33	0.39	0.99	0.47	0.23	*0.05*	0.73	0.82	0.88	0.39	0.47
q-val	0.77	0.63	0.97	0.77	0.77	0.99	0.77	0.77	0.63	0.99	0.99	0.99	0.77	0.77
Individual treatment*						++			−					
Village-level placebo														
p-val	0.60	0.36	0.77	0.44	0.90	*0.04*	0.38	0.79	*0.07*	0.30	0.43	0.79	0.85	0.60
q-val	0.79	0.77	0.86	0.77	0.90	0.46	0.77	0.86	0.56	0.77	0.77	0.86	0.89	0.79

							Hypothetical credit				
	Work time	Leisure time	Has savings	Total savings	Has credit	Total credit	1 year	5 years	10 years	Children enr.	Ed. spending
Individual treatment											++
p-val	0.12	0.83	0.78	0.37	0.81	0.21	0.11	0.03	0.61	0.45	0.54
q-val	0.49	0.94	0.94	0.90	0.94	0.65	0.49	0.34	0.90	0.90	0.90
Individual placebo											+
p-val	0.60	0.39	0.16	0.47	0.97	0.17	0.17	0.06	0.66	0.11	0.22
q-val	0.82	0.82	0.62	0.82	0.97	0.62	0.62	0.62	0.82	0.62	0.68
Village-level treatment	++										
p-val	0.03	0.40	0.93	0.98	0.58	0.64	0.60	0.23	0.89	0.23	0.43
q-val	0.43	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98
Individual treatment*											
Village-level treatment								+			
p-val	0.17	0.94	0.22	0.97	0.46	0.97	0.38	0.92	0.18	0.05	0.31
q-val	0.77	0.99	0.77	0.99	0.77	0.99	0.77	0.99	0.77	0.63	0.77
Individual treatment*							+	++			
Village-level placebo											
p-val	0.36	0.53	0.12	0.49	0.68	0.13	0.48	0.26	0.09	0.03	0.46
q-val	0.77	0.78	0.56	0.77	0.86	0.56	0.77	0.77	0.56	0.46	0.77

+ indicates a positive, significant effect with naive p value <0.1. ++ indicates a positive, significant effect with naive p value <0.05. +++ indicates a positive, significant effect with naive p value <0.1. Negative effects are indicated with minus signs. p values in italics lose significance after multiple testing corrections; p values in bold remain significant.

Table 5.6: Number of people in the treatment and placebo groups among the respondent's four closest friends

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

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

only a partial measure of an individual's exposure to treatment as it captures only four peers. Individuals may talk to many more people directly or indirectly exposed to the documentary, something better accounted for by the village-level treatment intensity variable.

B.1 Specification

In Equation 5.2, n_i^T measures the number of the respondent's four closest friends who were invited to a documentary session, while n_i^P measures the number invited to a placebo screening. The probability of having a given number of peers treated is in part determined by village-level treatment intensities, as shown in Table 5.6. Thus δ_3 measures the treatment effect on those who were treated themselves but have no peers treated (and no peers who watched the placebo). δ_4 is the effect of each additional peer being treated on an individual's outcome for individuals in the control group (who had no peers who watched the placebo). For individuals who were both treated and had peers who were treated, the treatment effect is $\delta_3 + \delta_4 n_i^T + \delta_5 n_i^T$ (if they had no peers who watched the placebo). I also include analogous controls for individuals who saw the placebo and the number of peers invited to placebo screenings:

$$y_{i2} = \alpha + \delta_3 T_i + \rho_3 P_i + \delta_4 n_i^T + \delta_5 T_i * n_i^T + \delta_6 P_i * n_i^T + \rho_4 n_i^P + \rho_5 P_i * n_i^P + \rho_6 T_i * n_i^P + \mu_s + \eta_i \quad (5.2)$$

B.2 Results

Tables 5.7 and 5.8 give the sign of each coefficient, the naive p-value, and the q-value after accounting for multiple testing. Detailed results are given in Table Appendix D. To correct for multiple testing, I use the same Benjamini and Hochberg (1995) procedure over all outcomes, but run the procedure separately for each coefficient. The p-values that lose significance are in italics, while those that remain significant are in bold. Although some individual coefficients measuring spillovers are significant, none survive corrections for multiple testing, so there is little evidence that there are spillovers when these are measured via exogenous variation in the intensity of treatment in individuals' networks of peers.

V. Conclusion

This chapter assesses the reduced-form effect of a short, consistently administered intervention on individuals' behaviour. The intervention exposed individuals to documentaries about people from villages like them who had succeeded in improving their economic circumstances through innovations in agriculture or small business. In the previous chapter, I show that exposure to the documentaries increased the extent to which participants believe that individual actions, rather than fate, determine outcomes; increased aspirations and expectations for children's education; and had some effects on self-esteem. The experiment can identify a link between watching the documentary and changes in psychological variables, and between watching the documentary and changes in future-oriented behaviour. However, I do not argue that changes in psychological variables cause changes in future-oriented behaviour, but merely that an intervention which altered psychological variables also caused changes in behaviour, so there is suggestive evidence that the behavioural changes were due to psychological changes.

In this chapter, I present consistent evidence of large and significant effects on investments in children's education. The number of children aged 7 to 15 enrolled in school was 0.28 larger in the treatment group compared to the control group and 0.20 larger compared to the placebo group. The latter effect is a 21 per cent increase from the baseline average of 1.02 children enrolled in school across all groups and 17 per cent larger than the control group average of 1.23 at the time of the endline survey. The Cohen's d effect size is 0.18 (the standard deviation at baseline is 1.11). A meta-analysis of 34 studies of interventions to improve enrolment in developing countries, such as school-building, providing resources like

Table 5.7: Tests for spillovers on psychological variables using network measures

	After 6 months		Locus of control			Causes of poverty				Life satisfaction			Risk aversion	
	Expectations	Aspirations	Chance	Others	Internal	Fate	Structural	Individual	Self-esteem	Best	Happiest	Discount	Coin	Market
Treated individual	+++	+++				– –					++			
p-val	**0.00**	**0.00**	0.60	0.48	0.94	*0.05*	0.90	0.61	0.37	0.64	**0.02**	0.69	0.49	0.70
q-val	0.02	0.03	0.92	0.88	0.95	0.17	0.95	0.92	0.77	0.92	0.09	0.92	0.88	0.92
Placebo individual			++											
p-val	0.34	0.36	*0.05*	0.18	0.15	0.67	0.45	0.59	0.42	0.60	0.19	0.33	0.96	0.36
q-val	0.70	0.70	0.68	0.68	0.68	0.83	0.71	0.83	0.70	0.83	0.68	0.70	1.00	0.70
Peers treated			–						+					
p-val	0.10	0.26	0.72	0.40	0.33	0.07	0.40	0.92	*0.08*	0.36	0.42	0.68	0.93	0.72
q-val	0.65	0.75	0.86	0.75	0.75	0.65	0.75	0.93	0.65	0.75	0.75	0.86	0.93	0.86
Peers placebo			++								+			
p-val	0.37	0.68	*0.03*	0.61	0.16	0.34	0.68	0.39	0.67	0.46	*0.05*	0.17	0.42	0.85
q-val	0.71	0.80	0.21	0.80	0.61	0.71	0.80	0.71	0.80	0.73	0.26	0.61	0.71	0.92
Treated*Peers treated														–
p-val	0.18	0.73	0.49	0.69	0.73	0.14	0.52	0.85	0.30	0.79	0.87	0.78	0.12	*0.06*
q-val	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98
Treated*Peers placebo	– – –	–									–			
p-val	*0.01*	*0.05*	0.29	0.54	0.14	0.73	0.38	*0.08*	0.45	0.74	*0.03*	0.52	0.20	0.89
q-val	0.14	0.25	0.60	0.79	0.40	0.84	0.67	0.25	0.75	0.84	0.19	0.79	0.50	0.90
Placebo*Peers treated														
p-val	0.53	0.65	0.31	0.44	0.37	0.42	0.30	0.83	0.90	0.80	0.26	0.59	0.76	0.96
q-val	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.96
Placebo*Peers placebo			– – –	–		–					–			
p-val	0.76	0.81	**0.00**	0.06	0.15	*0.07*	0.66	0.13	0.46	0.39	*0.06*	0.54	0.55	0.56
q-val	0.83	0.85	0.06	0.25	0.31	0.25	0.79	0.31	0.74	0.74	0.25	0.74	0.74	0.74

+ indicates a positive, significant effect with naive p value <0.1. ++ indicates a positive, significant effect with naive p value <0.05. +++ indicates a positive, significant effect with naive p value <0.1. Negative effects are indicated similarly with minus signs. p values in italics lose significance after multiple testing corrections; p values in bold remain significant.

Table 5.8: Tests for spillovers on behaviour using network measures

							Hypothetical credit				
	Work time	Leisure time	Has savings	Total savings	Has credit	Total credit	1 year	5 years	10 years	Children enr.	Ed. spending
Treated individual			+	++			+	++	++		+
p-val	0.95	0.87	*0.06*	*0.03*	0.93	0.77	*0.08*	*0.05*	**0.01**	0.31	0.08
q-val	0.95	0.95	0.19	0.16	0.95	0.95	0.20	0.17	0.09	0.71	0.20
Placebo individual	++										
p-val	0.31	0.39	0.16	0.99	0.99	0.88	0.13	0.37	0.18	0.69	0.68
q-val	0.70	0.70	0.68	1.00	1.00	1.00	0.68	0.70	0.68	0.83	0.83
Peers treated											
p-val	*0.04*	0.77	0.79	0.27	0.57	0.64	0.21	0.62	0.26	0.75	0.14
q-val	0.65	0.86	0.86	0.75	0.86	0.86	0.75	0.86	0.75	0.86	0.69
Peers placebo							++	+++	++		
p-val	0.70	0.61	0.39	0.40	0.94	0.37	*0.02*	*0.00*	*0.03*	0.90	0.31
q-val	0.80	0.80	0.71	0.71	0.94	0.71	0.21	0.10	0.21	0.94	0.71
Treated*Peers treated											
p-val	0.99	0.66	0.41	0.86	0.53	0.54	0.94	0.93	0.53	0.23	0.89
q-val	0.99	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98
Treated*Peers placebo				−			− −	− −	−		
p-val	0.58	0.82	0.23	*0.07*	0.64	0.37	*0.02*	*0.01*	*0.08*	0.89	0.66
q-val	0.80	0.89	0.51	0.25	0.83	0.67	0.14	0.14	0.25	0.90	0.83
Placebo*Peers treated											
p-val	0.75	0.31	0.53	0.44	0.74	0.54	0.87	0.79	0.34	0.27	0.15
q-val	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93
Placebo*Peers placebo						+	−	− −	−		
p-val	0.15	0.73	0.12	0.60	0.89	*0.08*	*0.05*	*0.03*	*0.06*	0.56	0.50
q-val	0.31	0.83	0.31	0.75	0.89	0.25	0.25	0.25	0.25	0.74	0.74

+ indicates a positive, significant effect with naive p value <0.1. ++ indicates a positive, significant effect with naive p value <0.05. +++ indicates a positive, significant effect with naive p value <0.1. Negative effects are indicated similarly with minus signs. p values in italics lose significance after multiple testing corrections; p values in bold remain significant.

textbooks and uniforms or conditional cash transfers, found that the average effect size was also 0.18 (Petrosino et al., 2012). Although many effect sizes in the review were measured after a 12 or 24 month follow-up, rather than six months, as in this study, this nonetheless indicates that the effects of the documentary on education investments are substantial. This intervention was extremely cost-effective compared to other interventions to improve enrolment, so similar interventions might have important potential for efforts to improve enrolment and attendance in schools in developing countries.

Households in the treatment group also spent more on education than the control group. At endline the control group spent 197 ETB and treated households spent 61 ETB more (31 per cent more), a difference significant at the 1 per cent level. The difference between the treatment and placebo groups is positive but not statistically significant. The difference between the treatment and control group is robust to corrections for multiple testing on both variables (although the difference between the treatment and placebo group is not), indicating that this is a relatively robust result. It is also striking that exposure to the documentary changed parents' educational aspirations, as well as their locus of control, which is correlated with investment in children's education in the developed world. This provides further support for the argument that there is a clear link between changes in self-belief and aspirations and changes in behaviour. Short, carefully designed, cost-effective interventions can build up individuals' beliefs in their own competence, ability to act and opportunities and can also alter behaviour in the short term. Furthermore, changes in these psychological variables and in behaviour can have intergenerational effects.

Exposure to the documentaries directly affected individuals' use of financial tools. There are small changes in savings and the use of credit, although individuals in this deprived area have limited stocks of savings and use of loans to begin with, so changes are large in relative terms. The control group had 182 ETB saved at the time of Round 2. The treated group had 123 ETB more (68 per cent more) saved than the control group and 124 ETB more saved than the placebo group. The control group had 101 ETB in loans at the time of the Round 2 survey, while the treated group had taken out 22 ETB more (22 per cent more), a difference which is significant at the 10 per cent level. The differences between the treatment and placebo groups are not significant. However, these results are not robust to corrections for multiple testing. Further research in a larger sample is necessary to test if these results would be large enough to withstand multiple testing corrections.

I find no strong evidence of spillovers from the treatment to other groups on either psychological outcomes or behaviour. Although there are some individual significant coefficients in tests for spillovers, none of these are robust to corrections for multiple testing. This is unsurprising, given that the intervention was relatively short and light-touch and that the follow-up is after a short period of time, so people who did not see the documentary might not have had time to observe changes in other people. This means that treatment effects, even from a blocked experimental design, are likely to be cleanly identified.

This experiment raises two promising directions for new research. One is to examine whether more sustained, intensive psychological interventions may have larger and longer-lasting effects. Another is to explore the effects of adding such psychological interventions to existing anti-poverty programmes already known to reduce poverty and improve savings, asset accumulation and investment in education. Changing people's beliefs about themselves and their future does not reduce the severe material constraints they face. Participants in this intervention may have increased savings by 68 per cent, but they held only 5 USD in savings. However, if people both change their beliefs and receive a programme that also lifts material constraints, the combined intervention may have larger and more lasting effects on poverty than either intervention on its own.

A

Summary of documentaries and placebo

The treatment consisted of four documentaries about two men and two women. Two documentaries are described below. Two documentaries are not summarised here, "Immortal Treasure", about Ayelech Fikre, and "The Exemplary Achievement", about Waki Feyyera. The four documentaries and an example of the placebo segments are available at:

https://www.youtube.com/channel/UCqfoNjCzt8YPjTRWQaMQfAg.

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

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

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

Beshir Malim Yisak is a farmer living 658 kilometres south of Addis Ababa. He is 27 years old, married, with two children. He has no formal education but is considered a model farmer in the area for his considerable achievement in a short period of time. Five years ago, in an area where most of the inhabitants usually breed cattle, Beshir started crop production. He consulted an agricultural expert in a local NGO about good farming practices and implemented everything he learned. He started planting vegetables, which he sold at the market, and bought a pair of oxen after a good harvest. Three years later, Beshir used money he had saved to purchase a water pump from Addis Ababa, with the help of the agricultural expert. Beshir was able to water a larger area with his pump than with buckets, so he rented additional land to expand his farm. He started planting papaya, sugarcane and maize and increased his productivity by improving his soil fertility. He gradually built up a large herd of cattle. He started keeping bees for honey and producing tree seedlings for sale. During 2007, when tree planting was encouraged by village administrations, he produced and distributed seedlings to seven peasant associations and a local NGO in the area. Extension agents and fellow farmers speak of him as an innovator and hard worker.

III. Example segment from placebo treatment

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

B

Validity of measures

Table B.1: Cronbach's alpha for locus of control, causes of poverty and self-esteem scales

	Items	Cronbach's alpha	n
Locus of control			
Internal	5	0.724	2,052
When I make plans, I am almost certain/guaranteed/sure to make them work			
I can mostly determine what will happen in my life			
I am usually able to protect my personal interests			
When I get what I want, it is usually because I worked hard for it			
My life is determined by my own actions			
Powerful others	5	0.744	2,045
I feel like what happens in my life is mostly determined by powerful people			
My life is chiefly controlled by other powerful people			
In order to have my plans work, I make sure that they fit in with the desires of people who have power over me			
People like myself have very little chance of protecting our personal interests when they conflict with those of more powerful people			
Getting what I want requires making those people above me (people with higher status) happy with me			
Chance	5	0.676	2,030
When I get what I want, it is usually/mostly because I'm lucky			
Often there is no chance of protecting my personal interests from bad luck happenings			
To a great extent my life is controlled by accidental/chance happenings			
It is not always wise for me to plan too far ahead because many things turn out to be a matter of good or bad fortune			
My experience in my life has been that what is going to happen will happen			
Perceptions of causes of poverty			
Individual	4	0.703	2,047
They lack the ability to manage money or other assets			
They waste their money on inappropriate items (e.g. alcohol, cigarettes, gambling)			
They do not actively seek to improve their lives			
They are not motivated because of food aid (e.g. direct support programme, food parcels)			
Structural	4	0.626	2,049
They are exploited by rich people			
Society fails to help and protect the most vulnerable			
The distribution of land between poor and rich people is uneven/unequal			
They lack opportunities due to the fact that they come from poor families			

Table B.1: Cronbach's alpha for locus of control, causes of poverty and self-esteem scales

	Items	Cronbach's alpha	n
Fate	3	0.898	2,037
They lack luck			
They have encountered misfortunes			
They have bad fate/destiny			
Self-esteem	6	0.572	2,000
I feel that I have a number of good qualities			
All in all, I am inclined to feel that I am a failure			
I am able to do things as well as most people			
I feel I do not have much to be proud of			
On the whole I am satisfied with myself			
I wish I could have more respect for myself than I have now			

Table B.2: Weights to capture the subjective importance of dimensions of life

	Mean	Treatment	Placebo	Control	T–C	P–C	T–P	N
Income	5.51	5.59	5.41	5.53	0.07	−0.12	0.19**	2,063
	(1.63)	(1.71)	(1.43)	(1.74)	(0.10)	(0.09)	(0.09)	
Wealth	4.83	4.79	4.92	4.78	0.01	0.15*	−0.14*	2,063
	(1.40)	(1.38)	(1.31)	(1.50)	(0.08)	(0.08)	(0.07)	
Education	4.38	4.27	4.47	4.39	−0.12	0.08	−0.2	2,063
	(1.44)	(1.53)	(1.41)	(1.39)	(0.08)	(0.08)	(0.08)	
Social status	5.05	5.02	5.07	5.05	−0.03	0.01	−0.05**	2,063
	(1.81)	(1.90)	(1.71)	(1.82)	(0.10)	(0.10)	(0.10)	

*p below 0.1, **p below 0.05, ***p below 0.01. Robust standard errors clustered at household level are in parentheses. The T–C difference and P–C difference are the treatment and placebo dummies run on the baseline data.

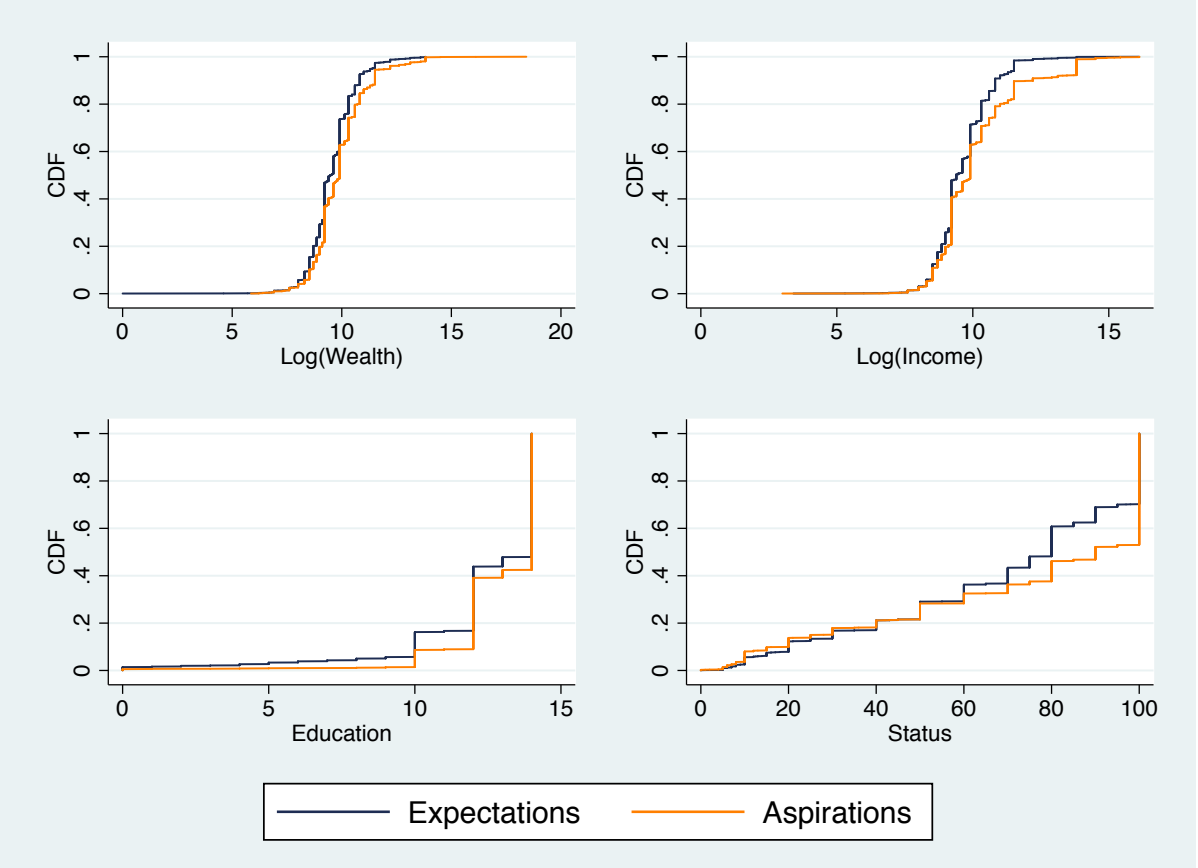
Table B.3: Correlations at baseline between psychological measures and aspirations and expectations

	Asp.					Exp.				
	Index	Income	Wealth	Ed.	Status	Index	Income	Wealth	Ed.	Status
Aspirations: level you'd like to achieve										
Index	1.00									
Income	0.57***	1.00								
Wealth	0.51***	0.02	1.00							
Ed.	0.48***	−0.00	0.03	1.00						
Status	0.49***	0.12***	0.03	0.08***	1.00					
Expectations: level you think you'll reach in 10 years										
Index	0.33***	0.07***	0.02	0.28***	0.34***	1.00				
Income	0.02	0.06**	0.00	0.01	−0.04*	0.48***	1.00			
Wealth	0.10***	0.03	0.07***	0.09***	0.03	0.61***	0.05**	1.00		
Ed.	0.25***	−0.02	−0.03	0.49***	0.10***	0.53***	0.03	0.08***	1.00	
Status	0.39***	0.11***	0.01	0.08***	0.79***	0.44***	−0.02	0.08***	0.09***	1.00

			Locus of control			Causes of poverty				Life satis.			Risk	
	Asp.	Exp.	Int.	Others	Chance	Indiv.	Struct.	Fate	Self-est.	Best	Happiest	Discount	Coin	Market
Asp.	1.00													
Exp.	0.33***	1.00												
Locus of control														
Internal	0.10***	0.15***	1.00											
Others	0.00	−0.03	0.04	1.00										
Chance	−0.02	−0.02	−0.22***	0.59***	1.00									
Individual	0.00	0.07***	0.29***	−0.01	−0.08***	1.00								
Causes of poverty														
Structural	0.01	−0.06***	−0.09***	0.17***	0.19***	0.26***	1.00							
Fate	−0.06***	−0.09***	−0.31***	0.29***	0.49***	0.06***	0.43***	1.00						
Self-esteem	−0.04*	0.03	0.20***	−0.27***	−0.24***	0.18***	−0.15***	−0.19***	1.00					
Life satisfaction														
Best	0.01	0.13***	0.09***	−0.06***	−0.09***	0.10***	−0.08***	−0.05**	0.28***	1.00				
Happiest	0.21***	0.20***	0.13***	−0.01	−0.05**	−0.08***	−0.06***	−0.09***	0.07***	0.31***	1.00			
Discount	−0.14***	−0.15***	−0.15***	−0.01	−0.01	−0.05**	0.01	−0.00	−0.06***	0.01	−0.14***	1.00		
Risk aversion														
Coin	−0.04*	−0.04*	−0.15***	0.02	0.08***	−0.13***	−0.06***	0.03	0.01	−0.09***	−0.05**	0.01	1.00	
Market	−0.12***	−0.10***	−0.13***	−0.02	0.06***	−0.16***	−0.06**	0.02	−0.02	−0.06***	−0.10***	0.00	0.51***	1.00

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

Figure B.1: Cumulative distribution functions: dimensions of aspirations and expectations



The graphs show the cumulative distribution function of each separate dimension of expectations (in black) and aspirations (in grey) at baseline. I log the income and wealth measures for ease of display, but the measures are not logged when used in the index.

Table B.4: Structure of payoffs in risk aversion lotteries

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

Columns 2 and 3 give the payout options. Columns 4 and 5 give the mean and variance of each lottery. The successive lotteries offered increase in both mean and variance, so effectively payouts were ordered from most to least risk averse. Column 8 shows the range for the coefficient of partial risk aversion, calculated from the CPR that corresponds to each of the five payouts. For Column 9, I follow how Hill et al. (2009) scale this coefficient to assign risk aversion "numbers" to the discreet classes. For options 2-4, I take the geometric mean of the endpoints as the coefficient: as the interval length decreases the alternatives get more risky. For option 1, the upper bound for the coefficient is infinity and the lower bound is 3.26. Only 12 per cent of individuals chose this option, so their partial risk aversion is unlikely to exceed 3.26 by much, so I allocated them a value of 3.26. For option 5, which has an endpoint of 0 (assuming no respondent is risk loving), I use the arithmetic mean.

Table B.5: Baseline correlates of beliefs, aspirations and expectations

	Current minimum others can achieve				Current maximum others can achieve			
	Income	Wealth	Ed.	Status	Income	Wealth	Ed.	Status
Age	−0.10	−0.02	−0.00	0.02	−118.67**	−83.59	−0.00	0.05
	(2.65)	(2.06)	(0.00)	(0.03)	(52.15)	(70.11)	(0.01)	(0.05)
Male	61.70	−42.39	0.05	0.11	973.81	1,566.38	0.31**	2.12**
	(61.01)	(48.03)	(0.05)	(0.61)	(822.04)	(1,844.64)	(0.13)	(1.01)
Assets ('000 ETB)	7.89	10.39**	−0.00	−0.01	179.23***	388.66***	0.00	−0.04
	(5.44)	(4.34)	(0.00)	(0.04)	(50.00)	(128.16)	(0.01)	(0.06)
Highest grade	2.49	15.36	0.01	0.01	−126.08	1,239.76**	0.05*	0.15
	(13.32)	(11.83)	(0.01)	(0.15)	(205.19)	(532.14)	(0.03)	(0.24)
Single	−170.32	−202.32***	−0.10**	2.67*	118.66	−1,181.68	−0.05	3.65
	(119.01)	(75.74)	(0.04)	(1.38)	(1,242.46)	(2,254.15)	(0.25)	(2.29)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2,003	1,993	1,989	2,009	2,041	2,039	2,031	2,042

	Aspirations					Expectations				
	Index	Income	Wealth	Ed.	Status	Index	Income	Wealth	Ed.	Status
Age	0.00	−911.83	3,812.52	0.00	0.13**	−0.00**	−1,043.25	−170.82*	−0.02***	0.12**
	(0.00)	(1,339.91)	(2,918.19)	(0.00)	(0.06)	(0.00)	(757.68)	(102.84)	(0.01)	(0.05)
Male	0.16***	92,559.38***	37,820.79	0.36***	8.14***	0.07**	20,423.37	−4,805.70	0.17	6.90***
	(0.02)	(26,102.26)	(65,944.87)	(0.08)	(1.15)	(0.04)	(24,448.72)	(5,124.94)	(0.11)	(1.09)
Assets ('000 ETB)	0.00*	5,089.10**	2,881.00	0.00	−0.11*	0.01***	558.09	2,535.58***	0.02***	−0.07
	(0.00)	(2,479.02)	(4,830.84)	(0.00)	(0.07)	(0.00)	(1,143.60)	(810.78)	(0.01)	(0.06)
Highest grade	0.02***	486.91	70,974.69*	0.10***	0.89***	0.03***	819.75	3,074.77***	0.08***	1.20***
	(0.01)	(4,306.63)	(42,387.84)	(0.01)	(0.28)	(0.01)	(2,034.04)	(1,125.50)	(0.02)	(0.26)
Single	−0.03	−49,276.79*	−24,193.14	−0.14	−1.00	−0.03	−2,524.98	−8,353.34**	−0.35	1.46
	(0.04)	(29,179.69)	(62,172.06)	(0.22)	(2.52)	(0.04)	(7,396.76)	(3,541.16)	(0.33)	(2.30)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2,058	2,047	2,049	2,015	2,039	2,055	2,045	2,049	1,936	2,040

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

Table B.6: Baseline correlates of psychological outcomes

	Locus of control			Causes of poverty				Life satisfaction	
	Chance	Others	Internal	Fate	Structural	Individual	Self-esteem	Best life	Happiest life
Age	0.02***	0.00	−0.01	0.02***	0.00	−0.00	−0.01**	0.00	−0.01**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Male	−0.14	−0.21	0.07	−0.18	−0.02	−0.04	−0.08	−0.67***	0.03
	(0.12)	(0.14)	(0.11)	(0.16)	(0.03)	(0.03)	(0.10)	(0.09)	(0.11)
Assets ('000 ETB)	−0.02**	−0.02*	0.02**	−0.01	0.00	0.01**	0.04***	0.07***	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Highest grade	−0.09***	−0.04	0.12***	−0.14***	−0.02***	0.01	0.10***	0.07***	0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Single	0.01	0.13	−0.10	−0.57*	0.01	−0.06	−0.36*	−0.84***	−0.10
	(0.24)	(0.25)	(0.27)	(0.32)	(0.05)	(0.06)	(0.19)	(0.16)	(0.22)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	2,030	2,045	2,052	2,049	2,049	2,052	2,043	2,055	2,037

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

C

Robustness checks

Table C.1: Treatment effects on psychological outcomes and preferences

Locus of control	Chance			Others			Internal		
Treated	−0.05	−0.06	−0.00	−0.07	−0.07	−0.05	0.30**	0.29**	0.28*
	(0.17)	(0.16)	(0.16)	(0.18)	(0.18)	(0.18)	(0.15)	(0.15)	(0.14)
Placebo	−0.00	−0.02	−0.01	0.04	0.02	0.02	−0.09	−0.08	−0.08
	(0.16)	(0.16)	(0.15)	(0.17)	(0.17)	(0.17)	(0.15)	(0.15)	(0.14)
Treated-Placebo	−0.05	−0.04	0.01	−0.12	−0.09	−0.07	0.39***	0.37***	0.36***
	(0.16)	(0.16)	(0.16)	(0.17)	(0.17)	(0.17)	(0.15)	(0.15)	(0.15)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2,039	2,008	2,008	2,052	2,035	2,035	2,060	2,049	2,049
Control mean	13.35	13.35	13.35	12.62	12.62	12.62	16.05	16.05	16.05
Causes of poverty	Fate			Structural			Individual		
Treated individual	−0.43**	−0.43**	−0.39*	−0.00	0.02	0.05	0.41**	0.40**	0.40**
	(0.20)	(0.21)	(0.20)	(0.15)	(0.15)	(0.15)	(0.16)	(0.16)	(0.16)
Placebo individual	0.01	−0.00	−0.00	0.27*	0.28*	0.28*	0.31*	0.32**	0.33**
	(0.20)	(0.21)	(0.20)	(0.15)	(0.15)	(0.15)	(0.16)	(0.16)	(0.16)
Treated-Placebo	−0.44**	−0.43**	−0.39*	−0.28*	−0.26	−0.23	0.10	0.08	0.08
	(0.20)	(0.21)	(0.20)	(0.15)	(0.15)	(0.15)	(0.16)	(0.16)	(0.16)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2,063	2,049	2,049	2,063	2,049	2,049	2,063	2,052	2,052
Control mean	9.85	9.85	9.85	10.36	10.36	10.36	11.47	11.47	11.47
	Self-esteem			Best life			Happiest life		
Treated	0.17	0.17	0.16	0.06	0.04	0.03	0.24*	0.23*	0.22*
	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)	(0.12)	(0.12)	(0.12)
Placebo	−0.08	−0.08	−0.07	0.03	0.09	0.09	−0.02	−0.01	−0.00
	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)	(0.13)	(0.13)	(0.12)
Treated-Placebo	0.25**	0.24**	0.23*	0.03	−0.05	−0.06	0.26**	0.24**	0.22*
(se)	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)	(0.12)	(0.12)	(0.12)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2,058	1,995	1,995	2,063	2,055	2,055	2,063	2,037	2,037
Control mean	17.03	17.03	17.03	4.85	4.85	4.85	6.86	6.86	6.86
	Subjective discount factor			Risk aversion					
				Coin			Market		
Treated	−0.01	−0.01	−0.01	−0.11*	−0.10*	−0.09	−0.05	−0.03	−0.03
	(0.02)	(0.02)	(0.02)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Placebo	−0.03	−0.02	−0.02	0.01	0.03	0.03	0.07	0.09	0.08
	(0.02)	(0.02)	(0.02)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Treated-Placebo	0.02	0.02	0.02	−0.12*	−0.13**	−0.12*	−0.12**	−0.12*	−0.12*
	(0.02)	(0.02)	(0.02)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2,061	2,037	2,037	2,061	2,035	2,035	2,061	2,024	2,024
Control mean	0.54	0.54	0.54	1.26	1.26	1.26	1.25	1.25	1.25

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

Table C.2: Treatment effects on expectations and aspirations indices

	Expectations						Aspirations					
	t=1			t=2			t=1			t=2		
Treated individual	0.12**	0.12**	0.12**	0.06***	0.06***	0.05***	0.13*	0.13**	0.13*	0.04*	0.04*	0.03*
	(0.06)	(0.05)	(0.06)	(0.02)	(0.02)	(0.02)	(0.07)	(0.07)	(0.07)	(0.02)	(0.02)	(0.02)
	0.03	0.02	0.03	0.01	0.01	0.01	0.05	0.05	0.05	0.06	0.06	0.08
Placebo individual	0.02	0.03	0.02	0.02	0.02	0.02	−0.00	−0.00	−0.00	0.03	0.02	0.03
	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
	0.56	0.49	0.56	0.27	0.29	0.24	0.97	0.97	0.97	0.16	0.20	0.15
Treated-Placebo	0.10**	0.10**	0.10**	0.03	0.03*	0.03	0.13**	0.13**	0.13**	0.01	0.01	0.01
	(0.05)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.06)	(0.06)	(0.06)	(0.02)	(0.02)	(0.02)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	1,959	1,954	1,959	2,062	2,054	2,054	1,959	1,957	1,959	2,063	2,058	2,058
Control mean	0.02	0.02	0.02	-0.01	-0.01	-0.01	0.03	0.03	0.03	0.03	0.03	0.03

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

Table C.3: Treatment effects on expectations in different dimensions of life

	Income			Wealth			Education			Status		
						Panel 1: After screening						
Treated	43,649.20*	44,107.87*	42,081.91*	−6,015.59	−6,233.92	−7,390.95	0.49***	0.46***	0.45***	1.38	2.00	1.72
	(23,775.28)	(23,938.20)	(23,026.10)	(9,735.80)	(9,015.27)	(9,230.98)	(0.16)	(0.13)	(0.13)	(1.35)	(1.24)	(1.23)
Placebo	−2,804.60	−3,721.79	−3,938.34	−10,120.33	−9,770.02	−7,341.44	0.44***	0.55***	0.55***	0.17	0.33	0.27
	(6,090.47)	(6,117.84)	(6,124.91)	(9,333.04)	(8,578.99)	(7,475.58)	(0.15)	(0.13)	(0.13)	(1.31)	(1.20)	(1.19)
Treated-Placebo	46,453.80**	47,829.66**	46,020.25**	4,104.75	3,536.10	−49.50	0.05	−0.08	−0.09	1.21	1.66	1.45
	(23,549.34)	(23,875.06)	(23,643.10)	(3,603.29)	(3,415.48)	(4,625.73)	(0.12)	(0.11)	(0.11)	(1.33)	(1.21)	(1.20)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	1,954	1,942	1,942	1,956	1,946	1,946	1,849	1,770	1,770	1,956	1,937	1,937
Control mean	31,510.57	31,510.57	31,510.57	35,471.56	35,471.56	35,471.56	12.27	12.27	12.27	72.15	72.15	72.15
						Panel 2: After six months						
Treated	−3,866.67	−3,948.39	−4,710.71	1,654.69	1,480.41	1,052.18	0.33***	0.23*	0.22*	2.95**	2.85**	2.58**
	(5,243.19)	(5,290.43)	(5,233.51)	(1,770.50)	(1,734.96)	(1,699.11)	(0.13)	(0.12)	(0.12)	(1.29)	(1.28)	(1.27)
Placebo	−2,970.60	−3,097.50	−2,909.35	−992.94	−1,275.24	−1,011.47	0.11	0.11	0.10	2.30*	2.02	1.94
	(5,473.04)	(5,525.54)	(5,516.95)	(1,640.43)	(1,602.60)	(1,540.06)	(0.13)	(0.13)	(0.13)	(1.30)	(1.30)	(1.28)
Treated-Placebo	−896.07	−850.89	−1,801.36	2,647.63	2,755.64	2,063.65	0.22*	0.12	0.12	0.65	0.84	0.64
	(4,456.28)	(4,490.09)	(4,572.26)	(1,760.72)	(1,728.39)	(1,702.51)	(0.13)	(0.13)	(0.13)	(1.29)	(1.29)	(1.28)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2,056	2,038	2,038	2,030	2,017	2,017	1,890	1,791	1,791	2,053	2,030	2,030
Control mean	36,153.51	36,153.51	36,153.51	24,532.19	24,532.19	24,532.19	12.47	12.47	12.47	67.14	67.14	67.14

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

Table C.4: Treatment effects on aspirations in different dimensions of life

	Income			Wealth			Education			Status		
					Panel 1: After screening							
Treated	211,165.61	190,040.33	185,632.00	165,594.07	167,100.18	149,872.98	0.11	0.08	0.07	−0.81	0.06	−0.10
	(133,331.30)	(130,123.32)	(126,461.55)	(162,204.07)	(162,890.53)	(147,947.62)	(0.09)	(0.08)	(0.08)	(1.45)	(1.31)	(1.31)
Placebo	−27,989.61	−27,576.37	−27,514.63	19,959.24	20,030.98	25,132.70	0.08	0.07	0.07	−1.63	−1.30	−1.39
	(47,471.96)	(47,509.40)	(47,901.25)	(44,243.20)	(44,517.61)	(42,788.04)	(0.08)	(0.08)	(0.08)	(1.44)	(1.32)	(1.31)
Treated-Placebo	239,155.22*	217,616.70*	213,146.63*	145,634.83	147,069.21	124,740.28	0.02	0.01	0.00	0.81	1.36	1.29
	(130,035.53)	(127,017.26)	(126,901.53)	(156,463.31)	(157,053.53)	(144,725.17)	(0.08)	(0.08)	(0.08)	(1.39)	(1.29)	(1.28)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	1,956	1,945	1,945	1,955	1,945	1,945	1,930	1,897	1,897	1,952	1,932	1,932
Control mean	168,007.25	168,007.25	168,007.25	82,603.81	82,603.81	82,603.81	12.96	12.96	12.96	76.46	76.46	76.46
					Panel 2: After six months							
Treated	4,425.14	5,346.37	4,069.15	−2,806.54	−2,806.72	−4,076.35	0.18*	0.17*	0.16	0.62	0.86	0.71
	(12,196.14)	(12,239.87)	(12,093.86)	(5,040.27)	(5,078.43)	(4,991.15)	(0.10)	(0.10)	(0.10)	(1.43)	(1.46)	(1.45)
Placebo	11,342.53	10,887.69	11,678.66	−2,061.02	−2,542.94	−1,812.30	0.08	0.09	0.09	0.97	0.97	0.95
	(12,247.84)	(12,117.89)	(11,945.81)	(5,132.16)	(5,153.80)	(4,976.01)	(0.10)	(0.10)	(0.10)	(1.44)	(1.45)	(1.44)
Treated-Placebo	−6,917.39	−5,541.32	−7,609.51	−745.52	−263.78	−2,264.05	0.10	0.09	0.08	−0.35	−0.11	−0.24
	(12,752.68)	(12,724.91)	(12,622.63)	(4,923.55)	(4,926.73)	(4,854.86)	(0.10)	(0.09)	(0.09)	(1.45)	(1.46)	(1.45)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2,052	2,036	2,036	2,019	2,007	2,007	1,973	1,935	1,935	2,059	2,036	2,036
Control mean	110,079.21	110,079.21	110,079.21	55,265.97	55,265.97	55,265.97	12.89	12.89	12.89	80.39	80.39	80.39

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

Table C.5: Treatment effects on beliefs about the minimum others can achieve

	Income			Wealth			Education			Status		
						Panel 1: After screening						
Treated	42.83	7.89	3.75	38.27	47.68	49.73	0.02	0.01	0.01	−1.52*	−1.28	−1.33*
	(67.41)	(63.27)	(63.11)	(49.33)	(47.49)	(47.47)	(0.06)	(0.06)	(0.06)	(0.80)	(0.79)	(0.79)
Placebo	−4.48	21.70	28.81	−33.06	−22.28	−21.21	0.01	−0.00	0.00	−1.72**	−2.06***	−2.06***
	(67.73)	(64.62)	(64.47)	(48.89)	(47.07)	(46.73)	(0.06)	(0.06)	(0.06)	(0.79)	(0.77)	(0.77)
Treated-Placebo	47.32	−13.80	−25.07	71.34	69.96	70.94	0.01	0.01	0.01	0.20	0.78	0.73
	(71.38)	(68.12)	(67.95)	(48.96)	(47.57)	(47.38)	(0.06)	(0.06)	(0.06)	(0.80)	(0.77)	(0.77)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	1,949	1,894	1,894	1,941	1,881	1,881	1,956	1,890	1,890	1,953	1,906	1,906
Control mean	1,470.97	1,470.97	1,470.97	984.78	984.78	984.78	0.17	0.17	0.17	16.97	16.97	16.97
						Panel 2: After six months						
Treated	116.81*	95.79	89.04	58.40	55.89	55.10	0.16*	0.14*	0.15*	0.21	0.13	0.08
	(64.75)	(65.22)	(64.97)	(38.96)	(39.36)	(39.11)	(0.08)	(0.08)	(0.08)	(0.83)	(0.84)	(0.84)
Placebo	145.02**	139.20**	134.09**	18.57	11.23	12.91	−0.02	−0.03	−0.03	−0.15	−0.32	−0.37
	(62.74)	(63.52)	(63.48)	(37.84)	(38.64)	(38.34)	(0.07)	(0.07)	(0.07)	(0.83)	(0.84)	(0.84)
Treated-Placebo	−28.21	−43.41	−45.05	39.83	44.66	42.19	0.18**	0.18**	0.18**	0.35	0.45	0.45
	(64.60)	(65.23)	(65.15)	(37.87)	(38.15)	(37.70)	(0.08)	(0.08)	(0.08)	(0.83)	(0.85)	(0.85)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2,025	1,965	1,965	2,036	1,968	1,968	2,035	1,964	1,964	2,048	1,994	1,994
Control mean	1,538.77	1,538.77	1,538.77	813.58	813.58	813.58	0.27	0.27	0.27	15.33	15.33	15.33

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

Table C.6: Treatment effects on beliefs about the maximum others can achieve

	Income			Wealth			Education			Status		
					Panel 1: After screening							
Treated	1,078.67*	987.84	1,012.11	1,784.25	1,746.45	1,486.16	0.26**	0.22**	0.21**	−0.35	0.51	0.49
	(644.47)	(633.85)	(631.57)	(1,471.43)	(1,413.91)	(1,408.28)	(0.11)	(0.11)	(0.11)	(1.28)	(1.18)	(1.18)
Placebo	818.58	712.48	859.31	2,107.13	1,920.32	2,256.58	0.23**	0.25**	0.25**	−0.50	−0.84	−0.87
	(656.93)	(634.20)	(630.79)	(1,790.77)	(1,751.35)	(1,604.84)	(0.11)	(0.10)	(0.10)	(1.32)	(1.26)	(1.26)
Treated-Placebo	260.09	275.36	152.81	−322.87	−173.87	−770.42	0.03	−0.04	−0.04	0.15	1.35	1.36
	(687.92)	(667.05)	(663.64)	(1,883.00)	(1,851.43)	(1,776.65)	(0.11)	(0.10)	(0.10)	(1.25)	(1.17)	(1.18)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	1,932	1,917	1,917	1,954	1,934	1,934	1,952	1,928	1,928	1,953	1,937	1,937
Control mean	14,394.50	14,394.50	14,394.50	19,080.45	19,080.45	19,080.45	11.38	11.38	11.38	75.81	75.81	75.81
					Panel 2: After six months							
Treated	2,441.26***	2,328.32***	2,142.09***	643.19	618.48	80.12	0.11	0.10	0.08	−2.03	−1.94	−1.95
	(790.54)	(788.84)	(777.56)	(1,647.70)	(1,630.24)	(1,595.08)	(0.11)	(0.11)	(0.11)	(1.26)	(1.27)	(1.28)
Placebo	470.45	402.70	433.54	356.03	150.97	681.58	0.09	0.09	0.10	0.08	0.08	0.14
	(742.82)	(744.49)	(739.23)	(1,611.50)	(1,625.73)	(1,520.78)	(0.10)	(0.10)	(0.10)	(1.28)	(1.29)	(1.29)
Treated-Placebo	1,970.81***	1,925.63***	1,708.54**	287.16	467.51	−601.46	0.02	0.00	−0.02	−2.11*	−2.03	−2.09
	(787.29)	(784.61)	(772.79)	(1,528.78)	(1,539.66)	(1,515.95)	(0.10)	(0.10)	(0.10)	(1.29)	(1.30)	(1.30)
Village F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lagged outcome	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Controls	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*	*No*	*No*	*Yes*
Respondents	2,026	2,004	2,004	2,032	2,008	2,008	2,049	2,018	2,018	2,043	2,022	2,022
Control mean	17,703.42	17,703.42	17,703.42	21,260.75	21,260.75	21,260.75	11.47	11.47	11.47	77.23	77.23	77.23

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

D

Full results for tests for spillovers

Table D.1: Test for spillovers on psychological variables using village-level treatment intensities

	After six months		Locus of control			Causes of poverty		
	Exp.	Asp.	Chance	Others	Internal	Fate	Struct.	Indiv.
Treated individual	0.04	0.00	0.04	0.19	0.47*	−0.42	0.16	0.63***
	(0.03)	(0.02)	(0.30)	(0.36)	(0.26)	(0.37)	(0.27)	(0.22)
Placebo individual	0.02	0.01	−0.08	−0.16	−0.07	−0.48	0.13	0.35
	(0.03)	(0.03)	(0.22)	(0.26)	(0.25)	(0.30)	(0.22)	(0.21)
Treated village	−0.02	−0.02	−0.14	−0.16	−0.01	−0.44	0.02	0.02
	(0.07)	(0.05)	(0.33)	(0.46)	(0.28)	(0.32)	(0.26)	(0.24)
Individual treatment*	0.03	0.08*	−0.20	−0.54	−0.34	−0.01	−0.29	−0.44
Village-level treatment	(0.05)	(0.04)	(0.45)	(0.54)	(0.40)	(0.54)	(0.40)	(0.36)
Individual treatment*	0.02	0.04	0.11	0.35	−0.05	0.99**	0.30	−0.08
Village-level placebo	(0.04)	(0.04)	(0.36)	(0.44)	(0.37)	(0.46)	(0.34)	(0.30)
Screening site F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	2,062	2,063	2,039	2,052	2,058	2,063	2,063	2,063
Control mean	-0.01	0.03	13.35	12.62	16.05	9.85	10.36	11.47
Treated-Placebo	0.03	−0.01	0.12	0.34	0.53	0.06	0.03	0.28
	(0.03)	(0.03)	(0.27)	(0.31)	(0.39)	(0.32)	(0.33)	(0.26)

		Life satisfaction			Risk aversion	
	Self-esteem	Best life	Happiest life	Discount factor	Coin	Market
Treated individual	0.43**	0.10	0.20	−0.01	−0.05	0.00
	(0.21)	(0.21)	(0.23)	(0.02)	(0.09)	(0.09)
Placebo individual	0.13	0.16	0.09	−0.02	0.03	0.05
	(0.16)	(0.18)	(0.19)	(0.02)	(0.10)	(0.10)
Treated village	0.48**	0.16	−0.03	−0.01	0.01	−0.02
	(0.21)	(0.23)	(0.24)	(0.03)	(0.13)	(0.14)
Individual treatment*	−0.56*	−0.10	0.07	−0.01	−0.13	−0.10
Village-level treatment	(0.29)	(0.31)	(0.31)	(0.04)	(0.15)	(0.13)
Individual treatment*	−0.47*	−0.30	−0.21	−0.01	−0.02	0.07
Village-level placebo	(0.26)	(0.29)	(0.27)	(0.03)	(0.12)	(0.13)
Respondents	2,058	2,063	2,063	2,061	2,061	2,061
Control mean	17.03	4.85	6.86	0.54	1.26	1.25
Treated-Placebo	0.30	−0.06	0.10	0.02	−0.07	−0.05
	(0.22)	(0.18)	(0.22)	(0.03)	(0.09)	(0.09)

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parentheses.

Table D.2: Test for spillovers on behaviour using village-level treatment intensities

	Work time	Leisure time	Has savings	Total savings	Took out credit	Total credit
Treated individual	19.80	4.08	0.01	116.68	0.01	21.69
	(12.46)	(18.66)	(0.04)	(129.27)	(0.05)	(17.01)
Placebo individual	7.05	14.03	0.05	−38.59	−0.00	19.58
	(13.46)	(16.08)	(0.04)	(52.71)	(0.04)	(14.19)
Treated village	28.81**	−14.46	0.00	1.84	−0.02	6.57
	(13.29)	(17.05)	(0.04)	(70.18)	(0.03)	(14.05)
Individual treatment*	−25.42	1.90	0.07	4.99	0.04	0.82
Village-level treatment	(18.15)	(23.89)	(0.06)	(145.85)	(0.06)	(24.22)
Individual treatment*	−16.49	−14.34	−0.09	76.87	−0.02	−29.99
Village-level placebo	(17.90)	(22.88)	(0.06)	(111.27)	(0.05)	(19.79)
Screening site F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	2,052	2,052	2,063	2,053	2,063	2,044
Control mean	310.54	798.91	0.39	182.36	0.34	100.99
Treated-Placebo	12.75	−9.95	−0.04	155.27	0.01	2.12
	(14.81)	(17.97)	(0.04)	(112.78)	(0.04)	(18.25)

	Hypothetical credit: loan repayable in				
	1 year	5 years	10 years	Children enrolled	Ed. spending
Treated individual	−627.99	−2,369.47	2,771.86	0.20	58.04**
	(1,210.25)	(3,134.27)	(4,519.25)	(0.12)	(25.93)
Placebo individual	−549.93	−4,955.45	−3,835.42	0.14	54.97*
	(1,260.57)	(3,061.95)	(3,067.72)	(0.10)	(28.99)
Treated village	−232.51	−3,982.02	−3,619.05	0.06	36.25
	(1,614.30)	(3,290.46)	(4,576.51)	(0.11)	(29.80)
Individual treatment*	2,868.11	8,937.36*	6,474.94	0.16	4.56
Village-level treatment	(2,126.52)	(4,556.42)	(6,289.15)	(0.18)	(44.04)
Individual treatment*	3,817.65*	9,678.16**	4,142.27	−0.11	−47.55
Village-level placebo	(2,220.06)	(4,483.88)	(5,608.82)	(0.16)	(41.98)
Screening site F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	1,131	1,126	1,114	1,137	1,128
Control mean	10,525.73	22,604.28	33,955.50	1.23	197.42
Treated-Placebo	−78.05	2,585.99	6,607.28	0.06	3.07
	(1,093.67)	(1,802.57)	(4,198.79)	(0.11)	(33.48)

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parentheses.

Table D.3: Test for spillovers on psychological variables using peer network measures

	After six months		Locus of control			Causes of poverty		
	Exp.	Asp.	Chance	Others	Int.	Fate	Struct.	Indiv.
Treated individual	0.13***	0.08***	0.18	−0.28	0.02	−0.77**	−0.04	0.15
	(0.04)	(0.03)	(0.33)	(0.39)	(0.31)	(0.38)	(0.30)	(0.29)
Placebo individual	0.04	0.04	0.49**	0.46	−0.44	0.15	0.20	0.16
	(0.04)	(0.04)	(0.24)	(0.34)	(0.30)	(0.35)	(0.27)	(0.29)
Peers treated	0.04	0.02	−0.05	−0.14	−0.12	−0.28*	−0.11	−0.01
	(0.02)	(0.02)	(0.13)	(0.17)	(0.12)	(0.15)	(0.13)	(0.14)
Peers placebo	0.02	0.01	0.27**	0.09	−0.20	0.15	−0.06	−0.11
	(0.02)	(0.02)	(0.12)	(0.17)	(0.14)	(0.16)	(0.14)	(0.13)
Treated*Peers treated	−0.03	−0.01	−0.13	0.10	0.07	0.33	−0.11	0.04
	(0.02)	(0.02)	(0.19)	(0.25)	(0.20)	(0.22)	(0.17)	(0.22)
Treated*Peers placebo	−0.06***	−0.05*	−0.19	0.13	0.27	0.08	0.16	0.29*
	(0.02)	(0.02)	(0.18)	(0.20)	(0.18)	(0.22)	(0.18)	(0.16)
Placebo*Peers treated	−0.01	−0.01	−0.17	−0.18	0.17	0.20	0.16	−0.04
	(0.02)	(0.02)	(0.17)	(0.24)	(0.19)	(0.25)	(0.16)	(0.19)
Placebo*Peers placebo	0.01	0.01	−0.49***	−0.38*	0.28	−0.41*	−0.08	0.26
	(0.03)	(0.02)	(0.16)	(0.20)	(0.19)	(0.22)	(0.19)	(0.17)
Screening site F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	2,062	2,063	2,039	2,052	2,058	2,063	2,063	2,063
Control mean	-0.01	0.03	13.35	12.62	16.05	9.85	10.36	11.47
Treated - Placebo	0.10**	0.05	−0.31	−0.74**	0.46	−0.92**	−0.24	−0.01
	(0.04)	(0.04)	(0.33)	(0.33)	(0.35)	(0.38)	(0.31)	(0.31)

		Life satisfaction			Risk aversion	
	Self-esteem	Best life	Happiest life	Discount factor	Coin	Market
Treated individual	0.18	0.11	0.50**	−0.01	−0.09	0.05
	(0.20)	(0.24)	(0.20)	(0.03)	(0.13)	(0.14)
Placebo individual	−0.17	0.13	0.31	−0.03	−0.01	0.12
	(0.20)	(0.24)	(0.23)	(0.03)	(0.13)	(0.13)
Peers treated	0.13*	0.09	0.09	−0.01	0.01	0.02
	(0.08)	(0.10)	(0.10)	(0.01)	(0.06)	(0.06)
Peers placebo	0.04	0.08	0.24*	−0.02	−0.05	−0.01
	(0.09)	(0.11)	(0.12)	(0.01)	(0.06)	(0.06)
Treated*Peers treated	−0.14	−0.04	0.02	−0.01	−0.13	−0.14*
	(0.14)	(0.14)	(0.13)	(0.02)	(0.08)	(0.07)
Treated*Peers placebo	0.10	−0.05	−0.35**	0.01	0.10	0.01
	(0.13)	(0.14)	(0.16)	(0.01)	(0.08)	(0.07)
Placebo*Peers treated	−0.01	−0.03	−0.15	−0.01	−0.02	−0.00
	(0.11)	(0.13)	(0.13)	(0.02)	(0.07)	(0.07)
Placebo*Peers placebo	0.11	−0.12	−0.26*	0.01	0.05	−0.05
	(0.14)	(0.14)	(0.14)	(0.02)	(0.09)	(0.08)
Respondents	2,058	2,063	2,063	2,061	2,061	2,061
Control mean	17.03	4.85	6.86	0.54	1.26	1.25
Treated - Placebo	0.35	−0.01	0.18	0.02	−0.09	−0.06
	(0.24)	(0.24)	(0.24)	(0.04)	(0.13)	(0.14)

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parentheses.

Table D.4: Test for spillovers on behaviour using peer network measures

	Work time	Leisure time	Has savings	Total savings	Took credit	Total credit
Treated individual	1.19	2.77	0.10*	213.55**	0.00	6.82
	(20.29)	(16.97)	(0.05)	(96.96)	(0.05)	(23.72)
Placebo individual	−21.84	15.40	0.06	−0.65	0.00	−3.08
	(21.48)	(17.93)	(0.04)	(81.61)	(0.04)	(19.65)
Peers treated	22.70**	2.67	0.01	25.65	−0.01	−4.52
	(10.88)	(9.20)	(0.02)	(23.27)	(0.02)	(9.69)
Peers placebo	4.15	4.09	0.02	34.10	0.00	−8.31
	(10.88)	(7.89)	(0.02)	(40.11)	(0.02)	(9.27)
Treated*Peers treated	0.17	5.90	−0.02	−16.53	0.02	9.50
	(12.94)	(13.26)	(0.03)	(92.80)	(0.03)	(15.29)
Treated*Peers placebo	8.45	−2.78	−0.04	−98.37*	0.01	9.82
	(15.09)	(12.02)	(0.03)	(53.67)	(0.03)	(10.82)
Placebo*Peers treated	4.31	−12.96	−0.02	28.53	−0.01	−7.98
	(13.21)	(12.67)	(0.03)	(36.56)	(0.03)	(13.09)
Placebo*Peers placebo	23.56	3.94	−0.05	−31.89	0.00	19.99*
	(16.02)	(11.53)	(0.03)	(60.77)	(0.03)	(11.23)
Screening site F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	2,052	2,052	2,063	2,053	2,063	2,044
Control mean	310.54	798.91	0.39	182.36	0.34	100.99
Treated - Placebo	23.03	−12.63	0.04	214.19*	0.01	9.89
	(20.25)	(17.75)	(0.05)	(113.67)	(0.05)	(24.38)

	Hypothetical credit: loan repayable				
	1 year	5 years	10 years	Children enrolled	Ed. spending
Treated individual	3,432.32*	7,541.18**	12,319.67**	0.16	64.89*
	(1,909.05)	(3,737.19)	(4,912.78)	(0.16)	(36.32)
Placebo individual	3,600.48	3,316.73	5,644.77	−0.04	14.37
	(2,371.31)	(3,671.33)	(4,210.42)	(0.11)	(35.18)
Peers treated	1,042.41	817.27	3,180.34	0.02	25.75
	(824.35)	(1,656.03)	(2,801.30)	(0.07)	(17.17)
Peers placebo	2,500.60**	5,674.13***	5,591.49**	−0.01	14.43
	(1,058.85)	(1,903.39)	(2,454.05)	(0.07)	(14.20)
Treated*Peers treated	100.21	179.31	−1,797.28	0.14	3.72
	(1,350.83)	(2,117.31)	(2,873.72)	(0.12)	(26.29)
Treated*Peers placebo	−3,391.77**	−7,103.47**	−6,205.47*	0.01	−8.64
	(1,384.72)	(2,724.55)	(3,469.00)	(0.10)	(19.80)
Placebo*Peers treated	218.64	768.79	−3,381.58	0.10	30.58
	(1,363.48)	(2,819.41)	(3,528.15)	(0.09)	(21.16)
Placebo*Peers placebo	−3,313.96*	−5,485.69**	−6,059.96*	0.05	−16.13
	(1,666.28)	(2,516.55)	(3,122.42)	(0.09)	(23.82)
Screening site F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Respondents	1,131	1,126	1,114	1,137	1,128
Control mean	10,525.73	22,604.28	33,955.50	1.23	197.42
Treated-Placebo	−168.16	4,224.45	6,674.90	0.20	50.52
	(2,335.50)	(4,071.25)	(6,129.76)	(0.14)	(44.11)

*p below 0.10 **p below 0.05 ***p below 0.01. Robust standard errors clustered at village level in parentheses.

6

Children's social roles and preferences about time allocation

I. Introduction

In a standard neoclassical model, an individual's preferences are not affected by whether they conform to the expectations of those around them. Students choose schools, time in school and amount of effort to balance opportunity costs and direct costs with discounted returns. However, as argued in Chapter 3, economists increasingly puzzle over patterns in behaviour not predicted by the standard model. Children and families often do not invest in education when there are high returns, or do invest when returns are low. Whether children's peers are enrolled affects their investment decisions. Decisions about whether to invest in school inputs do not change in relation to price in the ways one might expect.

Some economists examine whether concepts of identity and social roles from sociology and social psychology might explain some of these patterns. Some research has simply examined the concrete economic benefits of group membership. For example, working-class Indian lower-caste parents mainly chose to send boys to local language schools, which led to employment in occupations traditionally performed by members of their caste, with a rate of return to a year of education of only 15 per cent; but sent girls to English language schools, with a rate of return of 24 per cent (Munshi and Rosenzweig, 2006). The authors attribute the behaviour to the institution of the caste system, which does not affect girls as they traditionally do not work for pay. There are concrete economic benefits (because of network externalities) to the whole caste if members stay in the caste occupation; so the caste places tacit restrictions on boys' occupational mobility. Once one understands the role of the caste

system as an institution, standard models predict parents' decisions. Other perspectives on identity go further: Akerlof and Kranton (2000) examine whether identity influences economic behaviour when it specifically does not have economic benefits, and may explain behaviour which is otherwise not predicted by standard models.

This chapter examines two broad issues which bear on the research questions of the thesis. Firstly, I examine whether identity and social roles predict investment in education and allocation of time, among children aged 12 to 15 in an Ethiopian village. I use longitudinal qualitative data collected with children, parents, teachers and community leaders in 2008 and 2010. I interpret children's choices of daily activities and "how they relate to moral categories and ideas of a good life on the ground, where everyday life is enacted" (Reynolds, 2006, 295).

I find that children's desire to behave in accordance with the social prescriptions attached to identities that are valued is important in their daily choices of activity. Being a student is a key source of pride and a mark of a good work ethic, and is associated with triumphing over material constraints and distractions. It is has connotations of being modern and having progressed, rather than remaining a farmer like one's parents. However, children also take pride in being independent earners, the identity they derive from their paid work. And they derive utility from adhering to more traditional roles as obedient children who contribute to the household by doing chores, and from learning household skills that they will use as adults.

The concept of identity explains patterns in behaviour that are not predicted by the standard model. The status associated with this identity potentially explains why students may stay in school, even when it has low average returns because of the very low likelihood that children will complete school and find formal employment. Children want to belong to this high-status identity. They are even prepared to work for pay to invest in schooling materials, which are physical markers of being a student. If children's peers have an identity as students, this may further reinforce their desire to stay in school.

Relatedly, given children's investments of time and often their own money in schooling, they engage in further behaviour which is irrational according to standard economic models, by continuing to work in their households despite its negative effects on their schooling. They also work for pay, partly so that they can purchase schooling materials, but also to contribute to their households. Identity explains this behaviour: their time allocation is driven not only by what will maximise returns on their investments of money in education, but also by the

desire to comply with other valued identities as independent earners and good and obedient children.

The apparent influence of identity on economic decisions poses a second question, namely which theoretical model best captures ideas of identity, and what adjustments to standard models might be required for them to incorporate them. On the one hand the Akerlof and Kranton (2000) model slightly modifies the standard model to allow for individuals' having one largely "fixed" identity, which provides prescriptions for behaviour across multiple contexts. This view does not account for individuals having multiple roles simultaneously, nor suggest how they might prioritise the prescriptions of multiple roles if they conflict. On the other hand there is the "frame-dependent" theory of identity (Hoff and Pandey, 2014) proposed by behavioural economists, in which identity is not "a set of values or preferences", but a "fragmented set of mental models, understandings, worldviews and guides to action" (Hoff and Pandey, 2014, 119). On this view the identity evoked in a particular situation influences which behaviours are acceptable and which choices individuals prefer (LeBoeuf, Shafir, and Bayuk, 2010, 49); with more drastic implications for the assumptions of the standard model, notable stable preferences across contexts. The qualitative data, however, require a more nuanced approach than either extreme. On this basis I shall argue in the conclusion of the chapter for an appreciable extension of the Akerlof and Kranton (2000) model, to recognise how the children, faced with complicated circumstances and potentially contradictory roles, often manage the trade-offs with thoughtful deliberation.

Furthermore, if identity is to be understood as a preference for complying with a social role, rather than a mental model, one should consider whether preferences to comply with a social role are self-interested, as preferences are usually assumed to be in standard economic models, or altruistic. I argue that preferences to comply with social roles can be altruistic.

In Section II, I describe the two potential perspectives for thinking about identity. Section III motivates the decision to use qualitative evidence in a question related to microeconomic theory and describes the data. Section IV gives some context about children's lives and activities in the study site. In Section V, I describe the different identities which emerge as important to children in the data. Section VI describes instances where these multiple identities apply simultaneously and what ensues. The conclusion discusses the implications of these findings for theory on identity.

II. Literature

In a standard neoclassical model, an individual's preferences are not affected by whether they conform to the expectations of those around them. As the concept of identity has been increasingly applied in economics, a debate has arisen between two theoretical models of identity. This debate captures the two alternative paths for incorporating psychological concepts in economics described in Chapter 3.

One path assumes identities, such as gender or caste, are relatively stable across a number of contexts. Preferences for complying with these identities can be incorporated relatively easily into standard models, leaving their assumptions about beliefs and preferences broadly intact. The other path emphasises that individuals have and make a choice among identities and that their preferences and choices are very largely context-dependent. Behavioural economists consider whether identity, like other types of preferences, may be context-specific and frame-dependent. This requires substantial, empirically motivated modifications to the assumptions of standard models.

A. Stable, context-independent, "fixed" identity

Akerlof and Kranton (2000) slightly modify a standard economic model to incorporate ideas of identity and social roles. People often use social categories to think of themselves and others. "Prescriptions" define the ideal attributes and behaviour appropriate for members of a category.[1] They argue that people's utility functions can include a preference for adhering to one's social role, to capture that conforming to their category's ideal generates utility by affirming people's self-image; whereas behaving contrary to the ideal results in anxiety and discomfort and thus disutility. People also gain utility if they belong to higher status social categories. Thus utility is not only derived from consumption of material goods, but also from status and belonging.

This model differs from models of social norms in economics. In other models of social norms, people comply with norms because they receive concrete censure if they do not (Akerlof, 1976; Kandori, 1992; Cole, Mailath, and Postlewaite, 1992). This may be from other group members rather than an outside authority (Fehr and Gächter, 2000; Fehr, Fischbacher, and Gächter, 2002). Although people may prefer to take an action, they do not because of

[1]The authors use the term "prescription" to differentiate the concept from the usual economic idea of a norm, because prescriptions influence behaviour whether or not people receive concrete censure from other people.

the costs associated. In contrast, in the Akerlof and Kranton (2000) model, adhering to one's social role can become intrinsically rewarding because people associate positive feelings with a behaviour itself, not with receiving approval from others (Woersdorfer, 2010).

The Akerlof and Kranton (2000) model predicts results unexplained by standard models, such as gender discrimination in the workplace, the economics of poverty and social exclusion, and household division of labour. Identity explains why people often behave in ways which seem detrimental to their interests, whether it is taking on the bulk of domestic tasks to comply with ideals of womanhood, or adopting "oppositional" identities (such as gang membership) that are acts of rebellion against a dominant culture but may also lead to poorer long-run outcomes. Their model has many empirical applications (Munshi and Wilson, 2011; Fang and Loury, 2005), including in developing countries (Munshi and Rosenzweig, 2006; Hoff and Sen, 2006).

This model accords with Stryker's (1968; 1980) identity theory, a widely held perspective on identity in sociology. For an individual, some of their group memberships or social roles acquire meaning through repeated social interactions in a process of socialisation. Importantly, people prioritise their identities hierarchically depending on which is most salient. Salience depends on how extensive the social interactions linked to particular identity are and how emotionally invested the person is in these relationships. People can hold multiple identities simultaneously, but they have a unified sense of self – a "stable, trans-situational self-concept" (Owens, Robinson, and Smith-Lovin, 2010, 478) – which prioritises identities across contexts. This self-image may shift gradually over time through sustained interactions with others, but it is relatively stable across contexts (Stets and Burke, 2005). As in the Akerlof and Kranton (2000) model, people act to reduce any discrepancy they perceive between the ideal of a particular identity and their behaviour (Burke, 1991).

B. Applications of the stable-identity model to education

Akerlof and Kranton (2002) consider how identity affects children's investments in education. They argue that children are largely concerned with fitting in and with social status, not with the long-term returns to schooling, as in a standard model. They draw on sociological studies of identity in American high schools, where membership in particular social categories is correlated with achievement, effort in school, behaviour outside school, and students' sense of belonging and self-confidence (Hollingshead, 1949; Coleman, 1961; Cohen, 1979; Eckert, 1989).

In their model, students choose which social category to belong to based on the match between their characteristics and the ideal of each category and the status of the categories. Students then choose their effort level and investment in education according to the prevalent effort level in their category.

Importantly, schools are "social institutions with social goals" (Akerlof and Kranton, 2002, 1180) and often set prescriptions for how the ideal student should behave. Teachers and principals reward students who are similar to the ideal student and punish those who disobey prescriptions (Erikson, 1966). For example, British schools in the 1970s (Willis, 1977) and American schools in the late nineteenth century (Hampel, 1986; Bloom, 1987; Tyack, 1974) sought to "reform" working class or immigrant youth and impose order and discipline. Schools may emphasise marketable skills and behaviours which will help students find jobs (Bowles, Gintis, and Osborne, 2001), but they may also emphasise religious or cultural identity (Peshkin, 1986) or privilege sporting over academic performance (Bishop, 1999). Schools and curricula may also impart political values (Friedman et al., 2011; Cantoni et al., 2014).

Students who fit the ideal identify strongly with the school, but students whose backgrounds or behaviour conflict with the ideal are "likely to resent the implication that there is something wrong with their background, and, by extension, with them. To avoid a loss in self-image, a student rejects the school and consequently exerts low effort" (Akerlof and Kranton, 2002, 1169). For example, Willis (1977) depicts how working class "lads" rebel against their teachers' attempts to discipline them and encourage them to aspire to a middle class identity. Foley (1990), Weiss (1990) and Ogbu (1999) provide other examples of oppositional identity in schools. This may explain why these students underinvest in education despite returns to completing it.

C. Frame-dependent identity

More recently, sociologists working in social identity theory have argued that an individual's self-concept includes many identities which are activated by different social contexts (Turner, 1985; Tajfel and Turner, 1986; Markus and Kunda, 1986). Cultural sociologists draw on cognitive psychology to explain the cognitive mechanisms which influence when particular aspects of identity become salient (DiMaggio, 1997).[2] Like behavioural economists, they argue

[2]The meaning of "salience" here is different to its meaning in identity theory (Stryker, 1968, 1980) in the previous sub-section. In the former, identity salience is a stable part of oneself, a result of commitment to the identity. While the theory allows multiple identities, individuals have one clear ranking or prioritisation of their identities which holds across contexts. In contrast, here the salience of an identity is determined by context.

that people think using schemata, or "knowledge structures that represent objects or events and provide default assumptions about their characteristics, relationships and entailments under conditions of incomplete information" (DiMaggio, 1997, 269). Social roles are one type of schema: they provide guidance for behaviour and social interactions in particular situations (Markus, Kitayama, and Heiman, 1996; Milburn, 1987). Rather than an underlying sense of self – a "strong latent or underlying variable" which is "unitary and internally coherent across groups and situations" (DiMaggio, 1997, 264) – people draw on a "toolkit" (Swidler, 1986) or "repertoire" (Tilly, 2002) of techniques and strategies to guide behaviour.

Behavioural economists draw on this literature to develop a theory of identity which strongly contrasts with the Akerlof and Kranton (2000) model. Identity is not a preference, but a set of mental models which "determine how an individual construes a situation and which actions seem possible and desirable in that situation, given a person's values" (Hoff and Pandey, 2014, 119). Rather than having one consistent set of preferences for complying with a particular identity, it may be "a useful construct to posit multiple preferences, one for each self-construal or worldview" (Hoff and Pandey, 2014, 121).

Experiments find that identities that are salient may vary, even in the same decision-making situation. The identity which has been triggered experimentally drives behaviour. Thus people's preferences for complying with a social role are not stable, but vary across context and time. For example, people primed to think about family had stronger preferences for choosing a family obligation over a work obligation, and vice versa (LeBoeuf, Shafir, and Bayuk, 2010). Prisoners who completed a questionnaire that highlighted their status as prisoners were more likely to cheat in games than prisoners who had not been primed to think about their identity (Cohn, Marechal, and Noll, 2013).

D. Applications of the frame-dependent model to education

This perspective has been applied to demonstrate that students' academic performance may decline if their environment triggers a particular identity. Students from low-status groups may be aware of negative stereotypes about the intelligence of members of these groups and worry that performing poorly will confirm this stereotype (Steele and Aronson, 1995; Steele, 1997). This anxiety may threaten their sense of self, resulting in stress and anxiety that negatively affects performance (Schmader, Johns, and Forbes, 2008).

This hypothesis has been tested in experiments which cue or trigger children's membership of a low-status group. Groups of high and low caste Indian boys were asked to solve mazes (Hoff and Pandey, 2014). Their caste could be discerned from physical markers. In groups where the boys did not know which caste others belonged to, there were no differences in performance between boys from high or low castes. But in groups where caste identity was revealed, boys from higher castes outperformed boys from lower castes. The results were replicated in China with urban and migrant, low-status children (Afridi, Li, and Ren, 2015). Similarly, African-American students did worse on scholastic aptitude tests when they had to declare their race beforehand (Steele and Aronson, 1995). Asian-American women performed worse on a maths test than the control group if their gender was primed, but better than the control group if their Asian identity was primed (Shih, Pittinsky, and Ambady, 1999).

Researchers have intervened to build up students' psychological resources against this sense of threat. African-American children in Grade 7 who completed exercises about why their most important values (e.g., relationships with friends and family) were important to them two to five times over a year had grade point average (GPA) scores 0.24 points higher than the control group after a year, equivalent to a 30 per cent reduction in the achievement gap between African-American and white students. The treatment effect was 0.41 among poor performers (Cohen et al., 2006, 2009). Effects persisted three years later; which is striking, as the effects of brief psychological interventions often disappear over time (Sherman et al., 2013; Cohen et al., 2009).

At face value, these results are more related to students' performance than their investment in education. However, one implication of these results is that students who feel threatened may continue to exert effort but feel anxious and stressed in ways which impair their performance (Cohen et al., 2006, 2009; Cohen and Sherman, 2014). The Akerlof and Kranton (2002) model, in contrast, suggests that students who feel threatened choose to identify with a rebellious group and invest less in their education.

III. Data

A. Qualitative data in economic research

Unusually, this chapter speaks to theory in microeconomics using original qualitative research. Social roles and prescriptions are anchored in particular societies, cultures, and time periods.

Understanding whether agents derive utility from adhering to their social role requires understanding which social categories children themselves prioritise in a particular context; it involves taking an emic approach which "investigates how local people think" (Kottak, 2006, 47). I thus use semi-structured methods and open-ended questions which make as few assumptions as possible regarding how children conceive of their roles and identities. I first understand which categories are meaningful locally and then explore which models of identity appear to best characterise patterns in behaviour by people in these categories.

Not many economists conduct their own qualitative research. Increasingly, economists draw on qualitative research by other social scientists when applying models to different contexts or exploring whether key assumptions are appropriate. Both the theoretical perspectives on identity discussed above draw heavily on and cite qualitative evidence from sociology and anthropology. Perhaps the most notable example is in models of intrahousehold bargaining: Bardhan and Ray (2006, 1) suggest that anthropological work heavily influenced a group of economists working in South Asia who instigated a "recent shift in economics from the unitary model where household members have a joint utility function, to the binary model where the utility functions are gender-specific" (see work by Sen, 1990; Basu, 2006; Agarwal, 1997). Qualitative research is useful when discussing sensitive issues, such as domestic violence, or developing models of populations whose perspectives have not been widely articulated (Rao, 1997a,b).

Bina Agarwal (1997, 6), an economist, argues that qualitative work is useful to capture "the complexity and historic variability" in social roles and practices. She describes the process of drawing on qualitative work as "analytical description":

> a formulation that seeks to comprehensively spell out both qualitative and quantitative factors that might impinge on outcomes, without being pre-constrained by the structure that formal modelling imposes, or by data limitations.

B. Sampling frame and representivity

I used survey data collected by Young Lives, a longitudinal study of children, on 1,000 older cohort children, which is described in more detail in the Appendix to Chapter 7. Children and their caregivers were surveyed in 2002/03, when the children were between 7 and 8 years old, again between October 2006 and April 2007, when children were between 11 and 12, and again between October 2009 and March 2010, when children were between 14 and 15.

I negotiated with Young Lives to conduct qualitative fieldwork with children who were also participants in the survey. I used the 2006 data as a sampling frame to select participants for qualitative research in a rural Young Lives site. I conducted one round of fieldwork between July and September 2008, one and a half years after the Round 2 survey, when children were between 12 and 13, and another between April and May 2010, just after the Round 3 survey, when children were between 14 and 15.

C. A child-centred approach

Researchers within childhood studies criticise other social scientists for failing to recognise children's agency and subjectivity (James and Prout, 1997). They plausibly argue that adult perceptions of what children think, do or need often differ from what children themselves actually think, do or need (Hill, 1997). Child-focused research views children as competent informants who can think, decide, feel and aspire, and sees children as the most accurate source of information on their daily lives (Ben-Arieh, 2005). Researchers thus interview children directly instead of asking their caregivers about them, as is common practice in economics. A child-centred approach also recognises that children's present lives, feelings and decisions are of as much interest as their preparations for adulthood and their future (Uprichard, 2008).

Some economists argue that their discipline needs to take a more child-centred approach. Levison (2000) criticises mainstream economists for conceiving of children as passive recipients of care, not as economic agents who make production and consumption choices. She argues that children are "actors in their own right, constrained by societies' constructions of appropriate spaces and activities for childhood but mediating the impacts of social boundaries by their choices and behaviour" (Levison, 2000, 125), as highlighted in anthropological research. Systematic incorporation of children's perspectives into economics, for example by interviewing them in research, is thus "appropriate in the interests of accuracy and an enhanced understanding of human behaviour"(Levison, 2000, 127).

While my research aims to be child-focused, I additionally interview children's caregivers. One motivation for interviewing caregivers was to study intrahousehold bargaining, which I examine in other work (Orkin, 2011). I also asked caregivers for their opinions about children in their community, rather than their own children, and discussed their perceptions of appropriate activities for children, children's obligations, and the role children should

play in their households. In this chapter, I treat the perspectives of adults as capturing the perceptions and values of the broader community.

D. Sampling and activities in 2008

In 2006, there were 584 Young Lives children living in 13 rural sites. I used the second round of survey data to identify one rural survey site, Leki, as a case-study village.[3] In 2006, 47 per cent of Young Lives children in Leki worked for pay, compared to only 8 per cent across all 13 rural Young Lives sites. I sought to understand why children and households decide to invest in schooling when it has limited returns and there is an opportunity cost, a pattern which is even more striking when there are opportunities for paid work. I thus purposively chose an "extreme" or "atypical" case that displays the characteristics of a phenomenon particularly vividly, enabling me to advance a theoretical argument about a phenomenon from a single case (Ragin, 1992; Yin, 2003).

I selected 24 of the 50 Young Lives children in Leki for focus groups. For each gender, I ensured the sample had a mix of children who did different types of work, either paid and/or subsistence work or chores. I also ensured a balance of children in three categories of educational attainment, those who had dropped out, were in the first four grades of primary school (lower than the number of years they should have attained if they enrolled at the right age and progressed through grades), or were in Grades 5 to 7 (having enrolled on time at age seven and progressed through a grade a year). The Young Lives sample was random within villages. To select respondents, I sorted children by survey identifier number and took the first children who fitted into each of the categories in Table 6.1. My sample is roughly typical of children in these categories in the village. There were four groups of children for focus groups, with six children in each group. Girls and boys were separated, but each group had a mix of children with different work and schooling statuses to ensure a diversity of opinions.

Each child participated in two focus group activities, conducted in the office if the village authority. In the first, children drew pictures of the activities they did and then ranked them in a group discussion according to various criteria, such as which activities were the most fun or the most tiring (Woodhead, 1998, 1999). This activity is widely used to gather working children's perspectives on the hazards and benefits of their work and its relationship to their schooling. In the second activity, each child drew a child of their own age and gender doing well at school, a child doing badly at school, or a child who had dropped out, and then described their picture to the group. This is an adaptation of a Wellbeing Exercise in the Young Lives qualitative component (Camfield, Crivello, and Woodhead, 2009).

[3] All names of children and study sites have been changed to protect the children and their families' confidentiality.

Table 6.1: Sample for qualitative work in Leki, July-September 2008 and April-May 2010

Schooling status	Participated in paid and/or subsistence work in 2008			Participated only in chores in 2008		
	Total	Focus groups only	Focus groups and interviews	Total	Focus groups only	Focus groups and interviews
Girls						
Grade 5-7	3	2	1	3	0	3
Grade 1-4	2	1	1	2	0	2
Dropped out	2	0	2	0*	0*	0*
Subtotal	7	3	4	5	0	5
Boys						
Grade 5-7	2	1	1	3	1	2
Grade 1-4	4	2	2	1	0	1
Dropped out	2	0	2	0*	0	0*
Subtotal	8	3	5	4	1	3
Total	15	6	9	9	1	8

*There were no girls or boys who had dropped out and were involved only in chores among the 50 children in the Young Lives sample in Leki.

Power dynamics when interviewing children are inescapable, especially in societies which marginalise children's views and are highly hierarchical (Boyden and Ennew, 1997; Punch, 2002; Hart and Tyrer, 2006). While "no method is inherently 'participatory"' (Beazley and Ennew, 2006, 192), activities which are often highly visual, involving, for example, mapping, ranking, or draw-and-tell techniques (Driessnack, 2006; Ennew and Morrow, 1994; Pridmore and Bendelow, 1995; Punch, 2002), encourage children not to be shy, to talk, and to enjoy the research activities. Draw-and-tell maintains children's interest by acting as a concrete frame for group discussions and ensures all children take turns to explain their own drawing (Mauthner, 1997, 26). Using visual data also enables children with limited literacy to participate (Clark and Moss, 2001).

After the focus groups, which also built rapport with participants (Eder and Fingerson, 2003; Keats, 1999; Crivello, Camfield, and Woodhead, 2009), I selected 17 of the 24 children for semi-structured interviews. There were still one or more children in each category, but I focused on children whose experiences related strongly to emerging hypotheses. We interviewed children outside, so children could not be overheard, but they were in sight of other people so they felt safe (Hershfield et al., 1983, 253), and sitting on the floor to make the interaction more informal (Tekola, Griffin, and Camfield, 2009).

I used the same process to select 10 of the 17 for follow-up interviews, home observations and interviews with children's mothers (two children's mothers had died, so we interviewed their stepmothers). Caregiver interviews were conducted at people's homes, so both parents usually participated, and we were careful to schedule interviews when children were not home. Informal participant observation was undertaken in children's homes and where

children worked. We also interviewed teachers, community leaders, managers of farms, and government officials such as the village agricultural extension agent and health worker.

Activities were conducted by an experienced fieldworker of the same gender as the children in the group in Oromiffa, the local language, from focus group and interview guides I developed. I was present in all activities and some translation occurred throughout so I could add questions or steer discussion. Interviews and focus groups were recorded and transcribed into English.

E. Sampling and activities in 2010

In 2010, I returned to Leki to revisit respondents. The fieldwork was conducted as part of a qualitative component conducted in five Young Lives survey sites, which was integrated into the quantitative survey of schools discussed in Chapter 7. I selected 13 children who were also part of the qualitative fieldwork in 2008, as shown in the Appendix. I could not interview all 17 as we had to ensure a balance of children attending different schools and children who had dropped out. Children and fieldworkers completed a variation of the Life-Course Timeline used in the Young Lives qualitative protocol (Crivello, Camfield, and Woodhead, 2009). The field researcher began by asking children about their schooling status in each year between ages 5 and 15. The timeline acted as a prompt for discussion about how they and their caregivers had felt about key transitions, such as joining school, moving from primary to secondary school, and changes in their work in the household and for pay (James, 2005; Christensen and James, 2000). Children were then asked about events that made them proud, and what had helped them to succeed, as well as events they considered setbacks. The protocol concluded with exploration of children's future aspirations and whether these were shared by their parents.

IV. The study site

Leki is a lakeside village of 2,835 people and 410 households (in 2008), two hours' walk from the nearest town and four hours drive from Addis Ababa. Of the 410 households 285 own land, on which they grow maize, wheat and teff (a cereal that is made into enjera, the staple flat bread). The chairperson of the village authority (the kebele) estimates that the average farmer owns four qarxii (one hectare) of land. Most households own goats and roughly half own cattle. The village economy is agricultural and until recently was subsistence-based.

Because of the lake, land around Leki is ideal for irrigated agriculture. In 2002, a commercial farm growing beans, peppers and asparagus for export to Italy was established. There are now five commercial vegetable farms. An NGO recruited 99 farmers in the village to form five irrigation co-operatives. These farmers can grow and sell a second harvest of peppers, tomatoes, or onions, which they sell to traders. An average-sized family can be fed for a year from the combination of maize or teff and cash income from half a hectare of land if it is irrigated, which is one-third of the land needed to feed them for a year if land is rainfed (kebele chairperson). In the words of the father of Geloye, one of the girls, "We produce maize, legumes and other crops within a short-time, and we can produce them as much as we want ... where before we wait for the seasonal rain."

Leki has a health post, a primary school, a church and a mosque. A flower farm that grows roses for export was established in 2007. Of the 49 households in the Young Lives older cohort in Leki in 2006, 49 per cent could access electricity. Communal pumps pipe up a steady underground water supply from the lake. The nearby town has a secondary school, three primary schools, a larger clinic and a number of NGOs. Of the 49 children in the Young Lives older cohort sample in Leki, 44 were from the Oromo ethnic group, while 4 were from ethnic minorities. Muslims were 12 in number, and Orthodox Christians 37.

A. School

In 2006, nearly all children in the Young Lives sample in Leki (94 per cent, 46 of the 49 children surveyed) were enrolled in school. In 2009, 81 per cent (39 of the 48 children surveyed) were enrolled in school. However, many children started school after age seven, the compulsory age of enrolment, because their parents could not pay schooling costs or needed children for work, or because parents or teachers thought children were too small physically for school.

Enrolment rates were not significantly different for boys and girls in either year. By 2009, more boys than girls have dropped out of school (7 boys of 27, compared to 2 girls of 21), but the difference is not statistically significant.

Most children (44 in 2006; 31 in 2009) attended the village primary school in Leki, which was built in 1973 and taught Grades 1 to 7. It added its Grade 7 class in 2007/08. Some children (4 in 2006, 8 in 2009) attended school in the nearby town, either because it was of better quality or because by 2009 they had reached Grade 8 and could no longer attend school

in the village. Some, like Geloye, commuted by foot or bicycle to school. A few, like Tatek and Galato, had parents who had moved to town and thus attended school there.

In both 2006 and 2009, the Leki village school taught only one shift, from 8:00 a.m. until 12:15 p.m. The school in town taught two shifts, both of four hours. If only school hours are considered, attending school was compatible with some work. Even so, some children did not attend regularly. Many came in the morning and left after the 10:00 a.m. break. Absence declined as children got older: in 2006 the average enrolled child in the Leki Young Lives sample missed 10.43 days of school, but by 2009 they missed only 3.18 days of school. Students who were often absent had dropped out by this stage. Teachers also reported high rates of dropout during the year, which is not captured by Young Lives data: by halfway through the 2007/08 school year, enrolment in Grades 1 and 2 had decreased by roughly one-third. Some children reported studying three or four hours a day besides school, even waking up early to study, but many did not report any studying.

Many respondents, including teachers, said part of the reason for poor attendance of students was a severe teacher shortage in the village school. In 2007/08 the school needed ten teachers. It had seven at the start of the year and lost four during the year for health reasons. Even when teachers attended school, they are often late: Kurabachew complains his teacher "kills our time" by not coming to school on time.

The village school lacked key resources, which may indicate that the quality of schooling was low. Classes were large, ranging from 50 to 100, and classrooms were poorly ventilated and dark, with shortages of desks and chairs. In 2007, government provided textbooks for each child for four subjects in Grades 1 to 5, but the school had not received textbooks for Grades 6 and 7.

Many students reported that the school in town was better. Chaltu moved from the Leki school to the school in town in Grade 3. She reported that teachers in town gave and marked assignments, whereas teachers in Leki did not do this. Teachers in town did not miss school often and if they did, another teacher substituted for them. Tufa wanted to move to school in town because the village school "was devoid of tables and chairs in the past years; it was only provided with such materials in the past year. On top of that, the classes are not clean and inviting." The facilities in Grades 1 and 2 were worse than in Grades 3 and 4. In 2010, Tigab did note that new classrooms had been built and the compound had been substantially cleaned and improved.

The school in Leki did not require payments for fees or uniforms. However, children had to buy exercise books, stationery and adequate clothes and shoes. The school in the nearby town was more expensive for families: according to Ayu's mother, students had to purchase uniforms and different exercise books for each subject and pay for cards to use the school library.

B. Work

In focus groups and interviews, children said that most children aged both 12 and 15 did chores and were involved in subsistence work. This agrees with other qualitative research in Ethiopia (Camfield, 2010; Abebe, 2007; Poluha, 2007b). Most of the children interviewed whose families owned land helped their families to farm. Boys helped with weeding, ploughing and harvesting and guarded crops from birds and cattle. Girls assisted with harvesting and sowing seed. Children of both genders started herding smaller animals at around age five. From age seven, boys herded cattle, although girls did this if there were no young boys in their families.

Girls did most of the household chores: they cleaned, went to market, washed clothes and made enjera and wot (sauce). Both girls and boys fetched water from the pump and collected firewood. While both genders took care of younger siblings, girls were responsible for caring for sick family members.

Vegetable farming around Leki is labour intensive. Vegetable seedlings are grown in pots and transplanted individually into the fields, which takes longer than planting maize. Tomatoes and onions are perishable and need to be harvested quickly. According to the farm manager, ETECO hires 65 permanent labourers for most of the year, and, additionally, up to 1,000 casual labourers during vegetable harvesting.

Children worked unpaid for their own families if they had irrigated land. But, unlike in many other rural areas, there were also opportunities for children to work as casual labourers for pay. Both commercial vegetable farmers and families with irrigated land often hired children. The ETECO farm foreman, interviewed separately from the manager, estimated that 10 to 15 per cent of the harvest workforce is children. Even children whose families had irrigated land worked for pay on commercial farms. On vegetable farms, girls transplanted seedlings and did weeding, hoeing and harvesting. Boys prepared the land, scared away birds

and fetched extra water. The duration, intensity, and flexibility of work varied depending on the employer.

Table 6.2 describes children's activities in Leki and across the 584 rural children in Young Lives. Across the whole rural sample, by the time children are 15, almost all have been to school at some point, and 13 per cent have dropped out. More girls do more than two hours of chores or caring work than boys, whether or not this is combined with subsistence work. As girls get older, fewer of them do subsistence work at all and more just do large numbers of household chores. When children are 12 more boys than girls do paid work, but this difference has disappeared when children are 15. Almost all children spend a substantial portion of time on tasks, even if it is only chores in the household: only 3 per cent of 12 year olds and 1 per cent of 15 year olds are in school and doing no other activities.

In Leki, a higher proportion of children have dropped out of school by the time they are 15 than in the rural sample as a whole: 19 per cent compared to 13 per cent, respectively. Much higher proportions of children do paid work than in the rest of the sample: 47 per cent of 12 year olds compared to 8 per cent, and 42 per cent of 15 year olds compared to 8 per cent.

V. Social roles

A. An educated person

Being a student and being educated are highly valued identities among children. When asked what she wanted to do when older, Shonah said, "I want to continue with my education and become an educated person" (2010). Quubsaa also thought education "would be useful for the future. If you are asked something, you can't answer anything unless you are an educated person" (2008).[4]

A.1 Opportunities for paid employment and to contribute to one's community

In standard economic models of decisions to invest in education, the likely future returns to education are a key consideration. Children certainly valued education because it opened up opportunities for employment. Ayu said, "If you complete Grade 10, the government takes you to Shashemene [the nearest larger town] and enables you to join college and continue with

[4]Parents also used this phrasing, so it appears to be relatively common. Quubsaa's mother wanted him to attend school to become an "educated person"; Kuftu's mother hoped she would complete her education and get "a job that can get regular payment...work that an educated person works"; and Tufa's mother says, "an educated person is good; he can support himself and his country".

Table 6.2: Children's combination of activities, Ethiopia 2006 and 2009

Variable		In 2006				In 2009			
		In all 13 sites (n=584)			In Leki (n=49)	In all 13 sites (n=572)			In Leki (n=48)
		Girls	Boys	Girls and Boys	Girls and Boys	Girls	Boys	Girls and Boys	Girls and Boys
% not in school									
	Never enrolled	4.24	8.64	6.51	4.08	1.45	3.70	2.62	0.00
	Dropped out	1.77	0.66	1.20	2.04	12.00	13.13	12.59	18.75
% in school	and doing paid work*	5.30	10.30	7.88	46.94	8.36	7.41	7.87	41.67
	and doing subsistence work								
	and more than two hours of chores/care	34.28	28.90	31.51	16.33	20.00	27.27	23.78	6.25
	and less than two hours of chores/care	7.77	37.87	23.29	10.20	1.09	34.01	18.18	16.67
	and doing no subsistence work								
	but more than two hours of chores/care	43.11	10.96	26.54	16.33	56.00	12.79	33.57	16.67
	and doing no other activities	3.53	2.66	3.08	4.08	1.09	1.68	1.40	0.00
		100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00

Source: Young Lives Older Cohort rural sample, 2006 and 2009. *They may also spend time on other types of tasks.

your education... if you continue with your education, you get money from the government" (2008). Ramato thought that if one completed school, "you may also get to medicate patients or do other government office works, if you are fortunate enough when you complete your education" (2008). Tatek's siblings hoped he would "study and become someone" (2010).

Children thought that becoming educated was valuable also because it enabled them to contribute to their community in various ways. Although this was perhaps unsurprising, because most educated people they knew were doctors, nurses or teachers, it was nevertheless striking that 12 and 15 year olds already articulated this sentiment. Tatek said: "I want to be a doctor because there are many patients in Ethiopia. For instance, there are many people who are infected with HIV. If I fail to join medical school, I want to be a teacher because our country is yet to develop. I want to train children so that they become self-supportive" (2010). Kuftu wants to "become a doctor and make some contributions for the development of my country" (2008). Tigab says, "I want to teach illiterate people in the countryside" (2010). Senayit says, "I aspire to learn and to reach higher place such as to work at health care centre or to be a teacher... Then I will transfer what I know to students" (2010).

However, these hopes for and ideas about opportunities are not entirely concrete. Children have only a vague idea of what level of education is required for different jobs – Quubsaa assumes that the same qualification will enable him to be a teacher and to write prescriptions for medication – let alone what people in different occupations earn. Future opportunities do enter into children's decisions, but children are certainly not making decisions by comparing discounted future returns and current costs.

A.2 Mastery

Children also valued being students because they enjoyed the process of learning. Galato thinks that "understanding new things is an achievement" (2008). It "changed my attitude" and "improved my mental capability" (Chaltu 2008). Tigist enjoys going to school "because it gives me mental satisfaction" (2008). Even though, as discussed below, he is often absent, Hassen says that children who do not join school are "fools", which means "someone who cannot think" (2010).

Relatedly, children associated schooling with working hard and studying. In describing children doing well at school, different respondents in focus groups said, "He studies during exam season. Besides, he studies very hard to add to his knowledge" and "He doesn't disturb

the class; doesn't wander around, and goes to school on time regularly. He does his homework after school" (Boys Group One, 16 August 2008). In another group, two respondents said, "He stays behind and studies during recess time", "He asks small children to play outside so that they may not disturb him. Besides, he studies with his friends and may also study while herding cattle" (Boys Group Two, 24 August 2008). Tatek also placed a lot of responsibility for his understanding his subjects on himself:

> When I join grade nine, I want to be very serious on my education. I am not going to spend too much of my time on playing games. I also plan to ask questions without being afraid. In addition, I would ask my brother to buy me a dictionary... I have also determined to hold my rank [in the class].

Children argued that even children from poorer backgrounds could succeed, in part because they worked hard:

> Brilliant students could not be from a rich family. . . children from the poor work hard and earn their own money to come out from poverty (Girls Group Two, 26 August 2008).

Many children reported with pride instances when they had done well at school. Quubsaa remembers, "They used to tell me that I am scoring a good grade. Finally... they also called our names mentioning the ranks we got. I was called at the fourth rank and I was awarded a notebook and a pen" (2010). Senayit remembers being encouraged by scoring full marks on a test, when the teacher told the other students to clap for her.

Students are clearly affirmed by building positive relationships with their teachers. Senayit feels happy when "teachers clearly explain the lessons and ask us questions and we answer correctly. [This] strengthens the relationship between teachers and students." When asked what the school should have done to support children who had dropped out, girls said that teachers should "ask children questions and if they fail discuss with them. . . and give children easy activities and when they do that give them a harder one. By doing this it is possible to show them that they can" (Girls Group Two, 14 August 2008).

A.3 Anxiety and pressure

Akerlof and Kranton (2002) argue that children who do not conform to the dominant ideals of a school will stop exerting effort. However, frame-dependent theorists of identity in education

note that students often continue to try hard, but feel threatened and anxious. Students often described such experiences.

Some appear to be relatively minor anxieties. Tufa says, "I enjoy reading, thus I always raise my hand to read some texts for the class". However, "If the teacher emphasises on discussion I do not feel good" (2008). Similarly, Quubsaa, who had dropped out by 2010, said, "I had some tension in my mind. I didn't have any problem when I go home but the feeling comes back when I go to school."

However, other students feel ashamed, scared and anxious. When Kuftu does not do her homework, she says, "I feel that I am being below standard. I feel ashamed" (2010). Tatek is "afraid to ask questions. When the teacher asks if the lesson is clear, I respond yes due to fear". Senayit reports that, although she was 10th in class before she repeated a grade, now she was 29th. When her grade was read out, "I was crying. My friends performed well." She dislikes it when "the questions our teacher asks us are not clear to us hence we cannot answer her. Thus the communication between the teacher and students stops." In a relatively rigid and hierarchical school system, it becomes clear why this is a problem: the teacher may beat students when they fail to answer questions, and Senayit adds, "I also dislike the beating" (2008).

A.4 Characteristics of educated children

Children who attend school may also be seen as having parents who care for them, buy them the materials they need, have regard to their future, and prevent them from worrying about work (as described below). Children accordingly look to their parents to provide this support and to recognise its worth. Tigist says that children "should be provided with all the necessary materials such as clothes, books and pens...If their parents always buy them books and pens and other necessary materials, they can be successful in their education and they may meet their goals" (2010). Kuftu says, "Parents should say to their children: 'If it is something we can do, we promise you will get that. Think of tomorrow instead of today. Please don't stop going to school"' (2008).

Some parents do prioritise providing children with these items. Chaltu's mother likes "to have dressed her well, and send her to school; not to have her stressed." Tufa's mother wants him "to have food to eat. If he eats balanced diet, he will become mentally active and succeed in his education." Geloye's father says, "We care and think about her shoes, clothes,

bag, bicycles, pens and exercise book" and has tried to save for a bicycle so she can get to school quickly.

This may suggest why students place such priority on having the physical markers of belonging to this category, such as school bags and stationery. Children desire to dress and appear in accordance with this valued, high-status identity. Even though children have low chances of progressing through the full course of schooling, poorer children may therefore invest considerable effort in attending school and in purchasing the necessary materials, often working part time to pay for schooling costs, as described in later sections.

Students and schools were also associated with organisation, scheduling, punctuality and diligence. Geloye's mother explains that

> The community has organised individuals who check and control the attendance and behaviour of the teachers so that they should be good examples for the students. The school calls the parents and asks them to discuss on the attendance and behaviours of their children. Teachers point out students who do not attend schools and obey their teachers so that their parents give them special attention. We also instruct our children and try to make them productive citizens.

This is recognised by the students. Bona's teacher "teaches us properly, and also he comes on time, and tells us to come to class on time, and so we do" (2008). Here, socio-economic status comes strongly into play: children distinguished between those who attended the poorer quality village school and those who attended school in town. In 2008, Tufa wanted his parents to transfer him to the latter, because students there "are well-mannered, they do not disturb". In the village school, "There are many students who do not care for their friends and students in general. They intrude into our class and disturb us as we study." In the village, Galato's mother says that children play truant as "there is no stiff control".

Educated people are also seen to be clean. When asked what benefits schooling has given him so far, Tatek says proudly, "I used to eat without washing my hands. After I joined school, I started to wash my hands before I eat. My parents were impressed to see that." Shonah's mother similarly says that school "opens up your eyes. When they go to school, children keep themselves clean; keep the house of their family clean. They also advise us to keep our hygiene." Geloye learned "to keep our clothes clean at school" (2008).

Government also uses the schooling system to promote certain ideals. The Education Sector Development Programme III in 2005, for example, argued that universal primary

education was important because educated people could use family planning to reduce rapid population growth (Ministry of Education, 2005, 18-19). Government has also instituted legislation banning early marriage (Boyden, 2011). Senayit says that her teachers "encourage us, telling the advantage of education and the problems in getting married early". Tigist says, "Because I am educated, I would give birth only to two or three kids and carefully raise those children. I would increase the gap between the births of my children too" (2008).

A.5 The opinions of community members

We asked how people in the community feel about children going to school. Children often answered that the community's approval motivated their attendance. Geloye says, "The society says I attend the school properly and thus I will help myself and my parents if I keep up studying hard. They are also happy for my parents...Thus, I have a great ambition to complete my education and make my family and the society happy." Geloye says her neighbours "feel happy because their own kids would aspire to be like me. My action shapes the mindset of their children" (2008). Similarly, Senayit says that some of her neighbours "even send their small kids with me so that I can take care of them and show them their classes" (2008).

The opinions of parents provide some guide to the affirmation children receive from attending school. Parents argue that more educational opportunities are now available to their children: Geloye's mother thought education would "liberate our children" and enable them to "take out themselves from darkness and get themselves to light". Tatek's mother hoped he would be able to complete his schooling so that "he will become a great person on whom I forget my illiteracy; he will compensate my ignorance; that is my hope and title of my prayers...he has the possibility to become a man of his fortune." Kuftu's mother said, "An illiterate person is good for nothing; he cannot lead good life"and thought school would give her children "knowledge that enables them properly use their money and take care of their lives."

B. Contrasting identities

Children drew strong contrasts between the current activities and future trajectories of children in education, who would have opportunities for future formal employment, and children who were idle or "delinquent", "chase money", and would only be able to do irregular piece work

on farms. As recognised in the Akerlof and Kranton (2002) model, some children who do not fit the school's ideal instead choose other categories and exert low effort in school or drop out.

B.1 Table football and truancy

Groups of children who were known to miss school engaged in particular activities, such as paying to watch television in the local video house or playing jotoni or table football, often for money. Tatek, who lived in the town near Leki with his brother, disapproved of these activities:

> I like education because education helps children to become someone. What I don't like is spending time in inappropriate places like watching video and playing jotoni. Such activities lead children to delinquency. I like that we study and play with schedules. It is bad to misbehave in school compound such as missing class (2010).

Geloye thought that children who played jotoni sometimes stole money from their parents to play. They lied to their parents, pretending to go to study with their friends while they spent the whole day playing the game.

B.2 "Chasing money" "here and there"

Children and their parents felt that paid work was a distraction from education. Working children were described as "worshipping money" (Senayit's parents), "money-focused" (Geloye 2008), or "money-enthusiasts" (teacher Dawit, 2008).

At worst, it would lead them to drop out. Tufa's mother said that "If students get accustomed to earning money, they incline towards trade and quit education". But even those who continued would be distracted. In 2008, Geloye did not work for pay because, if she "got used to income-generating jobs", her "attention goes where there is the job and the money". Tigab firmly described the options:

> Of course, I know it is not possible to work and learn side by side and become successful, one has to choose either the job or the learning (2010).

That was why, in Kuftu's view, parents should provide them with books, pens and clothes: so that they could "emphasise their education with undivided minds".

Those children who did manage to go to school and work in the afternoon or on weekends felt the distraction: "While I am in class or about doing the work while I am studying; this definitely affects my learning", whereas children who did not work did not have "any other thing to think about; they only concentrate on their studies" (Senayit 2008).

A number of parents described children who were not paying attention to schooling as "going here and there" and "wanderers". Hassen often leaves school after the 10 o'clock break and runs errands for people in the village, or earns money rowing boats across the lake. His mother is frustrated that he "throws his books away" and goes to work. "Search of money affects his mind." Similarly, Chaltu's mother says, "My pride comes when my children learn their education properly and become a good person, we don't favour their becoming wanderers."

B.3 "The last option": becoming a farmer

The option of staying in subsistence agriculture was viewed as a much less desirable option, although children often raised that it might not be possible to obtain a professional job. Kurabachew said that his goal for the future was "to complete my education and get a good job and have my own car". He would become a farmer only "if I fail to meet my goal". For children who were not successful in completing their education, "their last option is becoming farmers" (2008).

Others were even more pragmatic. Tufa noted that "parents always want their children to prioritise their education" so that "they may be employed". However, "If they are not successful in their education they work as daily labourers", so children should "be accustomed to doing works and successfully complete their education. It is better if they involve in both" (2010).

Many parents shared the opinions of their children that being educated was a better option than farming. Hassen's mother said that "education is not like farming, farms may fail some years, but education doesn't fail any year".

C. An independent earner

Anthropologists and sociologists (Nieuwenhuys, 1994; Bourdillon et al., 2011; Liebel, 2004; Reynolds, 1991), including in Ethiopia (Abebe, 2007; Abebe and Kjorholt, 2008; Camfield, 2009, 2011), document that children find meaning in their identities as earners and contributors to

their families. Children gain respect for working and are often proud of their work, which becomes a core part of identity (Woodhead, 1998, 56, 73). In particular, direct costs of schooling are often a barrier to enrolment (Burra, 1995, 181), so children often work to pay the direct and indirect costs of schooling (Schiefelbein, 1997).

C.1 School materials

Many children used their wages to buy school materials. Poorer children may have to pause schooling for a year to save. In 2008, two children interviewed had dropped out of school because their parents could not afford these materials, and it was common that "parents ask their children to work this year and go to school next year due to lack of money for materials" (Boys Group 2, 25 August 2008). Children then worked and saved money and returned to school the next year. Kurabachew's fishing did not yield enough money for materials in 2007: "I quit the school in this year and continue in the coming year" (2008). Kurabachew used money from his paid work when teachers asked for contributions to the school (2010). In Grade 3, when her mother was ill, Senayit "used up pens, books and clothes and we did not afford to buy her those materials. Thus she was forced to quit" (Senayit's father, 2008).

C.2 An opportunity to support oneself, be independent, and contribute

Children in this sample did not seek to earn only when there was no alternative. They also articulated a strong sense of obligation to contribute to their families by buying things they needed, like clothes and soap. One girl said: "Even when their parents can afford living, children say 'I have to help them, myself at least"' (Girls Group One, 24 August 2008). Kuftu was asked who should pay for children's financial needs and said, "Children should work and buy for themselves; the parents may support them if they are financially capable...They should not let their parents suffer, that is bad" (2010).

Children were proud of their independence. Kurabachew was from one of the wealthier families in the village and agreed that his parents would supply what he needed for school. But he felt obliged to contribute: "ultimately it is [children's] duty to do these jobs...children must help themselves, and fathers or mothers have to fulfil the remaining... Except few things, I buy most of the things I need myself. I help them when they face a problem" (2008).

Kuftu also says that she and her two brothers all do paid work in their spare time and "support themselves" (2010). The benefits are that "you earn money and it also helps you

build physical strength". Kurabachew said that he gave money from his work to his family because "If I don't contribute something it is too difficult for my parents to run the family" (2008). One focus group of girls discussed paid work as a profession. Two girls emphasised the importance of not leaving one's house unclean. But another girl answered that "You need a profession like working on farm ground. We have to work on onion farm. If we don't do that, how can we get money just by cleaning our houses?" (Girls Group Two, 15 August 2008).

Similarly, some parents viewed paid work as an opportunity: Hassen's mother said that children in her time were only able to help their parents, but children her son's age can both go to school and be paid for their work. Tatek's mother said "Their minds still aim for education, but they also think of how to cover their daily bread." Senayit's parents said that, while "...we used to work only for our parents. These days you can get the result of your work."

C.3 Resolving problems: of one's own, one's family, and others

Children saw earning wages as a way of tackling financial obstacles. Senayit said, "I will reside with my parents and work on vegetable [farms] until the obstacle will be removed" (2008). In 2010, she still relied on paid work to cover her schooling expenses: "When I don't have notebooks, they buy for me late. If not, I work in paid work" (2010). Although Tatek's brother has supported his schooling thus far, Tatek said, "I am not going to rely only on him if he tells me that he can't afford it. For instance, I will save money from now on" (2010).

This was particularly true when families faced difficulties, like illness. Girls in a group explained that when their mothers are sick, "I have to work and with the money I have to buy medicine" (Girls Group Two, 14 August 2008). When Senayit's father was imprisoned, she dropped out of school for a year to work to cover the family's expenses.

Children also contribute when others in the community face misfortune. Kuftu was asked how her paid work could help schooling. She mentioned that it enabled her to buy her own educational materials, but added, "My classmates have the habit to contribute money for the students who felt sick to help the student get treated; when I have no money to contribute I feel unhappy." Her paid work enabled the education of others. Quubsaa mentioned giving money to school collections for the blind.

C.4 Community perspective

There was disagreement about whether working for pay was appropriate for children. While Quubsaa says "Everybody thinks" that children should work for pay, his mother says "Different people may think differently". Kurabachew thought that the issue was one of whether children were physically capable of working, a concern echoed by others: "The society does not allow little children to do paid works since they are not physically mature. They classify work... according to physical capacity of the children" (2010). Norms about work may also have changed over time as economic activities have changed. Tufa's mother says,

> it was not morally acceptable to work paid works in the days I was a child. Our parents used to provide us with whatever we needed. There is economic problem these days. The prices of all goods have escalated...it is impossible for parents to run the household [on their own]. All members of the family should earn their livelihoods...

Some working children describe interactions with community members who strongly advocated for their continued school attendance. Tigab says, "Whenever they see me working on school days, they tell me not to repeat it again" (2010). Senayit dropped out of school in 2009 to work when her father was imprisoned. She described her interaction with a man

> ...who knows me at the workplace. On the next day he come up with eight exercise books as he think I dropped out school due to lack of exercise books. He gave me the exercise books and advised me to attend school as working there has no future value.

Some parents, like Chaltu's, do not want their children to work for pay because of worries that they will become distracted. But others merely want to ensure that the two do not conflict: Quubsaa's mother says, "I don't want them to work at the expense of their school time. They may work during their spare time." Indeed, parents and children believed that working for pay was a good use of time, compared to being idle: Galato's mother emphasised that "he doesn't sit idle": he goes to school, and he works when there are jobs available. Quubsaa's brother "saw me sitting idle and asked me to work for him" on his cabbage farm.

Other parents emphasised that their children's contribution to the family was essential. Senayit's mother explained that "His father and I are not physically strong to work as daily

labourers. We cannot earn money. He knows I'm helpless. That is why he is helping himself while attending school" (2008). In similar vein, Tufa's mother says,

> A person cannot eat food without working; he has to earn his livelihood. [Society] wants the children to be accustomed to doing work and successfully complete their education.

D. An obedient child

Parents and children both argued that obeying parents' orders to do work for the household, either in subsistence farming or chores at home, was important. Although anthropological studies find children question hierarchies and norms of obedience by negotiating with their parents (Poluha, 2004; Porter, 1996), children in this sample did not report such behaviour in interviews and picking this up may require more detailed anthropological observation of behaviour in addition to reports from the children.

Children were asked what they did if their parents asked them to do a task. Some simply obeyed: Senayit said, "If they [my parents] order me to work, I work. I cannot disobey them" (2008). Chaltu replied, "My mother gives me an order and I have to finish the work" (2010). Some felt they obeyed their parents because they felt a sense of obligation: Senayit said, "I am responsible to serve the family in the home." And Shonah argued, "There is no one to help [my mother] with the work and she cannot do everything by herself" (2010). Others felt it was desirable: Tigab says that ploughing for his family "is based on my willingness and not imposed on me by someone...I could have avoided it" (2008). And yet others did it to please: Geloye says, "The community and parents are happy if children do what they are assigned to do" (2008).

Whatever the rationale, assistance to one's family was praised by parents. Shonah's mother says of her, "She helps me with all tasks to her level best... I rely on her and God. She obeys what ever I tell her." Kuftu's mother said, "She does the works because she obeys me and likes to assist me in doing the household tasks", while Geloye's father said, "She just works as it is her responsibility to help her mother". Tufa's mother believes "The society wants children to do good things. They should not steal and disobey their parents."

However, some children did resent having to obey their parents, particularly because they felt doing chores detracted from their schooling. Geloye wanted to leave home to attend school in the nearby town:

> If I sit down to do my homework or for reading, they order me to do something, I would be obliged to stop reading and do what I am ordered to. So the jobs affect my studies... Here [in the village], if parents order us to do some task, we don't disobey, and we do it. Should we think of our studies or of obeying family orders? That is why I want to go to town" (2010).

Although obedience was generally viewed as valuable, in some instances a sense of responsibility was reinforced by the use of force by parents. Geloye, for example, cannot avoid doing household chores because "If I were the only child of my father and mother, and they were very old people, who would assist them in day to day chores?" but added "They would beat me if I try...everyone must do what he is expected to do". Quubsaa said, "No one compels you....They say that it is up to me. But if he does not do what he is told,"They would chase you out, refuse to give you food, and even beat you" (2008).

There was more variation in whether parents ordered children to be involved in paid work. Kuftu said her parents do ask her to work for pay but "They do not force me to be involved in the paid work. Primarily I am the one who wants to work" (2010). Senayit says, "No one forces me to do [work for pay], I can quit any time if I want to." On the other hand, Ayu said,

> If I become reluctant to do the paid work, she [my mother] forces me to be engaged. She says nothing can be gained sitting idle...If she orders me do the paid work, I do it. Similarly, if she commands me not to involve in the work I simply obey her (2008).

At the other end of the spectrum, some parents prohibited their children from working for pay, which the children did not agree with. Geloye's family did not permit her to work, although she would prefer to work for pay compared to housework because "you do little and you earn more money".

E. Becoming an adult

Although often a reaction to poverty, work has other purposes, so often children who are not poor participate in work. Work teaches skills, even when performing what might be considered boring and repetitive tasks, so children may view it as more immediately valuable than school (Boyden, Ling, and Myers, 1998, 266). Children's work may prepare them for adulthood and make them less dependent on their parents (Zelizer, 1985).

One focus group of girls ranked preparing *enjera* (the staple food) as the most important skill:

> It would be an insult if the woman marries without the skill to prepare enjera. We are also responsible to prepare enjera when our mothers are not at home. Mothers may come with guests and it is our responsibility to receive guests with our skills (Girls Group Two, 15 August 2008).

Preparing wot (sauce) is second because "without it enjera is nothing. We will be insulted if we fail to make it delicious." Preparing coffee is third because "It is an important skill for marriage." Indeed, "Without knowledge of it you won't be a good wife. The husband may be forced to marry another" (Girls Group Two, 15 August 2008). Similarly, boys ranked ploughing as the activity that taught them the most important skills, and took considerable pride in how early they had learned to plough.

Many children said that they would ask their own children to do the household tasks they were currently doing. Kuftu said parents want their children to do chores "to have work experience" (2010). Geloye thought it was important that children have to "learn jobs gradually; after learning cleaning the house and making coffee, they have to learn how to bake bread also through time" (2008). They "should know the housework very well as education is not only enough for her future" (2008). Similarly, her parents said that doing housework "will be a lesson for her future, and can be an exercise which will strengthen her". Quubsaa's mother reported that "we might ask to bake kita (a type of bread) to get the experience". Tufa's mother said, They do household tasks to accustom themselves to domestic chores since it is part of daily life" (2008).

F. Gender roles

Akerlof and Kranton (2000) show that gender sorting into different occupations often arises because certain tasks are not perceived as appropriate for men or for women. In Leki, different types of domestic chores are divided fairly rigidly between genders, with girls taking on most of them. All 17 interviewees agreed girls and boys did different tasks and used domestic chores as examples. As Geloye said, "A man can not bake *budden* [bread], cook *wat* [sauce] or make coffee and women cannot till the land." Other Ethiopian research describes how rural children learn gender-specific tasks from age five (Poluha, 2007a). Girls clean, grind crops,

spin and weed fields. Boys run errands, look after animals, and start ploughing around the age of 14. Both genders fetch water.

Existing perceptions about the types of tasks appropriate for each gender may influence how new occupations are divided between genders in times of economic change (Akerlof and Kranton, 2000). Girls and boys were allocated different paying tasks on the vegetable farms: "Boys water the vegetables and girls plant the seedlings and gather vegetables. The boys take the containers with vegetables to the warehouse" (Senayit 2008). Women's work is often conceived as less demanding and paid less (Akerlof and Kranton, 2000). Senayit, a girl, notes, "If we gather and fill vegetables in two containers they pay us two birr, whereas if boys carry two containers of vegetables to the warehouse they pay them three birr."

People justify task divisions with reference to differences in traits between genders. The manager of the NGO who supported irrigation co-operatives said girls were preferred for most tasks on vegetable farms: "They are taking care, especially for seedlings. They can perform this job properly. Also they are patient. It takes a long time" (2008). The leader of the burial association (*idir*) said, "Girls do their assignments cautiously and diligently and do not abuse things as boys do. That is why they prefer girls to boys" (2008).

Many children noted that girls work longer hours than boys: Ayu said, "Boys work for longer hours than girls in the paid work. But girls must also do household chores when they come home" (2008) and Kuftu noted that boys did either subsistence work or worked for pay, while girls did both domestic chores and work for pay (2008). Quubsaa, a boy, thought that this was because "Girls don't like to sit idle. They work a lot." Tatek argued, "In their educational performance, boys are better than girls because girls do a lot more domestic work" (2008). Yet their intrinsic diligence protects their school achievement, while boys are distracted by leisure: "Irrespective of this burden, girls do good, they are attentive in their studies. But boys do not concentrate and they forget what they learned while they play" (Tatek 2008).

Divisions of labour were changing. Boys who did not have sisters had often helped their mothers with domestic tasks: as the mother of Hassen (a boy) said, "Since I don't have any daughter, I will use him as a daughter." Galato and Kurabachew also had no sisters and therefore did household chores. Quubsaa's mother noted in addition that, "In the past, we didn't support our husbands with farm work. However, we are assisting them now. Our husbands don't know how to assist us with some work at home. But our male children are assisting us. Some boil tea or even cook food."

It is difficult to attribute changes in gender roles to specific policies as many factors contribute to any change. However, government's civics curriculum promotes a gender-neutral division of household tasks (Awan, 2007; Poluha, 2007b; Binyam, 2007). Girls' clubs have recently set up in schools to attempt to reduce dropout among teenage girls, and these promote division of tasks in the household (Jennings et al., 2011).

VI. Conflicting social roles

A. School and work for pay

There are complementarities between working for pay and schooling. Short school days under the shift system facilitate work before and after school (Nieuwenhuys, 1994). Many poorer children used their wages to buy school materials: "to survive and to attend school" (Boys Group Two, 24 August 2008). Ayu said work for pay "has positive impact [on schooling]. I commonly buy books and pens with the money I get working as daily labourer." She felt it was possible to balance the two activities: "I study as soon as I come home. After I finish, I go to the place of paid work. At night I do homework" (2008). Quubsaa said his work was "helpful" to his schooling: "For instance, if a book [the borrowed textbook] is torn and I am asked to pay, I pay from the income I earn from the daily labour" (2008).

Some children said their parents could not afford to pay for their schooling costs. If Kurabachew does not do paid work "The option is only quitting school. I quit the school in this year and continue in the coming year" (2008). Indeed, he did not make enough money from fishing in 2007 and he dropped out for the year. In Grade 3, when her mother was ill, Senayit "used up pens, books and clothes and we did not afford to buy her those materials. Thus she was forced to quit" (Senayit's father, 2008). This was a common pattern (Boys Group 2, 25 August 2008).

However, in combining work for pay with schooling, children often had to choose between the prescriptions of their identities as students as workers and contributors to their families. I already highlighted that children worried about finding jobs, which affected their concentration, even while at school. They worried because opportunities for paid work were scarce: girls said, "We cannot even get the job opportunity even when we want to do it" (Girls Group Two, 13 August 2008) and boys said "Sometimes children may compete over the work; they may want to work more" (Boys Group Two, 22 August 2008). Foremen "give priority to

the physically strong and experienced. They are reluctant to employ children" (Kurabachew 2008). This was particularly true for certain desirable jobs, such as picking tomatoes, which paid better than scaring birds away from crops (Tigab 2008).

The scarcity of jobs also meant children elected to miss some school if it conflicted with work (Girls Group Two, 15 August 2008). Senayit said this affected her results: "If we support ourselves by working as daily labourers, we may lose the lessons the other students have been taught ... and score less in tests."

Children often came late to school because of work and were punished, as in Woodhead (1998, 20) and Nieuwenhuys (1994, 70). Teachers disapproved of children's working for pay: one teacher, Dawit, said working children were "money-enthusiastic" and that "their parents do not push them to go to school as they value the daily money the children earn" (2008). They penalised late students. Senayit said, "Teachers close the gate against us so that we cannot disturb the class. We kill two or more periods just wandering in the fields. Sometimes teachers beat us ... order us to collect rubbish and clean the compound." Quubsaa mentioned that the village school hired a guard:

> He shuts the gate against students who come late to school. Sometime he allows us to enter and other times he does not allow us to ... I came while the other students were learning. The teacher asked me to kneel down for a while and later allowed me to go to my desk and take notes (2008).

Whereas Ayu simply said, "We stay outside of the school compound when we go to school late. Since it is due to our fault we accept staying outside of class" (2008).

Finally, children found work on commercial vegetable farms was particularly tiring. Girls said planting onions was worst: "You do it stooped over so you feel pain in your back" (Girls Group One, 13 August 2008). Some boys said, "Paid work has a huge impact. It is heavy and beyond our capacity" (Boys Group Two, 24 August 2008). Dasse, a boy, said, "The work in the commercial farms is difficult, and you can't take a break, you have to work all the ten hours in the sun. But with the individual farms you can take breaks and go home earlier."

Performing tiring tasks made it difficult to participate fully in school: "Whenever we go to school after doing heavy tasks, we cannot easily follow the lecture in the class" (Boys Group One, 22 August 2008). Ayu, a girl, said, "When I come home from the place of work I feel tired and fail to do assignments." A boy said, "If we have to work on the farm weeding, we go to bed early so that we may wake up in the night to study. We ask our parents to wake

us up." One of the teachers agreed: working children "come without doing their homework ... When we ask their friends after class, they tell us that the student was working on some vegetable farm the previous day." Girls complained they got more tired than boys as they planted and hoed and "spent the whole day bending over" while the boys could walk around or sit (Tigist 2008).

As outlined earlier, children recognised that their work facilitated their school attendance and made an important economic contribution to their families. However, many children expressed a preference for prioritising one activity over another. Kuftu said, "If it is possible I want to emphasise only on my education" (2008). Tufa said work did affect his education but "I have no alternative. I have to work and earn money" (2010). In 2008, he had the same opinion: "Working as daily labourer has impact on education because students miss school days to work. It creates conflicts. Doing different work at the same time is not good. It is better to focus on education." Tigab said:

> I want to fully concentrate on my schooling...I do this work to fulfil my need; if my family had enough wealth, I wouldn't have done. You know, the jobs are hurting. I am not doing these things voluntarily (2008).

B. Work at home and school

In contrast, many children thought it was possible to fulfil both their obligations at home and as students. Work for families could be divided into small chunks of time, which made it possible to combine with schooling: "Concerning the household work ... parents don't refuse to send you to school though you don't finish the work. You can finish the work after school. That is not the case with the paid work" (Boys Group Two, 24 August 2008). If grazing was plentiful, herding took place after school. Children could study while herding: one boy said: "When my father tells me to look after the cattle, I feel happy because I can read my books" (Boys Group Two, 25 August 2008). Senayit said "House chores do not bring serious impact on my education. I study my books and do assignments while or after I carry on the indoor works. It is the pay work that consumes much time and affects education" (2008). Shonah said "it [domestic work] doesn't take my time. We fetch water from nearby. Cooking is not that time consuming" (2010).

However, some home tasks, particularly those done by girls, took longer and were harder to combine with homework. "Girls are mostly busy and consequently get weak in their

education. The boys may take their exercise books when they go to herd cattle. But if girls try to study while they bake enjera, they may forget and the food may burn" (Girls Group Two, 24 August 2008). Another girl agreed, saying, "Making enjera has a lot of processes. It requires collecting firewood, mixing the flour with water and a lot more processes all at once". While fetching water was quick and could take place before or after school (Girls Group One, 12 August 2008), collecting firewood required travelling to the forest some way from the village. One girl highlighted how domestic chores had to be juggled with doing homework:

> She have to wake up early and support her family until the time of her school, she have to work whatever is ordered by her parents, she might have to fetch water or go shopping. In the evening after schooling she have to do the same before going to study or bed (Girls Group Two, 14 August 2008).

Boys described with some admiration how girls juggled studying with chores. "Girls study while boiling coffee... She studies even while cooking... She comes home from school at one pm and eats lunch. Then, she cleans the house, washes clothes, fetches water and collects firewood. After she finishes her assignments, she cooks" (Boys Group Two, 24 August 2008).

Girls also took on the bulk of responsibility for child care. Chaltu cared for her younger siblings as her mother was sick. She said:

> I couldn't study due to the work. I try to finish one work then pass to another one. But it doesn't finish quickly. For instance, if I finish Afan Oromo, the other assignments wait for me. Sometimes I am busy with the house chores until 10 pm and continue to do my assignments until 3 am. It is seriously affecting me. My brothers don't want to carry the children fearing that the babies pass stool on them (2010).

Given this gender assignment, boys felt more in control of their time: "They [teachers] teach us to use our time economically and finish the work we need to do so that we can have time for study" (Boys Group Two, 25 August 2008). "If students plan... they don't fail in their exams. What matters most is studying" (Boys Group Two, 24 August 2008).

VII. Conclusion

The fact that concepts of identity have predictive power raises the question of which of the theoretical models discussed can best capture the nuances of identity evident in the interviews

and discussions, and the extent of modification to the assumptions made by standard models that may be required.

A. Accounting for multiple identities

One issue is whether the behaviour related to identity, as described by the students, is better understood using the Akerlof and Kranton (2000) model, which conceives of individuals as having one "fixed self" that has similar preferences for complying with particular social roles across many contexts, or "frame-dependent" theories of identity (Hoff and Pandey, 2014) proposed by behavioural economists, in which identity is conceived of as a multitude of mental models or schemata, entailing different preference sets across contexts.

The key difference arises when there are two or more identities which have prescriptions relevant for a particular choice and those prescriptions conflict. Frame-dependent models of identity readily acknowledge that people face different and often conflicting expectations from identities which they hold simultaneously and assume that the immediate context will influence their choices. However, they assume that people respond almost automatically to cues in the situation and, depending on the extent to which a particular identity is cued, may make very different decisions in the same context. Preferences about whether to comply with a social role are thus highly context-dependent, unstable and inconsistent, implying that one needs to adopt a behavioural economics perspective and substantially modify assumptions in the standard model about preferences.

I argue this does not accurately characterise how children are balancing trade-offs between work and school. Indeed, much of the frame-dependent literature describes laboratory situations where subjects are asked to make snap judgements without real consequences and these may not resemble real life decisions. Children's balancing of conflicting prescriptions about how they ought to behave is often conscious and deliberate: as the examples vividly show, they list costs and benefits of different types of work; they explain their prioritisation of work for pay over school or vice versa if the two compete; they note where they have made less than ideal decisions because material constraints like poverty mean that they cannot realise their preferences of not working. Indeed, having to make a choice between two sets of priorities, both of which are important, in itself influences their utility. Conversely, they may gain utility when no choices or trade-offs are required.

Indeed, the very cultural sociologists whom frame-dependent behavioural economists cite clarify that people have both automatic and deliberative modes of decision-making and "override programmed modes of thought to think critically and reflexively" (DiMaggio, 1997, 271). They are more likely to do this when their attention is attracted to a problem, when a problem has strong moral salience, or when existing schemata do not provide guidance for behaviour. Both criteria apply in situations where social roles conflict. In their quest to apply behavioural economics to new topics, it appears frame-dependent theorists may have over-interpreted sociological evidence as arguing that identity works only through automatic thinking.

Relatedly, it seems that children's different identities are underpinned by a consistent sense of self. Girls' identity as students and their desire to do homework conflicts with their domestic obligations and requires them to form a preference. It is precisely because their identity as students is still salient that they articulate any conflict at all. In a frame-dependent model, their identity as good children would simply be evoked as salient at home and would override their desire to go to school.

Their core sense of self also enables them to prioritise, set goals, and make choices at a higher, more abstract level than immediate situational pressures. Children who decide to work for particular employers because they are allowed to leave work to get to school on time are choosing to balance their two identities. They choose not to enter a work context where they might be punished for leaving work early, or the school context where they might be punished for arriving late.

Sociologists working on identity also suggest a strong underlying sense of self. They argue people's multiple social roles are incorporated into their self-image, which is carried from situation to situation and do not see theories which emphasise roles internalised into one's self-image and theories which emphasise situational context as oppositional (Owens, Robinson, and Smith-Lovin, 2010, 485). Behavioural economists may also be over-zealous in claiming that identity theory in sociology is highly context-specific.

Akerlof and Kranton (2000) do not account for membership of multiple social categories or how individuals choose which prescription to obey when prescriptions from different identities conflict. They do concede that their model could be extended. They even allow that identities may be context-specific: "When an individual's identity is associated with multiple social categories, "individual behaviour would depend on what sociologists would call the

'situation': who is matched with whom and in what context" (Akerlof and Kranton, 2000, 731). However, their overall model is be more amenable to accommodating an underlying and consistent sense of self, and a stable preference ranking among different identities for each context in an individual's life.

This has the related implication that using concepts of identity does not require major modifications to standard models. Standard models can even allow that preferences are context-specific. To ensure stability and context independence, alternatives can be specified not as bundles of goods, but as bundles which capture everything affecting an agent's decision (Arrow, 1970). If one's choice of drink depends on both one's taste and the weather, one could specify alternatives as beer in summer, beer in winter, tea in summer and tea in winter. An agent's preferences over that set of alternatives would be stable, consistent and context-independent. It is in this manner that Ethiopian students evidently draw on different roles at different times. They may also prefer to prioritise one role in some instances and other roles in another instance. None of these behaviours necessarily reflects unstable, context-dependent or inconsistent preferences; and indeed such behaviours will still be consistent with the standard model.

B. Altruism

If identity is to be understood as a preference for complying with a social role, rather than a mental model, one should consider whether such preferences are self-interested or altruistic. Economists commonly assume that people prefer to maximise their material rewards (Teschl, 2013).

I argued in Chapter 3 that Sen (1977; 2007) differentiates between self-interested preferences, sympathy and commitment. The utility of individuals who are sympathetic is affected by the effect of their actions on others. However, individuals still act to maximise their own utility. Akerlof and Kranton (2000) allow that people's utility is affected by the effects of their behaviour on others. People may avoid disobeying prescriptions about behaviour in their category, whether or not they receive concrete censure from other people, because they want to avoid causing discomfort in other people. However, they avoid causing discomfort because it is ultimately in their own interest, even if only in the long run: they may value being part of a group or having the status that comes with group membership. Like most agents in standard models, they ultimately act to maximise their own utility.

In contrast, Sen (1977; 1985; 2007) suggests that adhering to social norms, including those related to one's identity or social role, is more like an action in accordance with a "commitment" than a self-interested or even sympathetic action. Commitments are considerations like morality or concern for others which do not affect a person's own welfare, however broadly it is defined.

Many of the preferences for complying with social roles articulated by the Ethiopian children are much more like commitments than self-interested preferences. They contribute financially to their families and to others who are suffering misfortune, look after their siblings, and do extensive work in the home. They express why they do these actions in terms of responsibilities and obligations. Indeed, many models of household decision-making commonly assume altruistic preferences among household members (for an overview, see Browning, Chiappori, and Weiss, 2014). Behavioural economists have developed and tested models where people hold "pro-social preferences" (Fischbacher, Gächter, and Fehr, 2001): people are often other- rather than self-regarding. They value and adhere to norms of trust, reciprocity and fairness (Rabin, 1993; Fehr, Kirchsteiger, and Riedl, 1993; Bohnet et al., 2008). Models of identity can equally allow commitments, rather than assuming that any other-regarding preferences are self-interested.

However, Sen (1977; 1985; 2007) additionally argues commitments cannot be integrated into standard accounts of rational choice, and therefore that economists should develop non-preference-based theories of choice to allow for motivations other than self-interest. He criticises the "common tendency to make 'preference' (or a general-purpose 'utility function') an all-embracing depository of a person's feelings, values, priorities, choices, and a great many other essentially diverse objects" (Sen, 1991, 589).

I rather suggest that preferences to comply with social roles, which may be be completely altruistic, in the sense of commitments, can be accommodated by notions of preference in standard models. Although the evidence requires a recognition that human motivation is complex, "it does not follow that it [economics] needs multiple notions of preference" (Hausman, 2012, 64). Indeed, no part of positive economics states or implies that individuals are always self-interested (Hausman, 2012, 2014), although this is commonly assumed.[5] The

[5]This is not the case with normative, or welfare, economics, which "becomes arbitrary and unmotivated unless it attributes largely self-interested preferences to people" (Hausman, 2014, 18), although the definition of self-interest can be quite broad.

sort of altruistic preferences expressed by the students, including for complying with social norms, can be accommodated by slightly expanding standard assumptions about preferences.

The route between the two paths that I distinguished in Chapter 3 is thus that identity can be incorporated into standard models with some necessary but modest modifications, and considerable benefit to the explanatory power of models of investment in education. Children's identities – manifested in thoughtful transactions among multiple roles, with their strongly experienced obligations – are a strong influence on their daily choices of activity and decisions about whether to invest in education.

Moreover, accounting for identity in this way explains two patterns in behaviour not explained by standard models. One is that children and parents invest in education in a context where it is likely to have low returns. The Ethiopian children strongly value being identified as students and hope to become educated people, a high-status identity associated with stable wages, contributing to society, and other positive characteristics like discipline and a modern outlook. The other pattern not explained by the standard model is that, given that they have invested in education, the children spend time in household chores and in work for pay not only to benefit themselves, but to contribute to their households. Children value being independent earners and also value being good, obedient children who contribute to their households. Systematic incorporation of the importance of complying with social roles into economic models is thus, to paraphrase slightly, "appropriate in the interests of accuracy and an enhanced understanding of human behaviour"(Levison, 2000, 127).

E

Lists of interviews

Table E.1: Dates of qualitative interviews with children and parents in 2008 and 2010

	2008	2010
Geloye (girl)	22 August, 1 September	19 May
Geloye's mother and father	24 August, 1 September	
Kurabachew (boy)	23 August, 1 September	10 May
Dasse (boy)	28 August, 1 September	19 May
Hassen (boy)	28 and 30 August	12 May
Hassen's mother	1 September	
Senayit	22 August	12 May
Senayit's mother and father	1 September	
Tufa (boy)	23 August	19 May
Tufa's mother	23 August	
Kuftu (girl)	25 and 31 August	12 May
Kuftu's mother	31 August	
Senayit (girl)	26 August	17 May
Tigab (boy)	28 August	17 May
Shonah (girl)	28 August	19 May
Shonah's stepmother	31 August	
Tigist (girl)	30 August	
Ramato (boy)	30 August	
Quubsaa (boy)	26 August	12 May
Quubsaa's mother	31 August	
Tatek (boy)	29 August	10 May
Tatek's mother	31 August	
Genet (girl)	29 August	11 May
Genet's mother	29 August	
Galato (boy)	29 August	
Galato's mother	29 August	

Table E.2: Dates of qualitative interviews with community members in 2008

	2008	Gender
Bedene, *idir* leader	30 August	Man
Damalech, agricultural extension worker	28 August	Woman
Ayu, health centre worker	22 August	Woman
Haizu, Kebele leader	23, 25 and 28 August	Man
Kebede, farm foreman	24 August	Man
Kibron, farm manager	24 August	Man
Halkeko, Food for the Hungry manager	2 September	Man
Dawit	31 August	Woman
Felekech	1 September	Woman
Irrigation manager	2 September	Man
Farm caretaker	2 September	Man

Table E.3: Focus groups in 2008

Group	Activity	Date in 2008
Girls Group One	Drawing children at school activity	12 August
Girls Group One	Types of work activity	13 August
Girls Group Two	Drawing children at school activity	14 August
Girls Group Two	Types of work activity	15 August
Boys Group One	Drawing children at school activity	16 August
Boys Group One	Types of work activity	22 August
Boys Group Two	Drawing children at school activity	25 August
Boys Group Two	Types of work activity	24 August

7

Lengthening the school day and children's achievement

I. Introduction

Many developing country schools teach using a shift system, where schools teach one group of children in the morning and another group in the afternoon. Each group has a short school day of three to four hours, and the school can teach roughly double the number of children with the same number of teachers and spending on land and buildings. Many governments implemented shift schooling when facing shortages of classrooms or teachers, often as they rapidly expanded enrolment to make primary education universally available. Now, many African and Latin American countries are exploring lengthening the school day as they have reached near-universal enrolment and are focusing on improving the quality of education (Bray, 2008, 55).

Many economists of education, as well as the World Bank and UNESCO, argue that lengthening the school day is an inefficient use of limited resources (Bray, 2008). Lengthening the school day has been relatively costly when it has been applied in other developing countries: moving from shift to full-day schooling can increase the costs of primary education by between 25 and 60 per cent (as shown in later calculations). Evidence from developed and middle-income countries finds that increases in instructional time have only very small effects on achievement, of between 0.009 and 0.12 standard deviations of the distribution of student test scores.

However, the evidence from developed countries is unlikely to be a useful guide. Most work examines only small variations in instructional time of about 10 per cent of the school

day, drawing on exogenous variation in the number of days before standardised tests (Sims, 2008; Fitzpatrick, Grissmer, and Hastedt, 2011; Aguero and Beleche, 2013), variation in school days because of unanticipated weather closures (Marcotte, 2007), or variation in instructional time across different subjects (Wößmann, 2003; Lavy, 2010). If low-income countries move from shift to full day schooling, calculations (discussed later in this chapter) show that this will be an increase of between 20 and 40 per cent of instructional time per day.

I provide the first evidence from a low-income country on the effect of a government reform to abolish the shift system and appreciably lengthen the school day. Teaching in two shifts was widespread in Ethiopia because of limitations of resources, so the school day in primary schools was only four hours long. This length of school day was applied across all schools, even those that were not constrained by of a shortage of resources to use the shift system. In 2006 the federal government issued a general guideline to regional governments to lengthen the school day to five and a quarter hours, an increase in instructional time of roughly 30 per cent, and to abolish teaching in shifts where it occurred.

The research design has four key advantages. First, regions issued directives to district education authorities to implement the reform at different times. According to interviews with government officials, there was no systematic targeting of particular types of districts for the reform. Within a district, schools received an instruction to implement the reform and largely obeyed these instructions. This reduces the likelihood of selection bias from more motivated principals or communities selecting into applying the reform. Schools which applied the full-day reform did not experience changes in the level of per-student resources or change the curriculum they taught. Schools were simply allocated extra resources to build classrooms or hire new teachers to keep the level of per-student resources roughly the same, according to government standards for desired pupil-teacher and pupil-classroom ratios.

Second, the dataset used enables use of a difference-in-differences estimation strategy. The Young Lives study follows two cohorts of children born eight years apart who live in the same communities and attend the same schools. The older cohort were surveyed before the reform in 2002; the younger cohort were surveyed in 2009 after some districts have received the directive to implement the reform. Difference-in-differences does not require that treatment is randomly assigned either to districts or to schools. It only requires the assumption of equal trends: in the absence of the full-day reform, the average unobserved characteristics of "short-day" and "reform" schools, and of children in these schools, would change over time in the

same way. Children in short-day and reform schools are similar on observable characteristics at baseline; the schools are similar on a wide range of observable school quality characteristics at endline; achievement scores are similar in short-day and reform schools before the reform, once child characteristics are controlled for; and there are no observable school characteristics which predict whether the school implemented the reform. The implementation of the reform also strengthens the case for this assumption: if implementation of the reform is decided at district level without choice by schools, it is likely that short-day and reform schools and the children in them are similar at baseline on unobservable characteristics, which makes it likelier that they will change over time in similar ways.

Third, in the Ethiopian context, it is very difficult for government teachers to transfer between schools in Ethiopia, and long distances to school make it very difficult for children to move school. It is thus difficult for students or teachers to select into certain types of schools in response to the reform. This is an advantage for estimation: if more able children or teachers could select into schools that had applied the reform, the average unobserved characteristics of short-day and reform schools and the children in these schools might change in different ways.

Using a difference-in-difference specification to control for trends in achievement and child and school controls, I find that lengthening the school day has large positive effects on basic measures of achievement. The only Young Live data on achievement is very basic. The numeracy item required children to solve a basic multiplication problem (2 x 4). The reading item required children to read three letters (T, A, H), one word ('hat'), and one sentence ('the sun is hot'). In the writing item, a fieldworker asked the children to write the sentence 'I like dogs'. Children were coded into three categories: not being able to write, writing with difficulty or errors, or writing without errors.

In 2009, children in "reform" schools which implemented the full-day reform were 1.96 times more likely to be numerate than children in "short-day" schools and 3.17 times more likely to be able to write with difficulty or write easily, versus not being able to write, in specifications controlling for child- and school-level variables. There are no statistically significant differences in reading ability. These effects are robust in sign and significance to inclusion of school fixed effects to control for school- or district-level unobservables. Children in reform schools are 1.71 times more likely to be numerate than children than children in

those same schools in 2002, before the reform was applied. They are also 3.96 times more likely to be able to write with difficulty or write easily.

The dataset is one of few in Africa with linked information on individual children's achievement, their households, and the quality of school they attend, so I can explore heterogeneity of treatment effects by child characteristics and household socio-economic status. Most datasets have only school-level administrative data. The limited research that does compare children of wealthier and poorer families in the same school finds increased instructional time has much larger effects for children from poorer families (Lavy, 2010, 2012). In my research, by contrast, effects are larger among better-off children: children who are not stunted, children from richer households and children in urban schools. The reform thus improves achievement on average, but may exacerbate gaps between wealthier and poorer children.

II. Literature review

The major econometric difficulty in identifying the casual effect of instructional time on achievement is that instructional time may be assigned to students non-randomly. If schools control the amount of instructional time, there are probably omitted school-level variables correlated with their choice. The covariance between instructional time and unobserved school-level variables is non-zero, rendering inferences from regression models to the population invalid. Linked to this are two selection problems. Firstly, parents choose better schools if they can. If more instructional time is thought to signal a better quality school, there may be a non-zero correlation between omitted variables (such as parental motivation or child ability) and the amount of instructional time a child receives. Secondly, certain types of teachers may sort into schools based on the amount of instructional time.

Time in school could be varied randomly in a field experiment, but the systematic reviews cited in Chapter 2 suggest this has not been done. Studies have adopted four strategies to deal with omitted variable bias and selection issues, each of which has benefits and costs, but which yield broadly similar results, summarised in Table 7.1. Some authors compare gains in achievement over time for children receiving different amounts of instructional time. White and Hispanic children in the US who attended full-day kindergarten learn more in first grade than those who attended half day kindergarten (DeCicca, 2007). High school students whose schools had longer terms saw greater achievement score gains between tenth and twelfth

grade (Eide and Showalter, 1998). However, these results could reflect selection bias (if better students both select into schools with longer days and improve more quickly) or bias from omitted school-level variables (if the schools which choose to teach a longer day are also better in unobservable ways).

Secondly, research examines whether the same child does better in subjects in which they receive more teaching time, which accounts for both child-level unobservables and time-invariant school-level unobservables if children stay in the same school (Wößmann, 2003; Lavy, 2010). Students are less likely to select into schools on the basis of subject-specific instructional time, although teachers may select into schools which prioritise their subject. However, this strategy assumes the effect of instructional time is the same across subjects and cannot account for instructional time spillovers (e.g., more time in English may affect maths scores). Thirdly, analysis exploiting cross-country variation in instructional time finds a small positive association between the length of the school year and achievement (Lee and Barro, 2001). However, it has only one year of data on term length and cannot control for country fixed effects to account for unobservables at country level correlated with both term length and achievement.

Finally, studies exploit exogenous variation in instructional time, such as variation in the number of days before standardised tests (Sims, 2008; Fitzpatrick, Grissmer, and Hastedt, 2011; Aguero and Beleche, 2013), variation in school days because of unanticipated weather closures (Marcotte, 2007), or school management or funding reforms which lengthen the day as part of a package of reforms (Bellei, 2009; Cerdan-Infantes and Vermeersch, 2007; Lavy, 2012). To some extent these address concerns about selection of better schools into reforms, or selection of children or teachers into treatment schools. If "treatment" and "control" schools are similar at baseline on observable characteristics and children and teachers do not appear to change schools in response to the reform, one can argue that selection into treatment schools is of limited concern. However, these studies have often faced the difficulty that changes in instructional time are often implemented as part of a broad package of school management, funding or curriculum reforms, making it difficult to identify effects due only to variation in instructional time.

The examination of the reform in my study is more informative than existing evidence for low-income countries that may be considering lengthening the school day. Table 7.2 shows the length of school day in some low-income countries, where moving from shift to full-day

Table 7.1: The effect, in various studies, of increasing time in instruction by 10 per cent

Authors, Place	Unit of increase	Age/grade	Pooled	Reading	Language	Maths
Cross country						
Lavy (2010), 23 developing	10 per cent, hours/week	Age 15, secondary	0.024 SD increase			
Wößmann (2003), 39 developed	10 per cent, minutes/year	Age 13, primary	0.015 SD increase (maths and science)			
Lee and Barro (2001), 58 developed	10 per cent, days/year	Age 10 and 14, primary		1.8 p.p. decrease		2.6 p.p. increase
Within student						
DeCicca (2007), US	10 per cent, hours/day	Grade 1 White		0.028 SD increase		0.037 SD increase
		Grade 1 Black		0.011 SD increase		0.037 SD decrease
		Grade 1 Hispanic		0.039 SD increase		0.021 SD increase
Eide and Showalter (1998), US	10 per cent, days/year	Grades 10, 12				25th quantile: no effect 50th: 0.1 SD increase 75th: 0.09 SD increase
Natural experiment						
Fitzpatrick, Grissmer, and Hastedt (2011), US	10 per cent, days/year	Last year of kindergarten		0.12 SD increase		0.09 SD increase
Sims (2008), Wisconsin, US	10 per cent, days/year	Grade 3		No effect	No effect	No effect
		Grade 4		No effect	No effect	0.05 SD increase
Marcotte (2007), Maryland, US	Increase of one SD in snowfall for winter	Grade 3		0.78 per cent fewer got satisfactory	0.61 per cent fewer got satisfactory	1.2 per cent fewer got satisfactory
		Grade 5		No effect	0.56 per cent fewer got satisfactory	0.93 per cent fewer got satisfactory
		Grade 8		No effect	No effect	0.94 per cent fewer got satisfactory
Lavy (2012), Israel	10 per cent, hours of subject/week	Grade 5	0.026 SD increase (English, maths and science)			
Cerdan-Infantes and Vermeersch (2007), Uruguay	10 per cent, hours/day	Grade 6			0.009 SD increase	0.013 SD increase
Aguero and Beleche (2013), Mexico	10 days/year	Grade 4				0.04 SD increase
		Grade 5				0.07 SD increase
Bellei (2009), Chile	10 per cent, hours/day	Grade 10			0.023 SD increase	0.032 SD increase

Table 7.2: Average hours of instruction per year in primary schools in African countries

Country	Hours of instruction in a year		% increase to move from shift to full-day
	Shift schools	Full-day schools	
Burkina Faso	603	858	42.29
Mali	645	888	37.67
Ethiopia	840	1,100	30.95
Ivory Coast	580	754	30.00
Guinea	585	747	27.69
Senegal	547	675	23.40
Ghana	772	903	16.97
Gambia	936	1,024	9.40
Zambia	804	804	0.00

Source: Abadzi (2009), World Bank (2005).

schooling would increase time in school by between 20 and 40 per cent. As noted, the change in time in Ethiopia was around 30 per cent, and schools which lengthened the school day did not experience changes in the level of per-student resources, or change in the curriculum that they taught. Many were simply moving from one four-hour shift to one five and a quarter hour shift. If they were moving from two shifts of a short day to a full day, they received additional resources to build new classrooms and hire teachers, as detailed in Section IV.

In developed or middle-income countries, six out of seven studies based on evaluations of reforms find positive effects on achievement of between 0.009 and 0.12 standard deviations of test scores, mostly for small changes in instructional time of around 10 per cent in schools with days of six or more hours, as shown in Table 7.1. Studies find heterogeneous treatment effects by age (Marcotte, 2007; Lavy, 2010). Brown and Saks (1987) find total elasticities of achievement with respect to total time allocated to teaching were 0.24 and 0.13 for maths and reading scores respectively for Grade 2 children, but were zero and 0.07 for Grade 5 children. They argue that learning more advanced material may require students to learn mainly outside class: for example, second grade reading requires decoding skills, which are learned well in intensive reading classes, but by fifth grade, comprehension and inference are required, which probably require self-study as well as more time in instruction. Similarly, a summer school for low-achieving Chicago children had effects half as large for Grade 6 as Grade 3 children (Jacob and Lefgren, 2004) (Grade 3 children had reading and maths scores that were higher by 12 per cent of the average annual learning gain after two years).

All the studies mentioned that examine subjects separately find instructional time has positive effects on maths, but much smaller or no effects on reading or language (Sims, 2008; Brown and Saks, 1987; Lavy, 2012; Bellei, 2009; DeCicca, 2007; Marcotte, 2007), as I find in

Table 7.3: Cost of implementing full-day schooling in selected countries

Country	Type of reform	Cost implications
Chile	Public schools required to teach a full day	Operational cost of public education increased 25 per cent
Jamaica	Move from single to double shift schooling	Saved 32 per cent of cost of facilities
Uruguay	New schools set up with an eight hour day	Schools cost 60 per cent more than normal schools
Zambia	Shortening of school day for rural Grade 5-7	Reduced costs of achieving UPE by 46 per cent
Vietnam	Estimate of cost to move from half to full day	Recurrent spending would increase by 40 per cent

Source: Bellei (2003), Cerdan-Infantes and Vermeersch (2007), Bray, (2008), Carr-Hill (2010)

this chapter. Fitzpatrick, Grissmer, and Hastedt (2011) is the only study to find larger effects for reading than for maths. Marcotte (2007) hypothesises that in maths students receive least guidance at home and may require more review, and that maths curricula tend to be more rigid and content-focussed than reading or language. In cross-country work, Lee and Barro (2001) find increased instructional time improves maths scores but actually decreases reading scores. They argue that children read in their spare time but do not practice maths and science, for which instruction and practice in classrooms is important.

Evaluations of instructional time reforms are policy relevant. Table 7.3 show that moving from shift to full-day schooling can increase the costs of primary education by between 25 and 60 per cent, particularly if all schools in a system are teaching two shifts. More classrooms and facilities must be built. If there are double shifts and teacher shortages and teachers teach both shifts, more must be trained and hired. If teachers teach only one short school day shift before the school day is lengthened, salaries will probably increase to reflect more hours of work.

III. Data

My investigation uses household and school data from Young Lives, a 15-year longitudinal study in four countries. The household survey follows 3,000 children, 1,000 in the older cohort and 2,000 in the younger cohort. The household dataset has three waves, one in 2002 before the full-day reform and two after the reform, in 2006 and 2009. I use the household surveys for 2002 and 2009 for children's achievement scores, controls at child and household level, and the name of the school attended by each child in each round.

Table 7.4: Young Lives survey waves used in this chapter

Wave	Year	Younger cohort (born 2000)	Older cohort (born 1994)
Round 1	October–December 2002	6–18 months	*7–8 years*
Round 3	October 2009–March 2010	*7–8 years*	14–15 years
School survey 1	March–May 2010	*7–8 years*	14–15 years

The sample is not nationally representative. In Ethiopia, the survey was conducted in Addis Ababa and the four most populated regions, Oromia, Amhara, the Southern Nations and Nationalities and People's Region (SNNP) and Tigray. The survey excluded six of eleven administrative regions – the city states of Harar and Dire Dawa, plus five pastoralist or sparsely populated regions. The regions covered include 96 per cent of the population of Ethiopia (Woldehanna, Alemu, and Tekie, 2008). Young Lives used a "sentinel site surveillance methodology" to purposively select three to five districts in each region to ensure sites had a balance of ethnic groups, rural-urban status and whether districts were food-insecure. Conclusions from this data cannot be generalised even to the five regions Young Lives studies because of the purposive site selection.

One village, the lowest level of local authority, was randomly selected in each district as a survey site. Within each site, households were randomly sampled (with replacement) until 100 households were identified with one child aged 6 to 18 months and 50 households with one child aged eight (Barnett et al., 2013). If there was more than one eligible child in the household, one was randomly selected (Wilson, Huttly, and Fenn, 2006, 362). The sample is representative within a site. The main use of the sample is to examine changes over time, as in this study.

Attrition rates due to refusal to participate in the survey or difficulty in tracing households have been low compared to other longitudinal studies in developing countries (Outes-Leon and Dercon, 2008): 2.1 per cent of children were lost from Round 1 to 3 in the older cohort and 5.75 per cent in the younger cohort.[1]

The data on whether schools had implemented the full-day reform is from a survey of schools of Young Lives children. In the 2009/10 school year, the same school year as the third round of the household survey, Young Lives surveyed the schools attended by both older and younger cohort children in 2009. I was one of three researchers involved in designing and managing the survey. The principal of each school was asked detailed questions about the

[1]There are my own calculations from data in Woldehanna et al. (2011). These rates are higher than those given in Boyden and James (2014), which I cannot replicate. Both rates include children or households who could not be traced or refused to participate.

Table 7.5: Children and schools in difference-in-differences sample

	(1)	(2)	(3)	(4)
	School survey sample		No missing values	
	Children	Schools	Children	Schools
Short-day				
2002 cohort	566	45	455	42
2009 cohort	1,007	78	1,002	78
Total	1,573	84	1,457	84
Full day				
2002 cohort	84	18	77	17
2009 cohort	254	30	254	30
Total	338	30	331	30
Total				
2002 cohort	650	63	532	59
2009 cohort	1,261	108	1,256	108
Total	1,911	114	1,788	114

Column 1 shows all the children attending schools that were covered by the school survey in 2010. Column 2 shows the number of schools they attend. Columns 3 and 4 show the children who do not have any missing values on variables used in the regressions in this chapter and the schools they attend.

length of the school day and implementation of the full-day school reform in each year in each grade between 2002/03 and 2009/10, which I use to classify schools as either "short-day" school which had never applied the reform or "reform" schools which applied the reform after 2002. Short-day schools either taught in two shifts or one shift.

The sample also covers all the schools attended by older cohort children in 2002. The earlier rounds of the household survey captured the name of the school children attended in each round of the survey. I matched the name of the school attended by each child to retrospective information collected in the school survey.

Table 7.5 shows the number of children in each cohort in the sample. This chapter draws upon the 1,000 older-cohort children in Round 1 in 2002/3, when they were eight years old and just starting school, and the 1,886 children in the younger cohort surveyed in Round 3 in 2009/10, when they were also eight years old.[2] Column 1 shows all the children attending schools surveyed in the school survey. Column 2 shows the number of schools attended by each cohort.

[2]Nine percent of the children who were enrolled at the time of the Round 3 survey between October and March had dropped out of school by the time of the school survey in May. I allocate them to the school they were attending at the time of the Round 3 survey, when their achievement scores were measured. Sixty-three children had also moved school between the Round 3 survey and the school survey. I assign the few students who later switched schools to their initial schools. Children's achievement scores were measured when they were in the schools they attended at the time of the Round 3 survey; that they subsequently move schools is not relevant.

At the time of Round 1 in 2002/3, 652 of the 1000 older-cohort children were enrolled in school. Nearly all of those enrolled, 650, were attending schools visited in the school survey and are included in the sample. They attended 63 of the 114 schools in the sample.

At the time of Round 3 in 2009/10, 1,424 of the 1,886 children in the younger cohort were enrolled. Some 1,261 of those enrolled were attending schools included in the school survey data.[3] The younger cohort children attended 108 schools. Of the 114 schools (63 in 2002 and 108 in 2009), 59 were attended by at least one child from each cohort. Columns 3 and 4 describe the sample of children examined in this chapter: the children who do not have any missing values on variables used in the regressions in this chapter.

The dataset is thus a repeated cross-section of children, with information on two cohorts of eight-year-old children, one attending school in 2002 and one attending in 2009. It is, however, a unbalanced panel of schools as it examines many of the same schools over time. There is very limited data available on the quality of children's schools in 2002, as a school survey was not conducted.

There were only 11 children attending four "always full-day" schools, and they were not included in the sample or in Table 7.5 . These schools were all in Addis Ababa and had taught a full day since the early 1990s, before the government began to argue for reduction in use of the shift system. The reform to lengthen the school day does not generate exogenous variation in whether schools fall into this category. It is likely that including a dummy for whether children are in a full-day school in the regression would be correlated with unobservables and bias all coefficients.

The survey only covers government schools. There is a small private school sector in urban areas, but almost none in rural areas. There is no public government data on enrolment in private schools, but World Bank analysis found that non-government schools, many of which are NGO-administered faith-based schools, accounted for only 5.3 per cent of primary enrolment in 2006/07 (World Bank, 2008, 145). In the Young Lives sample, only four per cent of enrolled 15-year-olds in 2009/10 were enrolled in private schools.

[3]Some children had migrated since Round 1 and were attending schools outside the districts surveyed. These schools were not included in the school survey.

IV. The full-day schooling reform

I described the two rapid expansions in Ethiopian primary education in Chapter 2. When the *Derg* regime rapidly expanded education in the 1980s, shift schools became the norm and the school day for full time students was only three hours long (Negash, 2006, 19). Enrolment dropped during the civil war but then grew rapidly in the 1990s after the rebel EPRDF and its allies came to power. The EPRDF government abolished fees and instituted a massive school-building campaign. The government explicitly allowed that there would be a trade-off between increasing access to education quickly and providing high quality schooling. Roughly-built schools were set up quickly in isolated, poorer areas, often drawing on communities to provide labour and local building materials.

In the first Education Sector Development Plan (ESDP-I) in 1997, government stated that average class size would be allowed to rise from 33 to 50 and the shift system should be applied to use the limited numbers of teachers and classrooms efficiently (Ministry of Education, 1998, 6). ESDP-II in 2002 maintained the policy of promoting teaching in shifts (Ministry of Education, 2002, 24):

> In a shift school, the day consists of six 40 minute periods and one 15 minute break in the shift. For the average Ethiopian primary shift school, the morning shift starts at 8 a.m. and ends at 12:15 p.m. The afternoon shift starts at 12:30 p.m. and ends at 4:45 p.m. If there is an evening shift, it operates from 6 p.m. to 8 p.m. Shift schools provide only 4 hours of instruction per day, in contrast with the 5.25 hours per day in full-day schools. The reduced hours are necessary so that, particularly for primary school children, they are able to attend during daylight hours, including getting to and from school (Ministry of Education, 2009, 253).

In 2010, I interviewed the two directors of planning who had worked in the Federal Ministry of Education from 1997 to 2007, as well as the directors of planning and curriculum at the regional education bureaux in each region covered in the dataset being used. They clarified that, in 2002, "one- shift schools" taught a four hour day to one-shift of students, while "two-shift schools" taught two shifts of students. In 2002, 59 per cent of schools in the five regions in this study were one-shift schools (Table 7.6). However, both one- and two-shift schools were "short-day" schools, so that children only received four hours of instructional time.

Table 7.6: Number of one and double shift schools in Ethiopia in 2002 and 2009

Region	2002/03				2009/10			
	One shift	Double shift	Total	Double shift (%)	One shift	Double shift	Total	Double shift (%)
Tigray	422	514	936	0.55	1295	611	1,906	0.32
Amhara	1,432	757	2,189	0.35	3714	2519	6,233	0.40
Oromia	2,834	1,850	4,684	0.40	7306	2777	10,083	0.28
SNNP	1,279	1,103	2,382	0.46	3362	1198	4,560	0.26
Addis	201	76	277	0.27	6	682	688	0.99
Five regions	6,168	4,300	10,468	0.411	15,683	7,787	23,470	0.33
Total	6,694	5,202	11,896	0.44	18,059	7,153	25,212	0.28

Source: Ministry of Education (2003, 88; 2010, 38).

A. The full-day schooling reform across Ethiopia

ESDP-III, passed in August 2005, included plans for lengthening the school day and reducing the use of double shifts, based on the argument that this would improve the quality of instruction:

> The longer pupils remain in school, the more they obtain academic support from teachers, use libraries and laboratories and engage in co-curricular activities. However, contact periods in a double shift system are short. It is therefore necessary to increase time-on-task by reducing the operation of the shift system (Ministry of Education, 2005, 59).

However, there was widespread post-election violence in June and November 2005, several opposition leaders were imprisoned, and civilians demonstrating in Addis Ababa were killed (Abbink, 2006). Donors froze Direct Budget Support, resulting in a massive budget shortfall (Dom, 2010, 78). Thus, the full-day reform was not implemented at the start of the 2005/06 school year in September 2005. By May 2006, donor relations had stabilised, funding was reinstated, and implementation of ESDP-III began (Dom, 2006). Officials from all regions agree that directives were sent from the federal education ministry to regions advising them to convert schools to teaching a full day in summer of 2006. Implementation of the full-day reform began in the school year beginning in September 2006. They also note that the reform was given momentum because the opposition parties made education quality a major issue in the 2005 election. The full-day reform was one highly public way of addressing concerns about education quality.

Both schools teaching one 4-hour shift and schools teaching two 4-hour shifts converted to a 5.25- hour day. Changing from short-day to full-day schooling would thus be a 31.25 per

cent increase in instructional time. Ministry documents from 2009 define the length of day in short-day and full-day schools.

> A full-day school operates one shift during the entire day, in contrast with a shift school. At primary level, the school day consists of 7 periods, each of 45 minutes, with one 15- minute break in the morning, and a 1.5-hour lunch break. The school day starts at 8:30 a.m. and ends at 3:30 p.m., with lunch taken between 12:30 p.m. and 2 p.m. A full-day school provides 5.25 hours of instruction per day, in contrast with the 4 hours per day in a shift school (Ministry of Education, 2009, 254).[4]

Unfortunately, the Ministry data captures only whether schools were teaching in one or two shifts, not the length of the day. Between 2002 and 2010, the proportion nationally of schools teaching double shifts declined dramatically, as these schools shifted to teaching one full-day shift. The proportion of schools teaching two shifts dropped from 41 per cent in 2002/03 at the time of the collection of the first round of data used in this chapter, to 28 per cent in 2009/10 at the time of the second round of data (Ministry of Education, 2003, 2011b). But this is not a good indication of the application of the reform nationally, because a number of schools teaching only one shift also lengthened the day.

Officials stated that the reform was supposed to be applied in all school districts and was not targeted at any particular type of school. However, regions phased in the reform in different districts, and districts applied the reform in different parts of the district at different points in time. Officials reported that there was no systematic reason that one district was selected over another. Schools could not decide whether or not to implement the reform: they received a directive from the district. Schools phased in the reform by lengthening the day first in higher grades and then in lower grades, or vice versa. As Dom (2010, 13) notes, "While in principle, the federal decentralised management system should accommodate Ethiopia's diversity, in reality the system of implementation directives is quite strong; this, combined with resource constraints, does not leave much space to decentralised authorities".

The officials whom I interviewed affirmed that many of the schools which lengthened the day did not need to be allocated new resources: they were only teaching one group of students with one group of teachers in one group of classrooms for four hours, and simply

[4] A World Bank report from 2005 also provides the same information on the length of the day (World Bank, 2005, 145).

lengthened the day. Double shift schools, however, did require new resources, and were allocated some extra resources to build classrooms or hire new teachers. ESDP-III specifically considers the resource implications of the full-day reform: "Construction of new schools or building additional classrooms in existing schools shall be undertaken in order to reduce the percentage of double shifts" (Ministry of Education, 2009, 254). The document notes that these conversions were included when calculating the number of new classrooms to be added to schools over the entire five year period (194,748, 95,142 for the first cycle and 99,606 for second cycle); the number of new teachers to be recruited (168,847 first-cycle and 125,913 second-cycle teachers), and the number of new textbooks to be distributed to maintain a pupil-textbook ratio of 1:1 (Ministry of Education, 2005, 40).

However, if anything, reform schools may have been disadvantaged by implementing the reform and having to stretch the same physical resources over more students. So the treatment effect may be attenuated by reform schools having lower levels of resources, but is unlikely to be upward-biased. Reform schools continued to receive the same per-student grant of funding for running expenses (Dom, 2010). New classrooms or toilet facilities were usually built and paid for by the community, while the district donated only more expensive building materials (Ministry of Education, 2005, 40), as was done for other school expansions in the period (Garcia and Rajkumar, 2008).

B. Changes in instructional time in the Young Lives sample

As described in above, the full-day reform was imposed at district level and was compulsory for schools within a district. However, because data was collected while the reform was being phased in, there is variation between and within districts in whether the reform has been applied. Table 7.7 shows the extent of the implementation of the reform by the 2009/10 school year in the 30 districts in the Young Lives survey.[5]

The extent of implementation of the reform was relatively uniform in most districts, which supports the evidence from qualitative interviews that schools could not decide whether to implement the reform. In 11 districts, shown in the first row, all schools still taught in shifts, either because schools had not received a directive to increase the length of the day or because

[5]In 2002, Young Lives sampled children in 20 survey sites, using the boundaries of the peasant association (*kebele*) in use at the time. Each *kebele* was part of only one district. However, district boundaries were altered in 2005, splitting some Young Lives districts into two. In addition, children from one *kebele* may attend schools in neighbouring districts. In this table and the rest of the chapter, each school is allocated to a district using the school's location and the boundaries of the district in 2005.

Table 7.7: Application of the full-day reform and teaching time in Young Lives districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Whether school reports implementing reform			Teaching time: Grade 1–4	
Implementation within the district	Districts	Schools	Shift school, directive not received	Shift school, directive received but not implemented	School with reform applied in Grades 1-4	School with reform applied above Grade 4	Less than 4 hours	More than 4 hours
All shift	11	27	14	13	0	0	27	0
Majority shift	5	42	11	15	11	5	33	9
Equal numbers	3	8	3	1	4	0	4	4
Majority reform	5	21	1	4	8	8	14	7
All reform	6	16	0	0	9	7	8	8
Total	30	114	29	33	32	20	86	28

Column 1 categorises the 30 Young Lives districts according to the proportion of schools that have implemented the full-day reform. Column 2 shows, for example, that there are 27 schools in the 11 districts where all schools were still teaching in shift. Columns 3-6 give the number of schools in each type of district that report implementing the full-day reform in Grades 1 to 4. Column 3 shows shift schools that did not receive a directive from the district. Column 4 shows shift schools that received a directive but had not yet implemented it. Column 5 shows schools which implemented the reform in Grades 1 to 4 between 2002 and 2009 (they could also have implemented the reform in other grades). Column 6 shows schools which implemented the reform, but only in grades above Grade 4. In Columns 7 and 8, schools are classified according to the total amount of teaching time children receive in Grade 1 to 4. 32 schools state that they implemented the full-day reform in Grades 1 to 4, but four of these schools did not increase the amount of teaching time to more than four hours.

they had received the directive but not yet implemented it. These districts had probably only recently sent directives to schools. In 8 districts, shown in the next two rows, schools had just begun implementing the reform, with either a majority still teaching in shifts, or equal numbers teaching in shifts and for a full day. The remaining 11 districts had largely implemented the reform: 6 districts have implemented the reform in all schools and 5 had implemented it in a majority of schools. The reform was implemented to some extent in districts all the five regions covered in the study, and in both rural and urban schools.

The school survey collected data about whether the school taught in shifts, the duration of the school day, the amount of time allocated to lessons in each grade, and the years when schools changed from shift to full-day. The data on teaching time was collected from examination of the school timetable for the relevant grades. The ESDP documents above state that short-day schools four hours of instruction in six lessons of 40 minutes, while full-day schools provided five-and-a-quarter hours of instruction in seven lessons of 45 minutes (World Bank, 2005, 145). However, the survey finds that schools which lengthened the day sometimes taught for slightly longer than 5.25 hours. In 2009 in Grades 1 to 4, shift schools in the Young Lives sample taught for an average of 3.93 hours a day, reform schools for 5.82 hours and schools which were always full-day for 5.62 hours. This may reflect measurement error in the survey measures of time in lessons, or that schools structured their timetables and number of lessons in slightly different ways.

In this chapter, a grade is classified as applying the reform if it changed its lesson time between 2002 and 2009 to provide class time of more than than four hours. I use the amount of time allocated to lessons rather than the length of the school day, because educational research emphasises that schools often allocate large amounts of the school day to activities in which no learning occurs (Patall, Cooper, and Allen, 2010; Abadzi, 2009). Table 7.7 shows that 29 schools who stated they applied the reform also lengthened teaching time to more than four hours. Four schools state that they implemented the reform in Grades 1 to 4, but did not increase the amount of teaching time to more than four hours. However, results are almost identical when I examine whether schools report implementing the reform rather than whether they actually increased teaching time.

V. Research design

I use a difference-in-differences design to compare outcomes among children attending reform schools before and after the reform was implemented, using the group of children attending short-day schools as a control group to remove any trends in achievement over time. This identifies the average treatment effect on the treated (ATT) of a policy which lengthens the school day. The ATT is valid only for the group of children in reform schools and does not necessarily predict the effect that lengthening the school day would have on children in short-day schools or on the sample as a whole. It is defined as follows:

$$\triangle^{ATT} \equiv \mathrm{E}\left[A(1)|\textbf{Reform}=1,\ \textbf{Post}=1,\ X\right] - \mathrm{E}\left[A(0)|\textbf{Reform}=1,\ \textbf{Post}=1,\ X\right] \quad (7.1)$$

where **Reform** $= 1$ for students in schools which applied the full-day reform. **Post** $= 1$ for students in the younger cohort surveyed in 2009, when the full-day reform had been implemented in some schools. **Post** $= 0$ for students in the older cohort surveyed in 2002, before the reform was applied (the 11 children in four schools which were already teaching a full day are not included in this sample). $A(1)$ is achievement among students who received the longer school day because they were attending reform schools when they had applied the reform. $A(0)$ is achievement for students who received a shorter school day, either because they were in schools which did not apply the reform or because they were in schools which applied the reform in the years after they were surveyed.

$\triangle^{ATT}$ compares the observed mean outcome for the treated group in the post-treatment period, students in reform schools in 2009, and the counterfactual mean outcome for the same group if they had not been treated, the scores of students in reform schools in 2009 if the reform had not occurred. The latter is unobserved. I thus estimate $\triangle^{ATT}$ with $\hat{\triangle}^{ATT}$.

$$\begin{aligned}\hat{\triangle}^{ATT} = {} & \mathrm{E}\left[A(1)|\textbf{Reform}=1,\ \textbf{Post}=1,\ X\right] - \mathrm{E}\left[A(0)|\textbf{Reform}=1,\ \textbf{Post}=0,\ X\right] \\ & -\mathrm{E}\left[A(0)|\textbf{Reform}=0,\ \textbf{Post}=1,\ X\right] + \mathrm{E}\left[A(0)|\textbf{Reform}=0,\ \textbf{Post}=0,\ X\right]\end{aligned} \quad (7.2)$$

For $\hat{\triangle}^{ATT}$ to identify $\triangle^{ATT}$, one must make the "equal time trends" assumption. This states that the unobserved counterfactual mean $\mathrm{E}\left[A(0)|\textbf{Reform}=1,\ \textbf{Post}=1,\ X\right]]$ in Equation 7.1 is equal to the time-adjusted mean outcome for children in reform schools in 2002:

$$\begin{aligned} \mathrm{E}\left[A(0)|\textbf{Reform}=1,\ \textbf{Post}=1,\ X\right]] &= \mathrm{E}\left[A(0)|\textbf{Reform}=1,\ \textbf{Post}=0,\ X\right]] \\ &\quad +\mathrm{E}\left[A(0)|\textbf{Reform}=0,\ \textbf{Post}=1,\ X\right]] \\ &\quad -\mathrm{E}\left[A(0)|\textbf{Reform}=0,\ \textbf{Post}=0,\ X\right]] \end{aligned} \tag{7.3}$$

This is equivalent to assuming that, in the absence of the reform, children's average unobserved characteristics would have changed over time in the same way for children in short-day and reform schools, and that unobserved characteristics at school level would also have changed over time in the same way for short-day and reform schools:

$$\begin{aligned} &\mathrm{E}\left[A(1)|\textbf{Reform}=1,\ \textbf{Post}=1,\ X\right]] - \mathrm{E}\left[A(0)|\textbf{Reform}=1,\ \textbf{Post}=0,\ X\right]] = \\ &\mathrm{E}\left[A(0)|\textbf{Reform}=0,\ \textbf{Post}=1,\ X\right]] - \mathrm{E}\left[A(0)|\textbf{Reform}=0,\ \textbf{Post}=0,\ X\right]] \end{aligned} \tag{7.4}$$

I estimate $\triangle^{ATT}$. A_{ist} is the achievement score of child i in school s at time t. $f(X_i)$ is a function of covariates of achievement at child level. $g(Y_s)$ is a function of covariates of achievement at school level. The error term ϵ contains a school-level error term μ_s and a child-level error term v_{ist}. β_1 captures trends in achievement between 2002 and 2009 common to children in both types of school. β_2 captures time-invariant differences between students in short-day and reform schools. The simplest specification does not include child- and school-level controls but is restricted to the sample of children for whom all control variables X_i and Y_s are available. It corresponds to Model I in the tables which follow:

$$A_i = \beta_0 + \beta_1 \text{Post}_t + \beta_2 \text{Reform}_s + \triangle \text{Post}_t * \text{Reform}_s + \epsilon \tag{7.5}$$

If $\text{Post}_t = 1$ and $\text{Reform}_s = 1$, the child attended a reform school in 2009, after the reform was applied in that school. $\triangle$ is the difference-in-differences estimate of the impact being in a reform school compared to being in a short-day school. If it is positive and significant, changing from short-day to full-day schooling has improved achievement for children in reform schools. $\triangle$ estimates $\triangle^{ATT}$ consistently if the equal time trends assumption holds. Students at the same school, both at the same time and in different time periods, are likely to

share unobservable characteristics, resulting in serial correlation in the error term (Moulton, 1986).[6] I use Liang and Zeger (1986) standard errors to account for clustering at the school level.[7]

Model II, which corresponds to Equation 7.5, includes child controls $f(X_i)$ and school-level controls $g(Y_s)$, as well as grade fixed effects ϕ_g:

$$A_i = \beta_0 + \beta_1 \text{Post}_t + \beta_2 \text{Reform}_s + \triangle \text{Post}_t * \text{Reform}_s + f(X_i) + g(Y_i) + \phi_g + \epsilon \quad (7.6)$$

The controls reduce bias from any differences in the type of children in short-day and reform schools in the pre- and post-reform cross-sections. Substantial changes in the student body in short-day or reform schools may still be of concern if they indicate problematic selection, an issue discussed further below. $f(X_i)$ and $g(Y_s)$ are assumed to be linear for simplicity. $\triangle$ estimates $\triangle^{ATT}$ consistently if the equal time trends assumption holds and $f(X_i)$ and $g(Y_s)$ are specified correctly. The grade fixed effects absorb variation due to children being in higher grades and effectively compare children to others of the same grade level. Results are robust to their exclusion.

Model III drops the Reform_s dummy, which captures time-invariant differences between students in short-day and reform schools, and school-level controls $g(Y_s)$. These are replaced with school-specific intercepts μ_s to control for time-invariant differences between schools not due to treatment and for the mean over time of any school-year shocks μ_{st}.[8] β_1 still captures variation in achievement between 2002 and 2009 across children in all schools. $\triangle$ captures any variation between younger cohort children across all short-day schools and younger cohort children across all reform schools. $\triangle$ estimates $\triangle^{ATT}$ consistently if the equal time trends assumption holds and $f(X_i)$ is specified correctly.

[6] In addition, while additive time effects are included in all models and additive school effects are included in Model III, *school* $*$ *year* shocks μ_{st} are not included. It is assumed that $E(\mu_{st}) = 0$, but some individual μ_{st} may not be equal to zero, and Bertrand, Duflo, and Mullainathan (2004) argue adjustments must also be made for serial correlation in the μ_{st} themselves.

[7] Estimation techniques that account for clustering use a pseudo-likelihood, rather than the true likelihood, to estimate coefficients, so likelihood ratio tests cannot be used (Chambers and Skinner, 2003, 22). I therefore use adjusted Wald tests, which approximates a likelihood ratio test asymptotically and if the model is correctly specified. I 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, because Liang and Zeger (1986) standard errors can be unreliable if there are fewer than about 100 clusters. When the t distribution is used, standard errors provide reasonably good corrections for serial correlation in panels with only ten clusters (Hansen, 2007).

[8] *school* $*$ *cohort* or *school* $*$ *year* shocks μ_{st} cannot be included: they would be perfectly collinear with the treatment effect Post $*$ Reform. But there is a large number of schools, so it is plausible that any *school* $*$ *year* shocks average out to zero ($E(\mu_{st}) = 0$).

$$A_i = \beta_0 + \beta_1 \text{Post}_t + \triangle \text{Post}_t * \text{Reform}_s + f(X_i) + \mu_s + \phi_g + \epsilon \quad (7.7)$$

In Round 1 in 2002, very limited achievement measures were collected with the older cohort. These were repeated with the younger cohort in Round 3 in 2009. As noted earlier, the numeracy item required children to solve a basic multiplication problem (2 x 4). The reading item required children to read three letters (T, A, H), one word ('hat'), and one sentence ('the sun is hot'). Children were coded into four categories: unable to read, able to read letters, able to read words, or able to read the sentence. In the writing item, a fieldworker asked the children to write the sentence 'I like dogs'. Children were coded into three categories: not being able to write, writing with difficulty or errors, or writing without errors.

The reading and writing variables are ordinal: categories can be ranked but the distances between adjacent categories are not known. I therefore present ordered logit models for the reading and writing variables and a logit model for the numeracy variable. To control for school fixed effects for the binary logit model, I use the Chamberlain (1980) conditional logit estimator. There is no similarly well-established estimator for fixed effects ordered logit models. Often, researchers collapse the ordinal variables into binary variables and estimate models with fixed effects using the conditional logit model. But here, in models without fixed effects, the threshold parameters are statistically different from each other, so the categories should not be collapsed. Instead, I use one of a range of new estimators: the so-called "blow up and cluster" estimator, proposed by Staub, Winkelmann, and Baetschmann (2015).[9] In all non-linear models, the coefficients displayed are odds ratios. A coefficient larger than one is equivalent to a positive coefficient in a linear regression, while a coefficient smaller than one is equivalent to a negative coefficient.[10]

Finally, I examine treatment effects for different types of children. In Equation 7.8, for example, I examine if there are differences in the effect of being in a reform school for boys and girls. If $\triangle_2$ is significantly larger than zero, this suggests that the treatment effect on the

[9]Instead of selecting one cut-off point to dichotomise the ordinal variable, they "blow up" the sample size by replacing every observation with $K-1$ copies of itself and dichotomise every copy of the individual at a different cut-off point. They then estimate the conditional maximum likelihood logit using the whole sample. Some groups contribute to several terms in the log likelihood (e.g. a school where one child scores four and one scores one will contribute to the score if the cut-off is two or three), so there is dependence between these terms which is accounted for in the standard errors by using a cluster-robust variance estimator. The sample sizes displayed include the replications of each individual.

[10]It would be preferable to present marginal effects, but it is not possible to compute the marginal effect of an interaction term in the conditional logistic model (Karaca-Mandic, Norton, and Dowd, 2011, 267).

treated of being in a reform school after the reform was implemented is larger for boys than girls. $\triangle_1$ gives the treatment effect for girls and $\triangle_1 + \triangle_2$ gives the treatment effect for boys.

$$A = \beta_0 + \beta_1 \text{Post}_t + \triangle_1 \text{Post}_t * \text{Reform}_s + \triangle_2 \text{Post}_t * \text{Reform}_s * \text{Male} + f(X_i) + g(Y_i) + \phi_g \quad (7.8)$$

VI. Threats to the equal time trends assumption

To obtain valid estimates from a differences-in-differences model, the equal trends assumption must be plausible. This assumption can be argued to be plausible but cannot be tested directly. In this section, I examine whether three violations of this assumption occur. This does not demonstrate that the equal trends assumption holds, but merely that three common violations of this assumption do not occur.

Firstly, if short-day and reform schools or the children in those schools are very different on observables at baseline, this suggests that children's unobserved characteristics, or the unobserved characteristics of schools, are unlikely to change in the same way over time. Secondly, if the reform induced migration of children or teachers into or out of reform or short-day schools, these changes in composition of the children in short-day and reform schools, rather than the reform, which cause differences in outcomes. Finally, there may be shocks or policy changes which are correlated with treatment. Importantly, the equal time trends assumption does not require that treatment is randomly assigned.

A. Correlated shocks or policy changes

If one of the schools experiences a policy change or shock at the same time as the policy change of interest, this may confound results. This does not seem to have occurred here. Firstly, the reform was not attached to any other policy reforms, according to interviews with federal officials and the ESDP-III documents (Ministry of Education, 2005). Federal officials report that the curriculum was not changed for reform schools and the schools were not assessed differently in the National Learning Assessment. In other studies, interventions to change instructional time were part of school management or funding reforms which affect other school-level variables, making it difficult to identify effects due only to variation in instructional time. In Chile, full-day schools got a one-time investment in school facilities and a permanent increase in the grant per student (Bellei, 2009). In Uruguay, the full-day

Table 7.8: Descriptive statistics at school level in 83 schools in 2009

	(1) Short-day	(2) Reform	(3) Total	(4) (2)-(1) p-value	(5) (2)-(1) q-value
Urban school	0.35	0.77	0.47	0.01 **	0.03 **
	(0.06)	(0.08)	(0.05)		
Fewer than 8 grades	0.51	0.48	0.50	0.83	0.96
	(0.06)	(0.09)	(0.05)		
Average classroom is brick	0.57	0.47	0.55	0.99	0.99
	(0.06)	(0.13)	(0.05)		
Students have pit latrines	0.72	0.60	0.70	0.08*	0.54
	(0.05)	(0.13)	(0.05)		
Students have flush toilets	0.08	0.27	0.12	0.06*	0.54
	(0.03)	(0.12)	(0.03)		
Has piped drinking water	0.53	0.60	0.54	0.55	0.85
	(0.06)	(0.13)	(0.05)		
Has electricity	0.44	0.47	0.44	0.57	0.85
	(0.06)	(0.13)	(0.05)		
Has library	0.60	0.53	0.59	0.35	0.78
	(0.06)	(0.13)	(0.05)		
Cluster resource centre	0.50	0.53	0.51	0.78	0.96
	(0.06)	(0.13)	(0.06)		
Enrolment in first shift/whole day	529.04	473.50	519.19	0.32	0.78
	(59.28)	(98.50)	(51.87)		
Pupil-teacher ratio	40.35	38.05	39.94	0.53	0.85
	(1.25)	(2.81)	(1.15)		
Pupil-classroom ratio	47.49	49.59	47.83	0.92	0.96
	(2.15)	(5.76)	(2.04)		
Pupil-latrine ratio	91.79	90.91	91.62	0.74	0.95
	(11.04)	(21.76)	(9.85)		
Average Grd 2 classroom has children on floor	0.34	0.40	0.35	0.91	0.96
	(0.06)	(0.13)	(0.05)		
Number of classrooms	11.82	9.63	11.37	0.20	0.78
	(1.11)	(1.39)	(0.93)		
Teachers with degrees (%)	0.02	0.02	0.03	0.29	0.78
	(0.01)	(0.01)	(0.01)		
Teachers with diplomas (%)	0.53	0.47	0.52	0.38	0.78
	(0.03)	(0.07)	(0.03)		
Teachers with Grade 10 (%)	0.12	0.06	0.11	0.22	0.78
	(0.03)	(0.03)	(0.02)		
Annual salary and benefits, Grd 1-4 full-time teacher	10,049.83 (324.03)	10,629.88 (1032.02)	10,151.34 (323.57)	0.41	0.79
Teacher job satisfaction out of 5	1.83	1.86	1.84	0.87	0.96
	(0.03)	(0.07)	(0.03)		
Annual salary and benefits, principal	17,354.77	14,777.07	16,888.92	0.07*	0.54
	(1,083.27)	(1,412.31)	(929.94)		
Principal motivation index out of 5	4.66	4.67	4.66	0.52	0.85
	(0.07)	(0.16)	(0.07)		
Mean days teacher absence last week	0.12	0.17	0.13	0.30	0.78
	(0.03)	(0.07)	(0.03)		
Mean days teacher absence in year	3.66	3.67	3.66	0.73	0.95
	(0.35)	(0.83)	(0.32)		
Possible teacher rewards and censures					
Bonus or promotion	0.22	0.33	0.24	0.36	0.78
	(0.05)	(0.12)	(0.05)		
Pay deduction	0.57	0.27	0.52	0.26	0.78
	(0.06)	(0.12)	(0.06)		
Demoted or transferred	0.22	0.27	0.23	0.73	0.95
	(0.05)	(0.12)	(0.05)		

Column 4 gives the p value of the difference between Column 2 and Column 1. If the p-value of the adjusted Wald test of the difference is significant, the difference is starred. *p<0.01 **p<0.05 ***p<0.01.

school program included funding for construction of new classrooms, a reduction in class size, teacher training in a new pedagogical model, provision of teaching materials and nutritional and health care support (Cerdan-Infantes and Vermeersch, 2007).

In Ethiopia, there were other national reforms in the period, but these were implemented at national level across all schools. A Teacher Development Programme was implemented to train teachers who did not have relevant qualifications in 2006 and schools began to receive block grants directly from districts in 2005 (Dom, 2010). However, none of these were targeted specifically at reform schools, so they do not affect the assumption of equal trends.

Secondly, reform schools do not have substantially higher or lower levels of per-student funding or physical resources. If implementing the reform resulted in other unobserved changes in reform schools, such as improvements in resources because of extra funding or, alternatively, an increase in overcrowding, the effect of the full-day reform may pick up the effect of these changes, not the effect of a change in instructional time. Table 7.8 gives descriptive statistics in 2009 at school level for 83 schools in the sample.[11] Once p-values are corrected for multiple testing, there are few significant differences between short-day and reform schools in basic resources, on measures of overcrowding or on other measures of school quality after the implementation of the reform.

There are two possible explanations for the similarity in post-reform resource levels. Reform schools may have had similar resource levels to short-day schools at baseline and been given enough resources to keep per-student resource levels the same as in short-day schools, as officials argue was the case. District officials have tight targets for ratios such as the pupil-teacher ratio and pupil-classroom ratio, which they are required to send to regions regularly, and they use these to reallocate resources between schools (Engel, 2010; Dom, 2010). Alternatively, these schools may have had more classrooms, teachers and resources than short-day schools before they implemented the reform. Given the scarcity of resources in Ethiopia in general, it is likely that schools received small amounts of extra resources to implement the reform and maintain the same level of per-student resources.

Finally, it might be expected that lengthening the day increases teachers' teaching loads, so that teachers in reform schools may have been more tired or demotivated than those in short-day schools. Federal officials noted that teachers were not paid more in schools which

[11]It is not possible to examine pre-treatment differences in school quality at baseline because there is only limited baseline data on school quality, although I show in the next section that there are no differences at baseline in the number of grades covered by reform and short-day schools. Schools which cover fewer than eight grades tend to have been more recently built and have fewer resources.

Table 7.9: Average weekly teaching loads in hours, Oromia Region, 2002/03

	Teacher qualification				
Locality & grade taught	10+1; 10+2	12; 12+1	1 year certificate	2 year diploma	All teachers
Oromia	17.1	21.0	24.2	18.1	23.8
Grades 1-4	18.0	27.4	26.0	22.2	26.0
Grades 5-8	17.5	16.2	21.2	18.0	20.6
Urban schools	17.5	16.6	19.9	17.0	17.6
Grades 1-4	18.1	21.6	21.6	18.1	21.5
Grades 5-8	17.1	15.8	17.9	17.0	17.6
Rural schools	23.0	27.7	27.3	22.3	27.2
Grades 1-4	15.0	28.9	28.6	28.9	28.6
Grades 5-8	27.0	20.3	24.5	21.9	24.3

Source: World Bank (2005, 176). 10+1 is Grade 10 plus a one year certificate. 10+2 is Grade 10 plus a two year diploma.

lengthened the day. However, the limited evidence suggests that before the reform, teachers often taught both shifts, so teaching loads in reform schools were actually lower than or relatively similar to those in short-day schools. There is no national data on teaching loads, but n 2002/3 a teacher census in Oromia Region (the country's largest region) shown in Table 7.9, found an average teaching load of 26.0 hours for Grade 1–4 teachers (39 lessons a week). Average loads were 21.5 hours (33 lessons) for Grade 1–4 teachers in urban areas and 28.6 hours (43 lessons) in rural areas (World Bank, 2005, 145).[12] Teachers in rural Oromia were thus on average teaching in more than one shift, as there are only 30 lessons in a week in short-day schools. In rural schools which applied the full-day reform, teachers' teaching loads would actually have decreased from 43 to 35 lessons. In urban schools loads would have increased marginally from 33 to 35 lessons. If other regions were similar to Oromia, moving from short-day to full-day would have caused little change in teaching loads for most teachers.

B. Incomparable treatment and control groups

It is more reasonable to assume equal changes over time for two similar groups than two very different groups. A common check is whether treatment and control groups are similar at baseline on observable variables and whether trends in observable variables are the same over time. Table 7.10 presents summary statistics for outcome variables for children in short-day and reform schools in 2002, before the reform was implemented.

[12] This calculation assumes lessons are 40 minutes long.

Column 7 describes differences between reform and short-day schools for the older cohort in 2002 at baseline before the reform is implemented. This is from a regression (accounting for clustering at school level) of the treatment dummy on the relevant variable. Column 8 is the same for the younger cohort in 2009. For ordinal variables – reading, writing and father's education – Column 7 and 8 show the p-value of a Pearson chi-square test for clustered data of whether there are significant differences in the distribution across categories among children in reform and short-day schools. These are enclosed in square brackets.

For statistics in Columns 7 and 8, I present naive p-values. In Columns 9 and 10, I present q-values, which correct the p-values in Columns 7 and 8 for the fact that I test multiple outcomes, which increases the probability of false discoveries. I use the Benjamini and Hochberg (1995) procedure. Once this correction is performed, Table 7.10 shows there are no differences in outcome variables between children in shift and reform schools at baseline at the five per cent level.

Table 7.11 presents the same statistics for control variables at child and school level which may affect achievement. The columns are as in Table 7.10. After multiple testing corrections, the only difference between children in shift and reform schools is that children in reform schools have a higher consumer durables index at baseline. The index gives one point for each of eleven common household assets that a household owns (television, radio, mobile phone, phone, table and chairs, sofa, bed, car, motorbike, bicycle, fan), and is then divided by eleven to give a score out of one.

In addition, Column 11 gives the p values of the second difference: the difference in changes in control and outcome variables between pre- and post-reform students in short-day and reform. This is $\triangle$ from a linear regression, accounting for clustering at school level, where Y_i is the achievement score.

$$Y_i = \beta_0 + \beta_1 \text{Post}_t + \beta_2 \text{Reform}_s + \triangle \text{Post}_t * \text{Reform}_s + \epsilon \tag{7.9}$$

Column 12 gives the q-values for these p-values after a multiple testing correction. I do not present the second difference for achievement scores, as these could have been affected by the programme. The difference-in-differences estimates in Columns 11 and 12 are not significantly different from zero in all observable covariates. Thus there are no significant differences in changes between older and younger cohort children in short-day schools and

Table 7.10: Outcome variables at child level for eight-year-olds in 2002 and 2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Older cohort			Younger cohort			Differences p val		Differences q val	
	Short-day	Reform	Total	Short-day	Reform	Total	(2) - (1)	(5) - (4)	(2) - (1)	(5) - (4)
Outcomes										
Can't read	0.37	0.22	0.35	0.45	0.33	0.43	[0.02]**	[0.10]	[0.08]*	[0.31]
	(0.05)	(0.08)	(0.05)	(0.04)	(0.09)	(0.04)				
Reads letters	0.30	0.17	0.28	0.20	0.14	0.19				
	(0.05)	(0.05)	(0.04)	(0.02)	(0.03)	(0.02)				
Reads words	0.08	0.08	0.08	0.08	0.08	0.08				
	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)				
Reads sentence	0.25	0.53	0.29	0.27	0.45	0.30				
	(0.05)	(0.09)	(0.05)	(0.04)	(0.09)	(0.04)				
Can't write	0.39	0.39	0.39	0.55	0.43	0.53	[0.53]	[0.06]*	[0.91]	[0.31]
	(0.06)	(0.08)	(0.05)	(0.06)	(0.09)	(0.05)				
Writes with difficulty	0.26	0.34	0.27	0.30	0.26	0.29				
	(0.03)	(0.05)	(0.03)	(0.04)	(0.04)	(0.03)				
Writes easily	0.35	0.27	0.34	0.15	0.32	0.19				
	(0.05)	(0.08)	(0.04)	(0.03)	(0.07)	(0.03)				
Multiplication correct	0.50	0.47	0.49	0.43	0.54	0.45	0.69	0.21	0.91	0.46
	(0.05)	(0.05)	(0.04)	(0.04)	(0.06)	(0.03)				
Children	532			1,256			532	1,256	1,788	
Schools	59			108			59	108	114	

*p<0.01 **p<0.05 ***p<0.01. T stats, adjusted Wald tests and cluster-robust standard errors are used, where the cluster is the school. Column 7 is the p-value for the difference between Columns 1 and 2. Column 8 is the p-value for the difference between Columns 4 and 5. For ordinal variables, Columns 7 and 8 show the p-value of a Pearson chi-square test for clustered data of whether there are significant differences in the distribution across categories among children in reform and short-day schools. These are enclosed in square brackets. If the p-value of the adjusted Wald test of the difference is significant, the difference is starred. Columns 9 and 10 give q-values, which correct the p-values in Columns 7 and 8 for multiple testing.

reform schools on these covariates. Overall, there are not substantial differences in outcome variables at baseline, nor in control variables which might affect achievement. There are also no differences in trends over time between short-day and reform schools on these variables. This is reassuring: if there are equal trends in observables, it suggests that unobservable trends in the groups are similar.[13]

Table 7.11 also describes the sample. The average grade for a child is 1.57 in 2002 and 1.79 in 2009. 44 per cent of children in the sample are in Grade 1, 38 per cent in Grade 2 and 17 per cent in Grade 3, reflecting late or early enrolment or delayed progression. However, there is automatic promotion between grades for as long as children attend, so grade attained is poorly correlated with achievement: 25 per cent of children in Grade 3 cannot read a letter or read at all, for example. The proportion of children who cannot read anything and cannot write increases over time, while scores on the numeracy item remain stable.

52 per cent of children have fathers with no education, 21 per cent have fathers with between Grade 1 and Grade 4 and 26 per cent have fathers who progressed beyond Grade 4. Children spend slightly over an hour on household chores on average a day. 22 per cent are stunted in 2002, which declines to 15 per cent by 2009, reflecting improvements in health services and some economic growth over time. Scores on the consumer durables index have increased over time, reflecting some improvements in living standards in the period, but households are still poor on average, from owning 1.5 out of 11 of the list of durables in 2002 to 2.82 out of 11 in 2009.

There is only limited data available on schools at baseline. I code schools as rural or urban using the school's GPS location and a GPS shape file giving the boundaries of all towns in Ethiopia from the 2007 census (Central Statistical Agency of Ethiopia, 2007). Schools in a town or city in 2007 are classified as urban. Young Lives sites are either rural or urban, but children from a rural Young Lives site may attend a school which is in a neighbouring urban area. A higher proportion of children in reform than short-day schools attend an urban school at baseline (at the 10 per cent level), but there are no major pre-treatment differences between children in short-day and reform schools in whether their schools cover eight grades. Schools

[13]In any case, observable differences between groups are accounted for by the difference-in-differences specification. In all three specifications, the controls capture the effect on outcomes of any differences in observable variables from the pre- to post-reform period in the type of children in short-day and reform schools and the Post term captures any trends over time in achievement that occur across all schools. In Model III, the school fixed effects also account for all unobservable differences betweens schools that occur at baseline and do not change over time, and for the mean over time of any school-year shocks μ_{st}.

Table 7.11: Control variables at child level for eight-year-olds in 2002 and 2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Older cohort			Younger cohort			Differences p val		Differences q val		Second diff.	
	Short-day	Reform	Total	Short-day	Reform	Total	(2) - (1)	(5) - (4)	(2) - (1)	(5) - (4)	p val	q val
Child controls												
Male	0.50	0.53	0.51	0.53	0.52	0.52	0.65	0.97	0.91	0.97	0.69	0.90
	(0.02)	(0.07)	(0.02)	(0.02)	(0.03)	(0.02)						
Stunted	0.23	0.25	0.23	0.16	0.13	0.15	0.81	0.42	0.91	0.58	0.66	0.90
	(0.03)	(0.08)	(0.03)	(0.01)	(0.02)	(0.01)						
Father has	0.49	0.39	0.47	0.57	0.41	0.54	[0.36]	[0.02]**	[0.91]	[0.22]	0.73	0.90
no education	(0.06)	(0.06)	(0.05)	(0.04)	(0.04)	(0.04)						
Father has	0.23	0.22	0.23	0.20	0.22	0.20						
Grade 1-4	(0.04)	(0.05)	(0.03)	(0.02)	(0.05)	(0.02)						
Father has	0.28	0.39	0.30	0.23	0.37	0.26						
> Grade 4	(0.06)	(0.07)	(0.05)	(0.04)	(0.05)	(0.03)						
Consumer	0.13	0.26	0.15	0.26	0.35	0.28	0.00***	0.11	0.01**	0.31	0.17	0.68
durables index	(0.02)	(0.03)	(0.02)	(0.02)	(0.04)	(0.02)						
Grade child is	1.57	1.56	1.57	1.83	1.67	1.80	0.91	0.27	0.91	0.46	0.39	0.90
in	(0.05)	(0.12)	(0.05)	(0.08)	(0.10)	(0.07)						
Age of child in	96.51	96.58	96.52	98.90	98.93	98.91	0.87	0.81	0.91	0.89	0.79	0.90
months	(0.29)	(0.36)	(0.25)	(0.18)	(0.26)	(0.15)						
School controls												
Urban school	0.43	0.82	0.49	0.32	0.56	0.37	0.02**	0.29	0.08*	0.46	0.12	0.68
	(0.11)	(0.12)	(0.10)	(0.09)	(0.14)	(0.08)						
Fewer than 8	0.12	0.14	0.12	0.43	0.47	0.44	0.80	0.81	0.91	0.89	0.92	0.92
grades	(0.06)	(0.08)	(0.06)	(0.09)	(0.13)	(0.08)						
Children	532			1,256			532	1,256	1,788	1,788		
Schools	59			108			59	108	114			

*p<0.01 **p<0.05 ***p<0.01. T stats, adjusted Wald tests and cluster-robust standard errors are used, where the cluster is the school. Column 7 is the p-value for the difference between Columns 1 and 2. Column 8 is the p-value for the difference between Columns 4 and 5. For ordinal variables, Columns 7 and 8 show the p-value of a Pearson chi-square test for clustered data of whether there are significant differences in the distribution across categories among children in reform and short-day schools. These are enclosed in square brackets. If the p-value of the adjusted Wald test of the difference is significant, the difference is starred. Columns 9 and 10 give q-values, which correct the p-values in Columns 7 and 8 for multiple testing. Column 11 gives the p-value for the second difference. Column 12 gives the q-value.

Table 7.12: Average fitted pre-treatment differences in scores for eight-year-olds in 2002

	Reading		Writing		Numeracy	
	(I)	(II)	(I)	(II)	(I)	(II)
Reform	2.90**	1.80	0.87	0.56	0.89	0.72
	(1.33)	(0.80)	(0.33)	(0.23)	(0.26)	(0.23)
Controls	No	Yes	No	Yes	No	Yes
School F.E.	No	No	No	No	No	No
Grade F.E.	No	Yes	No	Yes	No	Yes
Children	532	532	532	532	532	532
Schools	59	59	59	59	59	59
Adj. Wald test: F stat	5.36	10.75	0.13	5.47	0.17	3.10
Adj. Wald test: Prob>F	0.02	0.00	0.72	0.00	0.69	0.00

Coefficients are odds ratios, so a coefficient less than one denotes a negative effect. T stats, adjusted Wald tests and cluster-robust standard errors are used, where the cluster is the school. *p<0.10 **p<0.05 ***p<0.01.

which cover fewer than eight grades tend to have been more recently built and have fewer resources.

More children attend rural schools in 2009 (64 per cent) than in 2002 (52 per cent). Within a site, Young Lives families either live in rural or urban areas (this sample does not include younger cohort children who have moved). Thus the spread of the younger and older cohort between Young Lives rural and urban sites is the same. The shift in urban school attendance reflects the building of schools closer to children in rural areas.

In an additional check, in Table 7.12, I fit regression models only for children in 2002, before the reform was applied. If reform schools had systematically lower or higher achievement before the reform was implemented, this might suggest that short-day and reform schools were likely to change in different and unobservable ways. Model I does not include controls; while Model II includes child controls, school controls and grade fixed effects. There are no significant pre-treatment differences between short-day and reform schools in writing or numeracy. There are significant differences in reading in Model I, with no controls. However, these disappear when controls are added for child and school variables. They also disappear when corrections for multiple testing are applied.

Finally, interviews with regional education bureau officials indicate that there was no policy of targeting particular types of schools systematically for the full-day reform. Particular types of districts were not targeted to implement the reform, and schools did not have to have certain numbers of classrooms or teachers available to implement the reform. In addition, the probability that a school implements the reform is not correlated with a wide range of observable variables, indicating that district-level bureaucrats were not unwittingly

targeting particular types of schools, at least on observable variables. Table 7.13 shows a linear regression which examines associations between the probability that a school applied the full-day reform in Grades 1–4 between 2002 and 2009 and various combinations of control variables.

The regression in Column 1 includes only the control variables describing characteristics of the school at baseline in 2002, while the regression in Column 2 includes regional dummies. There is variation between regions in the likelihood of implementing the reform: Addis Ababa and Tigray are more likely to implement the reform than other regions. This suggests that the process described by federal officials occurred in practice: the reform was sent from the federal government to regions to districts and then schools. Although urban schools are more likely to implement the reform in Model I, this effect disappears once regional effects are accounted for, suggesting that this is a feature of schools in Addis Ababa being more likely to apply the reform. Regional variation will be captured by the school fixed effects in Model III as they are time-invariant. Column 3 shows that implementation of the reform is not correlated with the level of contact that the school has with the district. It is not the case that schools which are otherwise more heavily monitored (and might thus be better quality) implemented the reform.

Columns 4-7 of Table 7.13 include each of a set of three indices of school quality variables. These are measured in 2009 after the reform was implemented, so reverse causality is possible, and they are also only available for a subset of schools. The school resources index is the first component from a principal components analysis of variables measuring school resources captured in Table 7.8: whether the average classroom is brick, the type of latrine, whether the school has electricity, piped drinking water, or a library, whether the school is a cluster resource centre (a central school with more teaching resources that serves as a resource for other schools in the district), and the proportion of teachers with degrees and diplomas. The overcrowding index includes the variables in the second panel of Table 7.8. The staff motivation index includes the average salary and benefits for Grade 1 to 4 teachers and the principal, the measures of principal and teacher job satisfaction and motivation and the measures of teacher absenteeism.

Implementing the reform is not correlated with any observable variables capturing the demographics of children in the school, including the average level of fathers' education, average consumer durables index, and average level of stunting of children in the school. It is

Table 7.13: Correlates of if schools applied the full day reform between 2002 and 2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
School demographics							
Mean consumer durables index	0.29	0.29	−0.036	−0.093	−0.063	0.23	0.023
	(0.32)	(0.34)	(0.34)	(0.36)	(0.35)	(0.39)	(0.41)
Mean father's education (grade)	0.018	0.0077	0.022	0.018	0.022	0.016	0.018
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.018)	(0.019)
Mean stunting	0.011	0.069	0.085	0.047	0.044	−0.014	−0.014
	(0.17)	(0.16)	(0.16)	(0.17)	(0.17)	(0.20)	(0.20)
Fewer than 8 grades	0.17*	0.19**	0.15*	0.074	0.10	0.13	−0.025
	(0.086)	(0.083)	(0.085)	(0.14)	(0.091)	(0.097)	(0.15)
Urban school	0.26***	0.13	−0.067	−0.040	−0.029	−0.20	−0.071
	(0.097)	(0.10)	(0.10)	(0.12)	(0.11)	(0.12)	(0.14)
Region (Oromia=0)							
Addis Ababa		0.37**	0.19	0.32*	0.35*	0.35**	0.44**
		(0.15)	(0.16)	(0.18)	(0.18)	(0.17)	(0.21)
Tigray		0.21*	0.039	0.068	−0.013	0.057	0.061
		(0.12)	(0.11)	(0.13)	(0.12)	(0.13)	(0.14)
Amhara		−0.028	−0.20*	−0.20*	−0.24**	−0.19	−0.18
		(0.11)	(0.11)	(0.12)	(0.11)	(0.12)	(0.14)
SNNP		−0.18	−0.31**	−0.25*	−0.29**	−0.28*	−0.31*
		(0.13)	(0.13)	(0.13)	(0.13)	(0.15)	(0.16)
District officials visit school > once/fortnight			0.13				0.11
			(0.089)				(0.10)
School resources index				−0.035			−0.032
				(0.051)			(0.058)
Overcrowding index					0.065		0.087
					(0.046)		(0.056)
Staff motivation index						0.0029	−0.0063
						(0.041)	(0.044)
Constant	−0.079	−0.072	0.083	0.15	0.16	0.12	0.16
	(0.094)	(0.11)	(0.11)	(0.12)	(0.12)	(0.12)	(0.13)
Schools	114	114	88	84	84	74	70
F stat	4.78	4.79	2.34	2.26	2.48	2.18	2.03
Adj. R squared	0.14	0.23	0.13	0.13	0.15	0.14	0.16

Coefficients are from a standard linear regression. *p<0.10 **p<0.05 ***p<0.01.

also not correlated with indices of school resourcing, overcrowding or teacher motivation. It remains possible that implementing the reform is correlated with unobservable variables.

C. Changes in group composition

One might expect parents to think that more instructional time signals a better quality school, and to move children into schools which had applied the full-day reform, resulting in differences in unobservables (such as parental motivation or child ability) between children in short-day and reform schools. Poorer students might also moved out of reform schools in response to the reform because they could not take time from work to attend a longer

school day, also resulting in changes in the composition of the student body on unobservable variables.

However, this issue is of limited concern in this context. Firstly, in Ethiopia, children have fewer choices of school than in many other countries. In rural areas, there is usually one Grade 1–4 primary school per village, and one Grade 1–8 primary school for every three or four villages. In this sample, even children who had more than one choice of school rarely had the ability to choose between short-day and reform schools. Table 7.7 shows that in 17 of 30 districts either all the schools, or none, had applied the reform.

Furthermore, even if parents had a choice of school, they often do not have the resources to take advantage of these choices. Distance to school strongly influences where parents send children to school, possibly because of the cost of transport, worries that girls will not be safe on the way to school, or the fact that even the closest school can be quite far away (Camfield, 2009). Parents are also unlikely to be aware of exactly when a school was implementing the reform, in order to move children to a different school.

Indeed, the difference-in-differences estimates in Column 9 of Table 7.10 are not significantly different from zero in all observable covariates and the point estimates are close to zero in most cases, indicating that time trends in the composition of the student body in short-day and reform schools on these covariates were approximately equal. There is little evidence that certain types of children moved into or out of short-day or reform schools in response to the reform. There are differences in the distribution across the categories of father's education between shift and reform schools in the younger cohort. (There were also some differences at baseline, although these are not significant.) However, there is no significant difference in how patterns in father's education have changed over time between shift and reform schools, as shown in Column 9, which might indicate a change in the composition of children in each type of school.

Similarly, there may be a worry that once changes in the school day have been made, better, more motivated teachers may sort into schools based on the length of the day. But in Ethiopian government schools, teachers have limited ability to move between schools. After finishing training teachers must teach in a government school in the region where they trained for the first two years, and are allocated to schools by lottery. After teachers have completed this two-year period, they can apply to transfer school, but more transfers are requested than are granted, and transfers may take years. Table 7.8 presents descriptive statistics for a range

of school quality variables in 2009. Table 7.8 shows that there are no significant differences in the qualification levels, motivation levels or absenteeism of teachers in short-day and reform schools after the reform is implemented, both with and without corrections for multiple testing.

VII. Results

In Table 7.14, I show difference-in-difference estimates of the effect of schools changing from shift to full-day on the achievement of children aged eight, in respect of reading, writing and numeracy. Model I includes only variables reflecting whether the school implemented the reform. Model II adds child and school controls and grade fixed effects. Model III, which controls for child- and household-level factors and school fixed effects, provides the most complete estimates. School-level variables are not included in Model III because they only change over time in a tiny fraction of schools.

The full-day reform improves writing and numeracy scores significantly. Model II, without school fixed effects, finds children in reform schools are 1.96 times more likely to be numerate than children in short-day schools, controlling for child and school characteristics (this is $\triangle$, the coefficient on $\text{Post}_t * \text{Reform}_s$ in Equation 7.6). This is significant at the 5 per cent level both with and without corrections for multiple testing. Ordered logit and logit estimates for Model III, which includes school fixed effects, show that children in schools which have implemented the full-day reform are 1.71 times more likely to be numerate than children in those same schools before the reform was applied (this is the coefficient on $\text{Post}_t * \text{Reform}_s$ in Equation 7.7). However, this is significant only at the 10 per cent level without multiple testing corrections and is not significant when these corrections are applied.

For writing, the odds ratio from Model II, without school fixed effects, is 3.17. In Model III, children in schools which have implemented the full-day reform are 3.96 times more likely to be able to write with difficulty or write easily, versus not being able to write, than children in those same schools before the reform was applied (they are also 3.96 times more likely to be able to write easily versus not being able to write or being able to write with difficulty).[14] Both these effects are significant at the 1 per cent level with and without corrections for multiple

[14]Estimates of the treatment effect are not sensitive in magnitude, sign or significance to the imposition of various sample restrictions. One such restriction examines only children in schools where there is one child in each cohort. Another such restriction examines only the sample of children examined in Model III, who are in schools where there is within-school variation in outcomes.

Table 7.14: Difference-in-difference estimates for eight-year-olds in 2002 and 2009

	Reading			Writing			Numeracy		
	(I)	(II)	(III)	(I)	(II)	(III)	(I)	(II)	(III)
Post	0.89	0.84	0.81	0.46***	0.43***	0.28***	0.79	0.73	0.85
	(0.17)	(0.20)	(0.22)	(0.10)	(0.11)	(0.08)	(0.15)	(0.15)	(0.16)
Reform	2.66**	1.53		0.86	0.48*		0.89	0.61*	
	(1.11)	(0.54)		(0.35)	(0.19)		(0.25)	(0.17)	
Post*Reform	0.69	0.91	1.09	2.20*	3.17***	3.96***	1.58	1.96**	1.71*
	(0.32)	(0.36)	(0.43)	(1.03)	(1.40)	(1.98)	(0.53)	(0.57)	(0.55)
Male		0.94	0.99		1.01	1.00		1.13	1.11
		(0.11)	(0.11)		(0.11)	(0.11)		(0.14)	(0.16)
Stunted		0.46***	0.67***		0.48***	0.64***		0.67***	0.93
		(0.06)	(0.10)		(0.07)	(0.10)		(0.10)	(0.15)
Father has Grade 1-4		0.94	1.11		0.94	1.42**		0.95	0.98
		(0.13)	(0.17)		(0.14)	(0.24)		(0.14)	(0.15)
Father has >Grade 4		1.24	1.65***		1.16	1.31*		1.30	1.21
		(0.24)	(0.24)		(0.23)	(0.21)		(0.24)	(0.22)
Durables index		3.40***	1.88		1.49	0.83		2.23*	1.60
		(1.54)	(0.88)		(0.83)	(0.40)		(1.00)	(0.60)
Age of child		1.05***	1.02*		1.05***	1.03**		1.05***	1.03**
		(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)
Urban school		3.51***			3.68***			1.90***	
		(1.06)			(1.12)			(0.39)	
School < 8 grades		0.65*			0.67			0.66	
		(0.16)			(0.20)			(0.17)	
School F.E.	No	No	Yes	No	No	Yes	No	No	Yes
Grade F.E.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Children	1788	1788	4698	1788	1788	3037	1788	1788	1734
Schools	114	114	79	114	114	72	114	114	74
Children dropped			666			539			54
Schools dropped			147			105			40
Adj. Wald F stat	2.38	12.85	245.37	4.23	12.47	349.30	0.93	10.84	11.93
Adj. Wald Prob>F	0.07	0.00	0.00	0.01	0.00	0.00	0.43	0.00	0.00

Coefficients are odds ratios, so a coefficient less than one denotes a negative effect. t stats, adjusted Wald tests and cluster-robust standard errors are used, where the cluster is the school. *p<0.10 **p<0.05 ***p<0.01. The sample sizes for Model III for reading and writing reflect the replications of each individual generated for the Blow Up and Cluster estimator used to account for school fixed effects. Schools with no within school variation in outcomes are dropped from Model III. The number of children and schools dropped are shown in the rows 'Children dropped' and 'Schools dropped'. Results do not correct for multiple testing: if this changes results this is noted in the text.

Table 7.15: Heterogeneous treatment effects for eight-year-olds in 2002 and 2009

ATT	Boys	Girls	Median durables		Stunted		Urban	Rural
			Above	Below	Yes	No		
Reading	0.98	0.72	1.15	0.62	0.64	0.91	0.98	1.17
	(0.46)	(0.41)	(0.45)	(0.55)	(0.68)	(0.39)	(0.36)	(1.52)
Difference	1.35		1.85		0.58		0.84	
	(0.88)		(1.73)		(0.60)		(1.12)	
p-value	0.646		0.438		0.600		0.89	
Writing	3.46***	2.76*	4.72**	1.56	2.60	4.31***	3.47***	2.93
	(1.52)	(1.63)	(2.33)	(1.34)	(1.67)	(1.80)	(1.57)	(3.71)
Difference	1.25		3.02		0.19*		1.18	
	(0.58)		(2.67)		(0.19)		(1.57)	
p-value	0.626		0.196		0.089		0.899	
Numeracy	1.36	2.77**	2.12*	1.21	0.65	2.49***	2.53**	0.85
	(0.56)	(1.24)	(0.82)	(0.63)	(0.47)	(0.83)	(0.91)	(0.72)
Difference	0.49		1.76		0.26*		2.98	
	(0.29)		(1.14)		(0.21)		(2.77)	
p-value	0.224		0.563		0.086		0.239	

The median for wealth is in the sample of Young Lives households in the district where the Young Lives child lives. Coefficients are odds ratios, so a coefficient less than one denotes a negative effect. T stats, adjusted Wald tests and cluster-robust standard errors are used, where the cluster is the school. *p<0.10 **p<0.05 ***p<0.01. All regressions use the specification for Model II, which includes child and school controls and grade fixed effects but not school fixed effects. The difference between the coefficients for boys and girls, for example, is the coefficient on the term Post*Reform*Male.

testing. The full-day reform has no significant effect on reading, although these results should be treated with slight caution because of pre-treatment differences between reform and shift schools.

The control variables behave as expected. There are no significant differences in achievement between boys and girls aged eight, as is found in other work in Ethiopia (Ministry of Education/USAID, 2008). Children who are stunted perform less well on all achievement measures. Children whose fathers are more educated perform better on all measures, although differences are not always significant. Children whose households are wealthier perform better, although the coefficient on the consumer durables index is not always significant. This suggests that household wealth is correlated with time-invariant school-level unobservables, which may capture variation in school quality. In Model II, which does not include school fixed effects, children in urban schools perform better on all measures. Children in schools with fewer than eight grades, which tend to have lower levels of resources, perform worse, but coefficients are not statistically significant.

In Table 7.15, I examine whether treatment effects differ for different types of children. This analysis has not been possible in most papers on the effect of increasing time in instruction because data on children's achievement at school level is collected at schools and does not

contain child- or household-level variables. However, this data matches household data with data on schools, enabling such analysis. Equation 7.8 is:

$$\begin{aligned} A &= \alpha_0 + \alpha_1 \text{Post} + \alpha_2 \text{Reform} + \alpha_3 \text{Post*Reform} + \alpha_4 \text{Male} + \alpha_5 \text{Post*Male} + \alpha_6 \text{Reform} * \text{Male} \\ &\quad + \alpha_7 \text{Post} * \text{Reform} * \text{Male} + f(X_i) + g(Y_i) + \phi_g + \epsilon \end{aligned}$$

$f(X_i)$ includes all other control variables included in Equation 7.6. The treatment effect for girls is the coefficient α_3, captured as the coefficient in the column 'Girls', which is 0.72 for reading in Table 7.15 and not statistically different from zero. The treatment effect for boys is $\alpha_3 + \alpha_7$, which is 0.98 for reading and also not statistically different from zero. The coefficient labelled 'Difference' is α_7, the coefficient on Post $*$ Reform $*$ Male in Equation 7.8, is the difference between the treatment effect for boys and the treatment effect for girls. If $\alpha_5 \neq 0$, there is a significant difference in the treatment effects for boys and girls. There are no treatment effects on reading for either boys or girls and no differences between treatment effects. Indeed, lengthening the school day does not appear to affect reading scores for all groups of children examined.

For writing, there are significant positive treatment effects for boys and girls. For numeracy, there are significant positive treatment effects for girls, but not for boys, although the differences in the treatment effect between genders is not statistically significant. Thus there are not systematic patterns in treatment effects by gender.

There is suggestive evidence that there are larger treatment effects for children whose households lie above the median level for consumer durables, which I use as a proxy for household assets. There are no statistically significant benefits of the full-day reform for children from poorer households, and large positive improvements in numeracy and writing for children from wealthier households, although the differences between better-off and poorer children are not significant. One explanation may be that wealthier children have better nutrition and access to school materials and thus learn more in the same amount of time than poorer children.

Similarly, stunted children do not benefit from increase in time in instruction, while children who are not stunted derive a large benefit. However, differences in the treatment effect between stunted and non-stunted children are smaller and not statistically significant. Stunted children may learn less in a unit of time than non-stunted children as there may be

long-term damage to their ability to concentrate (Powell et al., 1998). It is likely that long-term stunting is correlated with poor current levels of nutrition, and children who are hungry are unlikely to benefit from more time at school.

Finally, there is suggestive evidence that higher scores among children who experience the full-day reform may be driven largely by substantial improvements by children in urban schools, which are also likely to be better-quality. Children in urban reform schools do substantially better than children in urban shift schools on both writing and numeracy scores. In contrast, children in rural reform schools do not do significantly better than children in rural shift schools. This is unsurprising: children derive more benefit from an increase in time in instruction when the quality of instruction is higher.

VIII. Discussion

Many of the results found are consistent with previous literature. Most other research also finds effects on numeracy but much smaller or no effects on reading or language (Sims, 2008; Brown and Saks, 1987; Lavy, 2012; Bellei, 2009; DeCicca, 2007; Marcotte, 2007). Other studies also larger treatment effects for younger children (Marcotte, 2007; Lavy, 2010; Brown and Saks, 1987).

However, effects are larger than others in the literature. In developed or middle-income countries, six out of seven studies based on evaluations of reforms find positive effects on achievement of between 0.009 and 0.12 standard deviations of test scores, mostly for small changes in instructional time of around 10 per cent in schools with days of six or more hours. I find larger effects: the odds ratio of 1.951 for numeracy (p=0.023) is equivalent to roughly 0.368 standard deviations for numeracy, while the odds ratio of 3.173 for writing (p=0.01) is equivalent to 0.637 standard deviations.[15]

Firstly, the achievement measures used are very basic and are categorical. Point estimates on odds ratios are by definition wider. The 95 per cent confidence interval for numeracy is (1.098; 3.470), or from 0.051 to 0.686 standard deviations using the same conversion; for writing it is (1.327; 7.586) or from 0.156 to 1.117 standard deviations. The sign and significance, rather than exact magnitude, of the coefficients are of interest, and further research should

[15]The logistic and normal distributions differ little except in the tails of the distributions. The standard logistic distribution has variance $\frac{\pi^2}{3}$. A difference in normal equivalent deviate can be converted into the approximate difference in log odds by multiplying by $\frac{\pi}{\sqrt{3}}$, which is 1.81 (to 2 decimal places). Log odds can then be transformed to an odds ratio (Chinn, 2000).

consider such changes using more common achievement measures. The research suggests the importance of analysing the same reform in Ethiopia using better measure of achievement.

But, second, the effects are within the range found by other studies. If one assumes that the returns to time in instruction are linear, a 30 per cent change in instructional time in other studies would have yielded at maximum a 0.36 standard deviation effect size. Other studies which examine large changes in instructional time find slightly smaller but similar effects for numeracy, although lower effects for language. Bellei (2009) finds in Chile that a 27 per cent increase, from a four-and-a-half to six hour day in Grade 10, increased student test scores in third grade by 0.07 standard deviations per year in mathematics and 0.05 standard deviations in language. These effect sizes are roughly equivalent to odds ratios of 1.135 for maths and 1.082 for language. Cerdan-Infantes and Vermeersch (2007) find that a 100 per cent increase in Grade 6 from a three-and-a-half to a seven hour day in Uruguay increased student test scores in third grade by 0.063 standard deviations per year in mathematics and 0.044 standard deviations in language. These effect sizes are roughly equivalent to odds ratios of 1.121 for maths and 1.095 for language.

Furthermore, effects may plausibly be larger. In Ethiopia, schools have only four hours of instructional time before the reform, so instructional time increases by roughly 30 per cent because of the reform. Other research has largely examined increases of roughly 10 per cent in instructional time in countries where the school days were longer to begin with. There are diminishing marginal returns to instructional time (Lavy, 2010). If there are diminishing marginal returns, increases in instructional time will have larger effects when there are low initial amounts of instruction, as in this study, than when the initial amounts of instruction were higher. Most studies examine older children than those in this sample, who are 8 years old and in Grades 1 to 3, and other evidence suggests larger treatment effects for younger children. Currently many primary schools in Ethiopia implement the reform only in the higher grades and many countries lengthen the school day as children get older, but this may not be an optimal use of resources.

Finally, there may be a more theoretical reason for variation in estimated coefficients, which applies to most estimation of the effects school inputs on test scores (Glewwe, 2002; Hanushek, 2003). It is often difficult to establish whether studies are estimating the direct effect of a policy holding all other factors constant or the effect including beneficiaries' responses (Glewwe and Kremer, 2006; Glewwe et al., 2014). Empirical literature in public

finance often disentangles the two (Meyer, 1990; Cutler and Gruber, 1996; Moffitt, 2001), but the empirical literature estimating education production functions has largely neglected to explore household reoptimisation in response to public spending, with work on school feeding schemes as an exception (Powell et al., 1998; Jacoby, 2002). The few studies that collect information on household responses to interventions at school level show that the direct effect of the intervention and the effect including household responses are often significantly different (Das et al., 2013; Pop-Eleches and Urquiola, 2011).

In a simple education production function, A is skills learned, S is years of schooling, **Q**, **C**, **H** and **I** are vectors of school and teacher characteristics; child characteristics, including ability; household inputs, including parents' taste for schooling and parents' education; and school inputs under the control of parents, such as children's daily attendance and purchases of textbooks and stationery (this presentation is due to Glewwe et al., 2004, 2014). **C** and**H** are exogenous to the household; any child characteristics that are endogenous to the household that affect A are included in **I**, all elements of which are endogenous. This structural relationship holds regardless of the actions of schools, parents or teachers.

$$A = a(S, \mathbf{Q}, \mathbf{C}, \mathbf{H}, \mathbf{I}) \tag{7.10}$$

If there is one school,**Q** and **P**, a vector of prices for schooling inputs paid for by the household, are exogenous from the household's perspective. Given **Q**, **P**, and other constraints, parents choose S and **I** to maximise household utility.[16]

$$S = f(\mathbf{Q}, \mathbf{C}, \mathbf{H}, \mathbf{P}) \tag{7.11}$$

$$\mathbf{I} = f(\mathbf{Q}, \mathbf{C}, \mathbf{H}, \mathbf{P}) \tag{7.12}$$

Inserting 7.11 and 7.12 into 7.10 yields a reduced form, causal relationship which substitutes out all endogenous inputs, including inputs which are a function of **Q**. But it is not a standard production function, because it includes prices and reflects household preferences (in **H**).

$$A = h(\mathbf{Q}, \mathbf{C}, \mathbf{H}, \mathbf{P}) \tag{7.13}$$

[16]If there is more than one school, parents optimise utility, given available combinations of **Q** and **P**.

If there is an exogenous change in Q_i, one element of **Q**, such as lengthening of the school day, the partial derivative with respect to Q_i of the structural form of the production function (Equation 7.10) shows how a change in $Q_i$affects A when all other variables are held constant. It does not capture any changes in S and **I** in response to the change in Q_i. However, parents and children may adjust S and **I** in response to a change in **Q**. Longer school days may be harder to combine with work, so children may drop out earlier, reducing S. Many adjustments to **I**, which includes daily attendance and effort in school, may occur. Children may think less time on homework is needed if the school day is longer, or that less effort during the school day is required. Alternatively, they may think school quality is improving and exert more effort as they perceive education to be a more valuable investment. Such changes are better captured by estimating the impact of a change in Q_i on A from the reduced form (Equation 7.13). This total derivative shows what happens to A after a change in an element in **Q** and captures both the direct (structural) effects (the partial derivative) and indirect (behavioural) effects – agents' changes in S and **I** as they optimise in response to the change in Q_i.[17]

Randomised controlled trials usually capture the total derivative, because follow-ups are conducted some time after interventions, which allows agents to adjust other inputs. The effect captured by retrospective studies depends on the source of variation exploited. If any variation in unobserved inputs is determined by a random process, occurring only after decision-makers choose observed inputs, the effect of a change in Q_i will capture an unbiased estimate of the partial derivative (Glewwe et al., 2004). Then Q_i and unobserved inputs are orthogonal. In studies of the effects of instructional time relying on variation in the number of days before standardised tests or variation in school days because of unanticipated weather closures, parent inputs into spending on children's education have likely already been chosen, but children may vary input in the form of effort and time spent on homework. In studies, like this one, relying on variation due to a reform which changes the length of the school day, both parents and children may alter behaviour in response.

Levin and Tsang (1987) model student responses to changes in instructional time and assume that there are trade-offs between the time spent in school and the effort students put into learning. Students have an optimum amount of learning activity that maximises

[17]If parents can spend more on some consumer good because they can spend less on textbooks, and their welfare improves, this will not be captured by the total derivative. However, the total derivative will capture the decrease in A due to a decrease in parents' financial contributions. The partial derivative, in contrast, ignores both improved household consumption and decreased achievement. Because the welfare effects of reactions to an increase in $Q_i$tend to cancel each other out, the partial derivative is a better approximation of the overall welfare effect (Glewwe et al., 2014).

their utility. If a student is at their optimum amount of learning and there is an externally imposed increase in time spent in learning, they will respond by reducing effort so that their amount of learning is unchanged. If the increase in time is small, the model predicts students can reduce effort so there is no overall increase in learning activity. If the increase is large, students cannot reduce effort enough to achieve equilibrium. Achievement is predicted to increase by a small amount, because the increase in time in school is slightly larger than the decrease in student effort. They not only account for children's responses to a reform, but assume their responses will be of a particular form.

However, this assumption need not apply. If children are receiving a small initial amount of instruction, as in short-day schools in this study, they may not yet be at the equilibrium point where they are engaging in the optimum amount of learning activity. If they are highly motivated in general or because they perceive themselves to be attending a better quality school, they may even increase effort in response to the reform. There may thus be some room to increase time in instruction, overall learning activity and hence student scores without triggering a reduction in student effort.

IX. Conclusion

In qualitative interviews, national and regional officials and nearly all teachers and principals articulated why they believed teaching for a full day improves education quality: an increase in instructional time gives teachers more time to cover the curriculum, explain concepts in different ways and use different methods of instruction; and gives children more time-on-task in a classroom setting to absorb concepts and practice skills. However, many economists of education, as well as the World Bank and UNESCO, argue that lengthening the school day is an inefficient use of limited resources. Evidence from developed and middle-income countries finds that increases in instructional time have only very small effects on achievement, of between 0.009 and 0.12 standard deviations of the distribution of student test scores. Furthermore, lengthening the school day can be costly in systems where large numbers of schools teach in double shifts.

The research design exploits a reform where schools were told by districts to lengthen the day, reducing the the likelihood of selection bias from more motivated principals or communities selecting into applying the reform. In addition, I use a repeated cross-section of children and can use a difference-in-differences estimation strategy, which does not quire

that treatment is randomly assigned either to districts or to schools. It only requires the assumption of equal trends: in the absence of the full-day reform, the average unobserved characteristics of short-day and reform schools and children in these schools would change over time in the same way. This appears plausible: children in short-day and reform schools are similar on observable characteristics at baseline; the schools are similar on a wide range of observable school quality characteristics at endline; achievement scores are similar in short-day and reform schools before the reform once child characteristics are controlled for; and there are no observable school characteristics other than the region the school is in which predict whether the school implemented the reform. It is also difficult for children or teachers to select into schools.

This chapter shows that a lengthening the amount of instructional time children receive has large, positive effects on writing and maths scores, but not on reading. I also find significant differences in the effect of the reform on different types of children. The reform has larger benefits for children in urban schools who are also likely to be in better schools. It also has larger benefits for children who may learn more in from a unit of time, such as children who are not stunted and children who are wealthier and may have better access to school materials and nutrition. The reform on its own may exacerbate inequalities between children, without other initiatives to improve the quality of instructional time or to address disadvantages such as poverty or poor nutrition that affect children's ability to learn during the instructional time that they receive.

There are a number of possible explanations for this result. There may simply be constant (or increasing) marginal returns to time in school, although other evidence suggests this is not the case (Lavy, 2010). Teachers in Ethiopia may be highly motivated and have increasing marginal returns to teaching, so they manage to generate more achievement from students even if students do not like the extra hours. Or, alternatively, children may be highly motivated to learn and keep effort the same, or even increase it, when they receive more instruction. In relation to the broader arguments in this thesis, the chapter suggests that understanding teachers' children's preferences and likely responses to policies will improve the likelihood of successful predictions of the effects of policies. There may be wide variation in the magnitude of estimates of the effect of changes in instructional time, depending on the extent to which parents and children change behaviour in response to the reform and the nature of their

responses. Research designs which are able to disentangle policy effects and responses may yield a narrower range of estimates of the effects of changes in instructional time.

8

Conclusion

Human capital models have dominated the economics of education for over half a century. In this thesis, I join other economists, particularly in education, who are puzzled by empirical patterns in behaviour not predicted by standard human capital models, and have turned to a growing literature on how psychological factors affect investment decisions.

Here, I initially demonstrate how my empirical findings answer two of my three research questions. I asked whether parents' self-beliefs and aspirations, and whether children's preferences (particularly for adhering to social roles), matter for decisions about investment in children's education. I have presented strong quantitative and qualitative evidence, respectively, that conceptions of oneself, one's social role and one's future affect decisions about investment in children's education, and that children's preferences more broadly can influence their reactions to, and the overall effects of, education policies. Subsequently, I return to my third research question, which asked whether the fact that aspirations, self-efficacy and locus of control, and identities have empirical relevance in turn requires that assumptions in standard models of decision-making in economics need to be modified or even rejected.

I. Empirical research questions

A. Do parents' self-beliefs and aspirations matter for decisions about investment in children's education?

My first research question is whether parents' self-beliefs and aspirations for their children's education affect their investments in their children's education. In Chapters 4 and 5, I present evidence from a field experiment in which individuals from an isolated district

in rural Ethiopia were invited at random to watch documentaries about how people from similar backgrounds to theirs had improved their socio-economic position. The videos were motivational stories about how people from poor backgrounds succeeded in agriculture or business with limited resources by working hard, planning, setting goals, and persisting through obstacles. There were no other changes in opportunities or constraints for the treatment group. The videos aim to alter people's beliefs about their own capabilities to alter their circumstances through their own actions and to change their aspirations for the future. They draw on social learning theory in psychology, which argues that self-beliefs and aspirations are important influences on choices and can be altered not only through direct experiences but by observational learning from others' experiences.

By using an experimental design, I offer cleaner identification than previous non-experimental work of a link between exposure to the documentary and changes in psychological outcomes and behaviour. Through measurement of self-beliefs and beliefs about others' opportunities, I suggest mechanisms behind behaviour change.

I also build on experimental research in public health examining whether messages communicated by role models are more effective, and experimental research in psychology on the effects of exposing young women in male-dominated fields to female role models. My research brings the experimental approach into the field with adults in a developing country setting and re-surveys them after six months to examine the persistence of changes in psychological outcomes. Although the time horizon is shorter than many field experiments in economics, it is longer than many laboratory experiments.

Chapter 4 shows that, when individuals were followed up six months after the screening, there were significant increases in the extent to which people thought outcomes were contingent on individual actions. These results add support to existing research suggesting that measures of self-belief are not stable over time in adult populations and can be altered through light-touch interventions. There were significant increases in the aspirations of treated individuals, measured both immediately after the screening and six months later. Indices of aspirations and expectations increased by 0.07 and 0.1 standard deviations respectively after six months, although only the latter result is robust to multiple testing corrections. The intervention also increased parents' aspirations and expectations for children's education six months after the screening, by between 0.18 and 0.33 years of schooling, which is robust to multiple testing corrections.

There are increases in self-esteem and well-being and decreases in some measures of risk aversion, but they are small in magnitude and not robust to multiple testing. The intervention has no effect on discount rates. Self-belief measures, aspirations and well-being are correlated in baseline data and theoretically predicted to be correlated, so it is expected that they shift together. Results on risk aversion are unexpected: people with a higher self-concept in the developed world tend to be more, not less, risk-averse. However, these effects are small and not robust to multiple testing.

My design also includes a placebo group, who watched a popular Ethiopian entertainment show of short comedy sketches. I can thus test if impacts are just based on exposure to TV in a remote area, rather than on documentary content. In all but one case, the effect of being invited to the placebo is not significantly different from zero. In most cases, the treatment group's outcomes are significantly different from outcomes among both the placebo and the control group. However, the differences between the treatment and placebo groups are usually slightly smaller in magnitude than those between treatment and control and are usually not robust to multiple testing.

The intervention alters participants' beliefs about the opportunities facing other people in their village, in particular the minimum education level and the maximum income level that they can attain, but only after six months and not straight after the screening. These are largely not robust to multiple testing correction. This measure is an imperfect way to capture objective beliefs about opportunities and constraints; it may also measure participants' optimism or pessimism about available opportunities. Although the documentaries did not provide specific advice or information on returns to particular behaviours I cannot rule out that individuals derive new information or that existing information becomes more salient. Individuals may also become more optimistic. However, there is suggestive evidence that these changes in beliefs about other people's opportunities are driven by changes in self-beliefs rather than by new information. Changes in these beliefs arise only after six months, which may reflect that participants become more positive about their own capabilities, which in turn improves their assessment of other people's opportunities.

Chapter 5 assesses the reduced-form effect of the intervention on individuals' behaviour, with a focus on parents' investment in children's education and other future-oriented behaviour like savings. As outlined at the outset of the chapter, there is extensive longitudinal evidence in economics of relationships between self-beliefs and economic outcomes, par-

ticularly those requiring long-term investment, and some evidence on similar correlations between aspirations and choice of educational investment and occupation. However, there is limited experimental evidence. Interventions targeting only self-beliefs and aspirations have been mainly with children, so evidence does not necessarily apply to adults. Skills training, welfare and cash transfer programmes have measured self-beliefs, aspirations and behaviour and found effects, but it is difficult to disentangle whether interventions alter psychological variables directly or alter socio-economic circumstances, which then alter psychological variables. Laboratory experiments to alter self-beliefs have found that exogenously induced variation in self-beliefs affects behaviour on set tasks in a laboratory setting, but have not measured effects in real-world settings.

The experiment can identify a link between watching the documentary and changes in psychological variables, and between watching the documentary and changes in future-oriented behaviour. I do not argue that changes in psychological variables cause changes in future-oriented behaviour, but merely that an intervention which altered psychological variables also caused changes in behaviour when no other changes occurred. I thus suggest that the behavioural changes were due to psychological changes, the strongest of which were changes in self-beliefs and aspirations, particularly for children's educational attainment.

There are large and significant effects on investments in children's education. The Cohen's d effect size of the intervention on the number of children aged 7 to 15 enrolled in school was 0.18. This is the same as the average effect in a meta-analysis of 34 studies of interventions to improve enrolment in developing countries, such as school-building, providing resources like textbooks and uniforms or conditional cash transfers (Petrosino et al., 2012). Although many effect sizes in the review were measured after a 12 or 24 month follow-up, rather than 6 months, the effects of the documentary on education investments are substantial, particularly given the low intervention cost. Households in the treatment group also spent 31 per cent more on education than the control group. The difference between the treatment and control groups is robust to corrections for multiple testing on both variables (although the difference between the treatment and placebo groups is not), so this is a robust result. This result provides further support for the argument that there is a clear link between these psychological variables and behaviour change, because exposure to the documentary changed parents' educational aspirations and also changed their investment in education.

Exposure to the documentaries also affected individuals' use of financial tools. The treated group had 68 per cent more saved at endline than the control and placebo groups and had taken out 22 per cent more in loans than the control group. However, these results are not robust to corrections for multiple testing. Further research in a larger sample is necessary to test if these results would be large enough to withstand these corrections.

B. Do children's preferences, particularly for adhering to social roles, matter for decisions about investment in children's education?

My second research question is whether children's conceptions of their roles and identities in the household and at school, and the value they attach to fulfilling these roles, affect investments. In Chapter 6, the third of the empirical chapters, I present qualitative evidence from data collected from parents and their children in an Ethiopian village in 2008 and 2010 on how children make decisions about their time allocation to work and school and how they decide to spend money they earn from work. I explore how children's identities as students, financially independent workers, young adults preparing to have their own homes and farms, and obedient, caring sons and daughters and their perceptions of appropriate behaviour for members of each of those categories influence their decisions.

I reject a "frame-dependent" theory of identity (Hoff and Pandey, 2014), which builds on behavioural economics and cognitive psychology and acknowledges that individuals hold multiple identities simultaneously, although at any time they will prioritise one. On this view, the identity schema evoked by the situation influences which behaviours are considered acceptable and which choices individuals make, largely automatically (LeBoeuf, Shafir, and Bayuk, 2010, 49). I argue instead that children's balancing of conflicting prescriptions about how they ought to behave is often conscious and deliberate, rather than automatic. The theory also does not capture that people inhabit their multiple identities simultaneously and that identities are part of their development of plans, goals and priorities at a higher level than immediate situational pressures.

Moreover, frame-dependent theory and behavioural economics urge an alternative to the standard model. I argue rather for a further modification of Akerlof and Kranton's (2000) modification of the standard model. Their modification includes identity, and builds a preference for adhering to one's social role into people's utility function. But where common conceptions of preferences assume that they are self-interested, I argue, firstly, that preferences

to comply with social roles are often altruistic. Nonetheless, they can be accommodated by slightly expanding standard assumptions about preferences. Secondly, their model of identity accommodates only one identity at a time. However, it could be further modified to account for trade-offs between identities. When identities conflict, individuals form a preference between them. Their preference can be different depending on the context. All this can still be accommodated by notions of preference in standard models.

Finally, in their emphasis on people's ability to choose many identities, Akerlof and Kranton (2000) do not adequately account for very real structural constraints on action, both in the form of social structures which regulate economic and other activity and in the form of social norms. These structures and norms are particularly relevant for individuals who are not in situations of social power, such as children who are poor, and particularly girls who face greater family demands. These structures and norms may act as constraints on action rather than as freely chosen identities, in which case other perspectives viewing social norms as constraints may be more accurate (Platteau, 2000).

In Chapter 6, I present an unexpected result regarding how children react to the implementation of education policies. The existing literature on the responses of households to education policies largely examines their responses to policies which increase resource allocations to schools, but often does not capture that households can reallocate resources such that there is no net welfare gain from such a policy (Todd and Wolpin, 2003; Pop-Eleches and Urquiola, 2011; Glewwe, Kremer, and Moulin, 2009; Glewwe and Kremer, 2006). I apply this idea, but within the context of children's allocation of effort rather than household allocation of resources.

The literature on time in school suggests that children desire only a certain amount of learning and calibrate effort per hour depending on the number of hours they spend in school (Levin and Tsang, 1987). On this view, children will decrease effort in response to an increase in the school day, resulting in only small effects on achievement, a result found in much of the empirical literature in the developed world (Sims, 2008; Marcotte, 2007; Lavy, 2012; Pischke, 2007). Partly as a result of this evidence, international organisations like the World Bank and UNESCO have encouraged many developing country governments to adopt four-hour school days and teach two groups of children each day (Bray, 2008).

Contrary to this prediction, I find that a reform to lengthen the Ethiopian primary school day improved test scores. In 2006, the Ethiopian federal government directed school districts

to abolish teaching in shifts and lengthen the school day from four to five-and-a-quarter hours. I use a difference-in-differences specification controlling for time-invariant unobservables at school level on a unique longitudinal dataset. For eight-year-old children, a longer school day improved writing and mathematics scores, but had no significant effect on reading. However, effects are larger among better-off children: children who are not stunted, children from richer households and children in urban schools. (The exception is that the reform has larger positive effects on girls than boys.) The reform thus improves achievement on average, but may exacerbate gaps between wealthier and poorer children.

There are a number of possible explanations for this result. There may simply be constant (or increasing) marginal returns to time in school, although other evidence suggests this is not the case (Lavy, 2010). Teachers in Ethiopia may be highly motivated and have increasing marginal returns to teaching, so they manage to generate more achievement from students even if students do not like the extra hours. Or, alternatively, children may be highly motivated to learn and do not have a diminishing marginal return to time in school, at least up to a certain length of day. The chapter suggests that understanding teachers' and children's preferences and likely responses to policies will improve the likelihood of successful predictions of the effects of policies.

II. Theoretical research question: should standard models be modified or rejected?

My third research question asks whether the fact that aspirations, self-efficacy and locus of control, and identities have empirical relevance in turn requires that standard models of decision-making in economics need to be modified or even rejected. I focus on one dimension of this question: whether the *assumptions* made in standard economic models about beliefs and preferences need to be modified based on empirical findings about the characteristics of these concepts. In Chapter 3, I differentiate between two paths taken by economists when using psychological concepts in economics. Both paths continue to use standard economic *concepts*.

One path, taken by economists working on non-cognitive skills and some economists working on identity, does not really require modifications to the assumptions made in the standard model. For example, work on non-cognitive skills slightly broadens the neoclassical ideas of skills to include psychological characteristics and traits such as personality, persistence

or motivation. But skills are still conceived of as relatively stable and are included in an agent's endowment of ability in standard models (Almlund et al., 2011; Heckman and Kautz, 2012). All other assumptions in the standard model about beliefs and preferences remain unchanged. In similar vein, some work on identity defines identity in relation to preference: within a standard model, agents have a preference to comply with certain identities (Akerlof and Kranton, 2000).

The second path is that taken by behavioural economists. They argue that the assumptions the standard model makes about beliefs, preferences and processes of deliberation are often not accurate representations of actual beliefs, preferences and processes. They use laboratory experiments and field research to describe how agent deliberate and form beliefs and preferences. In Chapter 3, I described four key modifications they suggest to standard models. People often make decisions automatically, by following habits or using rules of thumb, not through conscious deliberation. They satisfice rather than optimise. They can be self-interested or altruistic, rather than only self-regarding. Their beliefs, preferences and even skills are not fixed, but "frame-dependent" (Hoff and Pandey, 2014, 120) and influenced by contexts, reference points and cues.

In my argument below, I pick a route between these two paths. I argue firstly that concepts used in this thesis – self-beliefs, aspirations and identity – can also be captured using concepts of belief and preference. I suggest some modifications to how these concepts are currently integrated into standard models, based on my empirical results. However, because of the particular characteristics of these psychological concepts, in the main they can be included in standard models without requiring many adjustments of standard assumptions about beliefs and preferences.

Secondly, I argue that continuing to use standard models with some modifications has substantial benefits. I suggest that the assumptions of standard models intentionally simplify the "messy and mysterious internal workings of actors" (Freese, 2009, 98): they are not intended to be accurate or realistic. The benefits of generality and mathematical tractability that they bring to theory are considerable. Thirdly, however, this thesis also highlights that a more fine-grained, empirically grounded understanding of the formation and behaviour of beliefs and preferences is valuable.

A. Self-beliefs, aspirations and identity can be accommodated by modified standard models

A.1 Self-beliefs

When psychological concepts are unstable or context-dependent, this is often used to argue for taking the behavioural economics path. I do find that self-beliefs such as locus of control are not stable over time, in accordance with other recent evidence in economics, but can evolve and change. This implies a need for revision of current empirical strategies in economic research on non-cognitive skills. These often assume concepts like locus of control are exogenous and not a function of the specific outcomes under consideration.

However, my experimental evidence suggests self-beliefs are relatively stable across different domains of life at a point in time. People's underlying self-concept is correlated with beliefs about others' opportunities, risk preferences, and beliefs about the factors causing poverty; and an intervention which changes self-beliefs also changes these beliefs about different domains of life. In other words, the same intervention that causes changes in self-beliefs also causes changes in quite different domains of behaviour.

Standard models do not assume beliefs are completely stable. Assumptions about stability relate to preferences, not beliefs: assumptions about beliefs are less clearly specified.[1] In fact, standard models which account for imperfect information or uncertainty allow that people update beliefs in response to new information. Where the standard model might need to be modified is in the face of evidence that beliefs can be highly context-dependent or change rapidly and unpredictably in response to new triggers or cues – behavioural economics has found evidence that some beliefs behave in this way. However, I have contended that *self-beliefs* do not appear to have these characteristics and do not require modifications to the standard model.

Secondly, much economic work uses measures of locus of control and self-esteem as measures of non-cognitive skills. I argued in Chapter 4 that locus of control and self-esteem should be conceived of as self-beliefs, rather than as non-cognitive skills. They are perceptions about one's abilities and capacities, rather than the capacities themselves. They can be altered without any practical opportunities to gain or practise a skill, simply through persuasion or vicarious experiences. Self-beliefs were, for example, altered by the experiment described in

[1] Furthermore, as I will argue below, even assumptions about preferences allow more variation across time and context than is commonly assumed.

Chapters 4 and 5, which simply exposed people to stories about the experiences of others. Relatedly, self-beliefs are not always correct representations of people's actual performance or capabilities. People interpret their past performance through the lens of their current self-beliefs, so that the same objective performance can be interpreted by different people in different ways.

Standard economic models used to assume that agents have perfect information. Models now commonly allow for imperfect information, which require that beliefs can be incorrect and updated to accommodate new information. But when people form different self-beliefs depending on the same information about their performance, this is not quite a case of imperfect information. Two people might have the same information on their performance or outcomes, but construe or interpret it differently. They may also form different subjective assessments of the probability of various outcomes, based in part on different assessments of their own capabilities. This may be better conceived as a case of a belief formed under uncertainty.Self-beliefs are often broad and relate to future outcomes which are not known and require In any case, philosophers of economics emphasise that this is precisely why the standard model allows that preferences are a subjective ranking and does not require capturing of the substantive content of preferences. Only the agent making choices knows what they take to be the alternatives they face and what they believe the consequences of their choices will be, and forms preferences given these beliefs (Hausman, 2012, 15)

A.2 Aspirations

Psychologists conceive of aspirations as forward-looking goals or targets (Locke and Latham, 1990). I have argued that in economic terms they are the set of future outcomes that people prefer among a set of outcomes they believe to be attainable, based on their beliefs about their own capabilities and about objective constraints and available resources. As such, they are a type of preference. However, it is possible to form preferences over outcomes which one does not believe are attainable. Indeed, game theorists explain players' choices of strategy given assumptions about their preferences over all possible outcomes, including choices which agents are never in a position to select (Rubenstein and Salant, 2008; Reiss, 2013, 62-63). Aspirations are not this type of preference.

I also disagree with a behavioural economics conception of aspirations as reference points which frame decisions, are used to regulate effort, and are revised in response to past

performance (Selten, 1998, 2008; Selten, Pittnauer, and Hohnisch, 2012). This perspective on aspirations is similar to one held by some psychologists, who argue that aspirations and self-beliefs are simply functions of past performance (Vancouver, Thompson, and Williams, 2001).

The definition of aspirations that I have favoured draws on social learning theory. On this basis, aspirations are shaped by self-beliefs as well as information on past performance. Past performance does affect aspirations, but is not the only influence on them. Although it is difficult to establish the relative influence of past performance and self-belief on aspirations, my results show that self-beliefs and aspirations change independently of changes in past or current performance. Straight after watching the video, individuals had not seen any change in performance or outcomes. Nonetheless, both aspirations and self-beliefs changed. While it is possible that they both changed independently, it is more likely that changes in them were linked, given that they were correlated strongly at baseline. This further suggests that aspirations and self-beliefs are strongly related, which is not captured by behavioural economics perspectives on aspirations.

Importantly, taking this perspective on goals and aspirations provides a theoretical framework for integrating these concepts into economics that does not rely on the behavioural economics view of people as satisficers, which is a key element of theory which views aspirations as reference points. This has the important implication that one does not have to depart from core assumptions of neoclassical economics for one to view aspirations as important.

A.3 Identity

In addressing my second research question, I argued against the frame-dependent theory of identity (Hoff and Pandey, 2014) and in favour of further modifying Akerlof and Kranton's (2000) modification of the standard economic model, which includes preferences for behaving in accordance with one's identity but views compliance with identity-based prescriptions about behaviour as often being directly in individuals' self-interest.

My first extension of Akerlof and Kranton's (2000) modification addresses their insistence on accommodating only one identity at a time. This has been workable for empirical research focusing on the effects on behaviour of one fairly fixed identity like caste, gender, religion or ethnic group (Munshi and Rosenzweig, 2006; Platteau, 2000; Bisin and Verdier, 2001). However, the evidence of Chapter 6 made clear in various ways that the model needs to be

further modified to account for trade-offs between identities. In the various circumstances, when identities conflicted the children in effect expressed a preference ranking over their different identities, suggesting which should be prioritised. Moreover, this preference could differ depending on the context, and when a child was in the same context at different points in time.

These instances can still be accommodated by notions of preference in standard models. Behavioural economists construe neoclassicists as suggesting that, every time a child is faced with a choice between work and school, they should have a consistent preference. However, neoclassical economists allow that much depends on how alternatives are specified. Alternatives can be specified to capture everything that will affect an agent's decision (Arrow, 1970). This cannot be so detailed as to lose all generality, but makes more allowance for the dynamics of real choices than behavioural economists tend to do.

Secondly, the children's assessments and reported behaviour my qualitative work suggests that preferences to comply with social roles are often altruistic. There is debate in philosophy of economics about whether altruistic preferences can be defined as preferences. Many standard models of household decision-making commonly assume altruistic preferences. Although economists may, in general, assume that preferences are self-interested, it is possible to slightly expand standard assumptions about preferences but continue to use the concept. My research implies that models of identity need to allow that compliance with identity-related prescriptions, in showing that the children's behaviour is often altruistic and based on moral commitments.

B. There is value in maintaining standard models...

Why try to argue that specific concepts highlighted by social psychology can be captured by concepts in standard economic models? Faced with the wealth of precisely differentiated moral commitments, aversions, whims, ambitions, passions, goals, expectations and desires that make up the conceptual toolkit of psychology, economists might wonder if beliefs and preferences and a few simplifying assumptions about them are not rather blunt instruments (Hausman, 2012, 57).

However, there are some tradeoffs at play. At its most ambitious, neoclassical economics and its application in human capital theory seek to provide a grand, unified, universally applicable set of generalisations about how the "motivational 'forces' with which economics

is mainly concerned" (Hausman, 1992a, 91) – consumerism and profit maximisation – affect all aspects of human behaviour. Indeed, neoclassical theory appealed to philosophers and economists alike because it looked like the mid-twentieth century view of what a science ought to be (Ross and Kincaid, 2009): it explained social phenomena as following from deeper regularities, generalisations or laws (Hempel, 1965) which were elegantly simple, context-free and universal.

Neoclassical economists aim to provide explanations and make accurate empirical predictions about behaviour, not merely to describe and categorise behavioural or mental phenomena. At one extreme, in the instrumentalist school of theory assessment popularised by Milton Friedman and adopted by many economists, theories are assessed solely by whether they provide valid predictions concerning the class of phenomena they intend to explain (Boland, 1989). If theories assume that people make decisions rationally, *as if* maximising expected returns, and generate accurate predictions based on these assumptions, they have achieved their purpose. "Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality" (Friedman, 1953, 153).

Friedman's position is now much less widely held. Indeed, seventy years later, many philosophers of science now argue that there are few real laws (Salmon, 1971, 1990), that the best we can hope for is to isolate causal processes, and that even these cannot be aggregated into an elegant unified framework and may be specific to a particular place and time (Woodward, 2003; Cartwright, 2007, 1983). In any case, it required some philosophical contortions to classify neoclassical assumptions as laws (Rosenberg, 1994; Hausman, 2009), and behavioural economists have demonstrated relatively conclusively that empirical patterns in actual preferences, beliefs and choices violate these assumptions.

Nevertheless, most economists would argue in favour of simple, mathematically tractable, general theories with strong predictive power which make some simplifying assumptions that are not empirically realistic (Stigler, 1965). In that sense, the neoclassical project, while scaled back, still has great influence within the discipline. Indeed, many behavioural economists also value the enterprise of generating relatively general theories of human behaviour, in part so that they can continue to use formal economic models and mathematical tools (Sugden, 2012, 199). Some behavioural economists are at pains to argue that they are working within the neoclassical approach, based on the ideas of equilibrium, efficiency and utility maximisation,

and modifying one or two assumptions, often those which are not core (Camerer and Loewenstein, 2003).

C. ...with a better understanding of when to modify assumptions

If, after all this trouble, we merely conclude that standard models remain broadly intact, what might economists gain from "looking under the hood" (Hausman, 1992b) and drawing on psychology, sociology and other social sciences to understand how preferences and beliefs are formed and how they relate to behaviour? I highlighted in Chapter 3 that, since the Paretian turn, economists focused only on the prediction of choices, and on their consequences. Neoclassical economists prefer to leave it to other social scientists to study the intricate, context-specific processes through which agents form and modify their preferences given their values, motivations, aspirations and beliefs (Hausman, 1992a). Economists take agents' preference rankings and beliefs as given, and study the consequences of choices based on them. Preferences, for example, are a ranking, not a "psychological, explanatory notion", so the concept as used by decision theorists is not intended to describe, differentiate between or understand human motivations (Gibbard, 1998, 250).

The empirical evidence presented in this thesis gives one example of how economists may benefit from developing substantive theories of the formation and behaviour of preferences and beliefs. I am able to argue that construals of oneself, one's role and one's future are captured by concepts of belief and preference. With some qualifications, I can also conclude that they behave according to standard assumptions about beliefs and preferences. But, importantly, my arguments in this regard are empirically based. They apply specifically to these concepts, but may not apply for other types of beliefs and preferences.

Development of substantive theories about the formation and behaviour of preferences and beliefs may have three benefits (Hausman, 1992b). Firstly, such an understanding can illuminate when particular assumptions about beliefs and preferences are more or less likely to apply, which might guide if a theory is appropriate to apply in a new context. In some cases, it will be legitimate to assume that agents have preferences that satisfy standard assumptions. Simple generalisations, such as "People prefer more commodities to fewer", often hold (Hausman, 2012, 118-119). However, other axioms, such as context-independent preferences, may be "a strong idealisation, which will be unreasonable in many contexts" (Hausman, 2012,

63). Economists can well draw on other disciplines to understand how preferences are formed so that they know when it is, and when it is not, sensible to use such idealisations.

Secondly, if a theory fails to produce accurate predictions, empirical evidence might suggest which of a number of underlying assumptions needs to be modified. However, without the ability to differentiate between working and non-functioning parts of a theory, the whole theory would need to be discarded when it fails (Hausman, 1992b). Instead, empirical investigation of the formation and behaviour of beliefs and preferences may lead to viable modifications of parts of the theory, even at its core.

Thus, behavioural economics is already moving from a first stage of criticising the standard model, by demonstrating that people's choices are systematically different from the assumptions of ordinal utility theory, to a further stage of testing whether beliefs and preferences are governed by other regularities (Angner and Loewenstein, 2012). They have also established that some parts of core neoclassical theory can be modified without much change in the broader theoretical edifice: "for example, there is nothing in core neoclassical theory that specifies that people should not care about fairness, that they should weight risky outcomes in a linear fashion, or that they must discount the future exponentially at a constant rate" (Camerer and Loewenstein, 2003, 4). Drawing on psychology and sociology to understand the formation of preferences and beliefs is thereby a critical part of discovering such regularities and making such modifications.

Finally, the application of any theoretical prediction to empirical data centrally relies on an inductive inference from the model world to the real world (Sugden, 2000, 2009, 2013). The assumptions of any model often do not "correspond with – or even with a simplification of – any particular real-world situation" (Sugden, 2000, 504): after all, theories prioritise generality, simplicity and tractability. There are two types of agents, buyers and sellers; two goods, guns and butter. Prices are held constant. Equilibrium obtains.

Evidently, theorists are ultimately engaged in an exercise of persuasion. They ultimately make an inductive argument: they suggest that the existence of a specific, idealised, relationship in the model world implies that it is likely, under certain assumptions, that an analogous relationship may exist in empirical data about the real world. Theorists have to persuade the audience that the model world "could be real – that it describes a state of affairs that is credible, given what we know (or think we know) about the general laws governing events in the real world" (Sugden, 2000, 502-3).

To test our confidence in inductive inferences from model to aspects of the real world, theorists explore the robustness of results to variations in elements of the model that are apparently inessential. But a more important factor is similarity between the model and reality. We have more confidence in our inferences from the model to the real world "the greater the extent to which we can understand the relevant model as a description of how the world *could* be" (Sugden, 2000, 503, italics in original). The knowledge that our assumptions about beliefs and preferences are reasonable and empirically grounded gives us more confidence in applying models in the world; more than enough reason, in my opinion, to look under the hood.

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