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ABSTRACT

Non-cognitive skill formation in poor neighbourhoods of urban India*

Recent labour market research has shown that a good education comprises investment in both cognitive and non-cognitive skills. We examine the impact of a long-term programme designed to raise non-cognitive skills of children and adolescents in slums in Bombay. We use a cross-cutting design with two comparison groups of peers for young adults who have attended the programme until leaving high school to analyse whether, compared to those from a similar environment and background, enrollment in the programme demonstrably raises such skills. We find evidence of substantial impacts on both self-esteem and self-efficacy (of about one standard deviation), as well as evidence of a smaller impact on life evaluation and aspirations. Furthermore, in line with the literature, both self-esteem and self-efficacy are positively related to success in school-leaving examinations and initial labour market outcomes.

JEL Classification: C93 and J24

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1 Introduction

Most people would agree that a good education is more than the acquisition of obvious cognitive skills, such as literacy and numeracy. There are other valuable personal characteristics that are not strictly cognitive, such as aspirations, agency and self-esteem, and these are generally formed during childhood. We usually assume that individuals with these characteristics are more likely to succeed than those who lack them. Employers reflect these beliefs when they seek personal interviews, letters of recommendation, and other personal evaluations, even when test scores and other measures of cognitive ability are available (Jencks (1979):p.122 [36]). Economists such as Heckman and his collaborators are now increasingly concerned with understanding how non-cognitive skills¹ affect socioeconomic outcomes, and whether the failures in building such skills are critical for children from disadvantaged backgrounds (see, for example, Carneiro et al., (2007) [10]; Cunha et al., (2006) [16] ; Heckman et al., (2006) [32]). The growing literature on non-cognitive skills makes it clear that such skills are vital determinant of future outcomes. For instance, as Cunha et al., (2010) [18] point out: "Non-cognitive abilities have direct effects on wages, schooling, teenage pregnancy, smoking, crime, performance on achievement tests, and many other aspects of social and economic life." (page 3). However, unlike the vast literature on investment in cognitive skills, little is known about how non-cognitive skills are formed, and whether initially poor non-cognitive skills can be remedied later in childhood. These are the questions we address using data from highly deprived slum areas of urban Bombay. Using carefully selected comparison groups, we find that those involved in a specific non-cognitive skill formation programme during their childhood and adolescence have higher non-cognitive skills such as agency and self-esteem as young adults, and that these skills correlate well with higher earnings and school performance, even after controlling for cognitive abilities.

While there is plenty of evidence on the effectiveness of interventions to raise cognitive (usually school-based) skills², there is much less on sustained intervention to raise non-cognitive skills, especially in developing countries. The existing literature on non-cognitive skill formation is based mainly on short-term early childhood interventions in the U.S. and U.K., summarised in reviews by Currie (2001) [19] and Grantham-Mcgregor et al. (2007) [30]. Overall, there is agreement that intervention in early childhood is likely to be beneficial. The impact of early childhood programmes such as the Perry School and the Carolina Abecedarian Project in the U.S. on a diverse range of outcomes including school attainment, earnings and antisocial behaviour, were found to be persistent in adulthood, with causal links via non-cognitive skills (Schweinhart (1993) [48] and

¹It should be emphasised that these skills require cognitive processes and as such the term non-cognitive is a misnomer. However, this is in popular use in the economic literature, even if psychosocial competencies might be a better description.

²See, for instance, Orazem and King (2008) [41] for an overview.

Heckman et al., (2006) [32]). While research suggests that early childhood is a critical period for brain development, we cannot conclude that this is the optimal age for child interventions, as there is no clear monotone relationship between brain development and general child development (see Gopnik et al.(1999) [29] and other references in Currie (2001) [19]). There is a dearth of longer-term interventions for school-age children and hence little evidence on the value of interventions in later childhood and adolescence. This study is among the first to offer some evidence on this issue.

In particular, we examine the impact of a long-term intervention targeting non-cognitive skills of children from poor backgrounds by a non-governmental organisation (NGO), Akanksha. Akanksha (which means aspiration in Hindi) focuses on a range of non-cognitive skills including self-esteem, a sense of agency and control, as well as aspirations through the use of workshops, mentoring, drama, art and story-telling. This programme is of tremendous interest as an opportunity to test whether non-cognitive skills can be changed through long-term intervention. We measure the impact of the programme on self-esteem and sense of agency (self-efficacy), and the more general outcomes of life-evaluation and aspirations.

The focus on self-esteem and self-efficacy as core non-cognitive skills is consistent with the evidence from psychology but also in line with practice among economists researching non-cognitive skills. These non-cognitive skills have been established as stable, measurable and strongly predictive of future outcomes by psychologists (Bandura (1977) [5], Donnellan et al., (2005) [24], Trezniewski et al., (2009) [54] and references cited therein and Cobb-Clark and Schurer (2013)[13]). As a consequence they are part of standard measures incorporated in large longitudinal studies such as the National Longitudinal Survey of Youth in the U.S.A., the German Socio-Economic Panel and the Household Income and Labour Dynamics in Australia Survey. Economists researching non-cognitive skills (Heineck and Anger,2010,[33] , Cobb-Clark and Tan, 2011 [12]) have relied on these data and the measures there³. The two other more general indicators, expected life-evaluation and aspirations, refer to subjective perceptions of achievement and ambition, also feature in recent economic analysis (see Deaton (2008) [20]; Ray (2006) [43]).

Akanksha admits children between the ages of six and eight who attend primary school into its after-school programme⁴. It is an intensive programme where the children are tutored for about three hours every day after school. The children are expected to stay in the programme until they leave secondary school. The programme began in the mid 1990s and this study focuses on the first cohort of 60 students to complete the programme in 2007, at ages between seventeen and nineteen. A key feature for the design of this evaluation is that enrollment in this cohort was not randomised so the main challenge

³Heckman et al. [32], use precisely these two measures: the Rotter Locus of Control Scale, which is a specific (early) measure of efficacy, and the Rosenberg Self-Esteem Scale.

⁴There is no connection between the schools that children enrolled in Akanksha attend and the programme offered by Akanksha.

in robustly identifying the impact of a non-randomised intervention ex-post is selection, both into and out of the programme. In the next section we describe the programme in detail and describe the features that might alleviate some of these concerns.

In addition, this study was designed so as to deal with these issues. First, we use purposive sampling to construct two comparison groups to isolate the treatment effect of Akanksha participants from neighbourhood and school. The first comparison group is that of peers from the same neighbourhood (slum) in which the 60 Akanksha participants were raised, sampled from a list of young people of similar age and sex to the treated who had also completed secondary school. We removed any peers who were acquaintances or friends of the participants and retained a group of 50. The second comparison group is that of classmates of the treated in their last two years in secondary school. This group was identified by the treated themselves. Consequently, in addition to controlling for the school effect, the comparison of the treated to their school peers yields a conservative estimate or lower bound on the true impact of the programme to the extent that peer effects matter in the transmission of non-cognitive skills. Secondly, the presence of two purposively different comparison groups allows an investigation of whether unobservable confounders matter for selection and outcomes. In particular, a comparison of the school and community peers can be used to infer potential selection bias arising from school choice; for instance, we assess whether the impact of Akanksha relative to the neighbourhood peers reflects differences in school experiences since selection by parents and pupils into Akanksha may well be correlated with school choice as well. Thirdly, we collected detailed recall data on parental non-cognitive skills and socioeconomic background to control further for observable confounders. Finally, matching methods were used to fine tune the comparison.

Our design allows us to analyse whether, compared to children from a similar environment and background, and controlling for differences in home and school environments, those who had been enrolled in Akanksha have demonstrably higher non-cognitive skills. Overall, we find evidence consistent with the value-added of this programme. The effects are large: it appears to raise self-esteem and efficacy by about one standard deviation of indicators for the relevant population; the effect on life evaluation and aspirations is smaller but still substantial at just under half a standard deviation of each of these indicators.

It might be questioned whether investment in non-cognitive skills for disadvantaged children is a luxury relative to improving schooling. Existing evidence indicates, however, that cognitive and non-cognitive skills are strongly complementary in affecting socioeconomic outcomes (see Heckman et al., (2006) [32] Carneiro et al., (2007) [10]). We also offer some descriptive evidence that differences in non-cognitive skills explain variation in education and employment outcomes that is not explained by cognitive skill differences. We show that, controlling for cognitive skills, higher self-esteem and sense of agency are

associated with better performance on school examinations and higher wages.

In the next section we describe the Akanksha intervention in more detail. Section 3 then places this study in the context of the literature. This is followed by Section 4, which sets out the analytical framework. Section 5 describes the survey, specific outcomes of interest, and the empirical methodology, while Section 6 presents the main results and robustness checks. Before concluding, Section 7 offers some descriptive evidence on the association between non-cognitive skills and key educational and employment outcomes. We conclude with Section 8.

2 The Akanksha Intervention

Akanksha is an NGO that has been active in Bombay for the last 18 years. It aims to raise the non-cognitive skills of deprived children over the long-term, offering a 10 level programme taught in daily after-school three hour sessions. In the first 7 levels the emphasis is on having a good time and building up non-cognitive skills. The last 3 levels help prepare for a job, and build life skills. The skills and values are developed in a variety of ways including lessons, activities, and mentoring schemes.

In the lessons, for instance, the teachers convey notions of various values and skills, such as kindness, compassion, self-confidence and control. Initially, they do this through depicting the personalities, actions and experiences of fictional characters, followed by non-fictional well-known figures and group exercises in which children work to identify these skills. As part of this group work the children keep diaries in which they record their daily encounters with the values and skills that they learn about in class and share in the group. According to Akanksha, applying the skills enables the children to implement what they have learned outside the protected environment of the Akanksha centres.

Another core component of the programme is sports, drama and art activities. Here the children participate in ambitious projects such as the production of an annual musical that force them to take on new challenges, work as a group and persevere. Psychologists believe that among the key determinants of an individual's sense of own agency/ability to attain goals are "mastery" and "vicarious" experiences, where the former refers to own experience of success while the latter to the experience of success of those an individual is surrounded by and can relate to (Bandura(1977) [4]). In addition to providing children with a good time, the use of sports, drama and art, is aimed at providing children with such "mastery" and "vicarious" experiences through guided successful completion of challenging but fun tasks.

Each child in Akanksha also has a mentor, whom they meet once a week to talk through the events of the week as well as think about the aspirations of the child. In addition, regular workshops are held by trained counsellors that help children talk about events that they find traumatic in their daily lives.

In order to understand selection into and out of the programme we interviewed those involved in the recruitment of the treatment group. These children were enrolled in the mid-1990's, and came from 4 different Bombay slum neighbourhoods: Colaba; P De Mello (Victoria Station); Gautam Nagar (Race Course); Mariamma Nagar (Worli). At the time Akanksha was an unknown organisation. Their first campaign was low-key involving a team of three who attempted to publicise the programme both at the community level (in the usual meeting places in the communities), and through visiting mothers with 6-7 year old children⁵. About 15-20 children per community were admitted in this first cohort (the programme has since grown) and taught in a centre usually located at a local school hall. There was no rationing of places, no targeting of a particular group by skills, and no inducement on offer (such as meals or vouchers); the only requirement was that parents had to make sure their children were enrolled in school. This does not throw up additional issues for selection since enrollment in primary school has been near universal at over 98 percent in these slums since the early 1990s. The choice to enrol in Akanksha, therefore, predominantly depended on parental choices; in interviews they reported that school timings and after school child care needs were the most important considerations. Particular abilities or skills in children were not targeted by the programmes, nor mentioned by parents as a reason for enrolment or non-enrolment into the programme.

Selection out of the programme appears to have been due to circumstances outside the control of the children and mostly within the first year. The treatment cohort consisted of about 77 children of whom 60 completed the programme. Ten students dropped out because their families moved away from the slum neighbourhood to slums elsewhere in Bombay. Others dropped out mainly because in the initial period, Akanksha lost access to classrooms near the slum that they had originally offered the children. Since that initial recruitment the programme has grown rapidly. They currently work with over 3,500 children in 58 centres and 6 schools in Bombay and nearby city of Pune. Current attrition rates are 4% per year - the bulk of dropouts occurs within the first two years of enrollment, mainly because of moves by the family or changes in timings in formal school.

3 Literature Review

The primary outcomes of interest in this study are self-esteem and efficacy (also known as agency or mastery). Self-efficacy captures a belief about the link between one's own behaviour and its consequences and one's capability to behave or act to achieve desired outcomes (Rotter (1966) [45]). Individuals who believe that outcomes are due to their own efforts have high self-efficacy, as opposed to those who defer to circumstances outside

⁵This alleviates concerns about systematic within-household selection of which child to enrol as only the 6-7 year-olds were eligible.

their control (Maddux (1991) [38]). More efficacious individuals are generally more active in improving their lives (Rotter(1966) [45]) and work out ways of exercising some measure of control even in the face of limited opportunities (Bandura (1977) [5]). Self-esteem, on the other hand, reflects a person’s overall evaluation of his own worth, in the sense of self-worth or self-respect (Rosenberg (1965) [47]). There is a large literature on how both of these skills are formed and their impact on outcomes. We briefly summarise this literature below. The specific measures used are discussed later in the data section (Section 5).

Both self-efficacy and self-esteem form during childhood and stabilise during adolescence (Sherman (1984) [50]). Studies, using samples of twins, have found that while variance in these skills is in part due to genetic factors, a substantial proportion of it is explained by environmental factors (Trzesniewski et al., (2003) [54]). Carton and Nowicki (1994) [11] offer a review of these factors. The first category includes parental influence; self-esteem and efficacy are positively correlated with having more nurturing, emotionally supportive, and warm parents who are consistent in their use of reward and punishment and encourage autonomy. Further, low self-efficacy and esteem in parents are related to low levels of these skills in children, suggesting persistence rather than mean-reversion in the transmission of these skills. Material circumstances are also an important determinant: Dercon and Krishnan (2009) [22] find a strong relationship between material poverty and self-esteem and efficacy for 12 year-old children across four countries. We focus on these channels of influence as key controls when estimating the impact of Akanksha.

The emphasis on self-esteem and efficacy in this study is motivated by a large literature showing their importance for a diverse range of outcomes. The psychology literature presents ample evidence of the link between these skills and academic and occupational achievement, as well as general physical and mental well-being, and antisocial behaviour⁶. A number of studies in economics have also established a link between this domain of non-cognitive skills and education, employment and socioeconomic outcomes⁷.

There is only limited evidence on the role of specific interventions to raise these non-cognitive skills. As mentioned earlier, early childhood programmes such as the Perry School and the Carolina Abecedarian Project were found to raise non-cognitive skills, leading to persistent effects on educational attainment, earnings and anti-social behaviour

⁶See Bandura(1977) [5]; Swartzter and Fuchs (1996) [53]; Baumeister et al., (2003) [6]; Crocker and Wolfe (2001) [15]; Donnellan et al., (2005) [24].

⁷Carneiro et al., (2007) [10] find that, controlling for cognitive ability, children who have higher non-cognitive skills at age 11 are more likely to stay on at school after the age of 16. Goldsmith et al., (1997) [28] use the U.S. National Longitudinal Survey of Youth to examine the effect of self-esteem on wages. They find that self-esteem has a higher impact on wages than does education. Heckman et al.,(2006) [32] compare the labour outcomes of high-school drop-outs with those of high school graduates with similar cognitive skills but lower non-cognitive skills; they show that those in the latter group have lower wages and higher job turnover.

(Schweinhart (1993) [48] and Heckman et al.,(2006) [32]). However, these were broad-based programmes not targeting a defined set of non-cognitive skills. There appears to be less systematic evidence on whether it is possible to intervene to raise a specific set of non-cognitive skills for children from deprived families or deprived environments, especially in later childhood and in a sustained way; our study is an attempt to offer evidence on this. The problems in designing such studies may be one of the reasons for the limited evidence: the evidence from psychology suggests that these skills are unstable in early adolescence, so that the impact of such an intervention can only be captured over the long term. In particular, it has been argued that these skills are unstable in childhood and adolescence and are most stable (and hence measurable) between early adulthood and middle age (see Trzesniewski et al., (2003, 2009) [54] [55], also Cobb-Clark and Schurer[13]). While our study recognises that these skills are formed during childhood and adolescence, we overcome the problem of unstable measurement by focusing on outcomes in early adulthood of a long-term intervention during childhood and adolescence.

4 Analytical Framework

We now turn to our study design and analytical framework. In order to achieve a plausible identification of impact, we use a design involving two comparison groups in addition to the treatment group. Our treatment group consists of the first set of Akanksha treated, who graduated from school and left Akanksha in the summer of 2007. The two comparison groups consist of (1) peers of the same age and sex from the communities where the treated live and (2) school peers of the treated, also from the same neighbourhood. Our key concern in this design was dealing with selection into the programme and its potential impact on the outcome variables. In addition to controlling for a rich set of observable characteristics of both parents and children, the comparison groups allow us to eliminate unobservable school and neighbourhood effects. Furthermore, we ensured that both comparison groups had completed secondary school to control for any relevant unobservable effects of selection into school completion. This controls for selection out of Akanksha to the extent that it is driven by the same unobservable characteristics as dropping out of school.

The entire sample comes from similar slum neighbourhoods. The treated are therefore treated by both Akanksha and the formal school that they (and the school peer comparison group) attended; the school peer comparison group are "treated" only by the formal school; and the community peer comparison group are not treated by either Akanksha or the formal school attended by the children in Akanksha⁸. We illustrate this below, with constant treatment effects and a linear specification for the outcomes.

⁸This design is similar to a cross-cutting one since apart from the Akanksha intervention, the effect of being in the same school can be seen as an implicit additional treatment.

Define two sets of schools: those attended by Akanksha students (and their classmates), denoted by S_A , and those attended by neighbourhood peers, S_N . Let X denote the vector of background variables including parental background and parental non-cognitive skills.

$$S_i = 1 \text{ if person } i \text{ in } S_A \text{ and } 0 \text{ otherwise}$$

$$W_i = 1 \text{ if person } i \text{ in Akanksha and } 0 \text{ otherwise}$$

We can then denote the outcome for individual i as:

$$Y_i = S_i W_i (Y_{11i}) + S_i (1 - W_i) (Y_{10i}) + (1 - S_i) (1 - W_i) (Y_{00i}), \text{ where}$$

$$Y_{11i} = \tau + \theta_{SA} + X_i \beta + \varepsilon_{11i} : \quad \text{Treated (in Akanksha)} \quad (1)$$

$$Y_{10i} = \theta_{SA} + X_i \beta + \varepsilon_{10i} : \quad \text{Classmate} \quad (2)$$

$$Y_{00i} = \theta_{SN} + X_i \beta + \varepsilon_{00i} : \quad \text{Neighbourhood} \quad (3)$$

τ denotes the constant treatment effect of the Akanksha programme; θ_{SA} and θ_{SN} denote the fixed (or treatment) effect of the school and class attended. Note that these fixed effects are common to Akanksha participants and their schoolmates in the formal school attended, but distinct for neighbourhood peers since they attended a different class and school.

With two comparison groups, we make the following assumption about conditional independence (CIA) of the potential treatment outcomes⁹:

$$\text{Assumption 1 : } [(Y_{00}, Y_{11}, Y_{10}) \perp (W, S)] \mid X$$

Under this assumption, we have:

$$E(Y_i \mid S_i = 1, W_i = 1, X) - E(Y_i \mid S_i = 1, W_i = 0, X) = \tau \quad (4)$$

$$E(Y_i \mid S_i = 1, W_i = 1, X) - E(Y_i \mid S_i = 0, W_i = 0, X) = \tau + (\theta_{SA} - \theta_{SN}) \quad (5)$$

$$E(Y_i \mid S_i = 1, W_i = 0, X) - E(Y_i \mid S_i = 0, W_i = 0, X) = (\theta_{SA} - \theta_{SN}) \quad (6)$$

⁹This assumption amounts to the usual conditional independence assumption that the potential treatment outcomes are independent of the assignment mechanism for any given value of a vector of attributes (X). Note that it implies the absence of unobservables that affect selection into either Akanksha or the neighbourhood school.

Under *Assumption 1*, the estimate obtained from (1) – (2), (difference between treated and classmates) as in (4) must be unbiased. Note that under this assumption, (5) – (6) (the difference in difference between the treated and neighbourhood peer and the schoolmates and neighbourhood peers) must be equal to the estimated impact in (4) (difference between treated and classmates). Furthermore, under this assumption school-specific fixed effects do not matter (or neighbourhood schools are of similar quality)¹⁰ so we would expect the estimate from (6) to be zero. Hence, if *Assumption 1* holds, the pattern of results must be such that the estimated effect from (4) is equal to the estimated effect from (5), while the estimated effect in (6) is zero. We note that this pattern of results is not sufficient to validate the CIA: however, as we will argue below, the potential biases that might lead to the violation of the CIA in the presence of this pattern are unlikely in the context we discuss here. Figure 1 provides a diagrammatic representation of the study design.

This design allows us to address potential selection effects attributable to neighbourhood and school influences, which are considered key determinants of non-cognitive skills (see Rosenberg (1965) [47]). The first comparison group of neighbourhood peers allows us to eliminate any common effects of the neighbourhood that they share, both on outcomes and on selection. The second comparison group allows us to control for any effects on outcomes that are due to formal schooling such as the classroom environment and teacher; similarly, if selection into Akanksha by parents was driven by similar (unobservable or observable) factors as school choice, then they are controlled for by using this comparison group. An important difference between the two comparison groups is that while the community peers were unacquainted with the treated, the school peers were identified by the treated themselves. Comparison of Akanksha treated to the classmates may therefore be contaminated to the extent that Akanksha treated identify systematically different classmates from themselves, or that peer effects matter in the transmission of non-cognitive skills¹¹. As with school-specific fixed effects, if these school-peer related effects do not matter, we would expect the estimated effect in (6) to be zero. Clearly, a non-zero effect in (6) would be difficult to interpret as it could be attributable to school-specific differences between the community peers and the other two groups, peers effects,

¹⁰There were approximately 10 secondary schools within a 2 km radius in any neighbourhood and the treated group of 58 attended about 7 different schools. The school environment is therefore different across the sample, but there is little to suggest that schools attended by the peers from the community differ greatly in quality.

¹¹The first possibility is that Akanksha alumni sort with classmates who differ substantially from them in non-cognitive skills. Suppose first that they select classmates with lower skills than them but are unaffected by them. We would then obtain a positive treatment effect of Akanksha which might, in fact, reflect this selection rather than the effect of the treatment. The second possibility is that they sort with stronger classmates - this would suggest that we have a negative (or insignificant) treatment effect. Finally, the peer effects that spillover to both groups (Akanksha and classmates) would bias the estimates of the treatment downwards towards zero.

or selection into the classmate comparison group¹². We return to this in the discussion of the results (Section 6).

5 Data

The survey was conducted in 2007, with the aim of interviewing all individuals who had completed Akanksha in that or the previous year. This is the first group to complete Akanksha since its inception in the mid 1990's. In total it consists of 60 young people and we were able to interview 58 of them¹³. We also interviewed 50 peers from the four communities in which the treated live, who are of the same age and sex as the treated and, like the treated, had completed secondary school. These peers were identified by our key informants and, importantly, were unacquainted with the treated. The second group consists of 46 young people who had been in the same class as the treated for at least the last two years of school; the individuals in this group were identified by the treated themselves. The relevant school catchment area was the same slum neighbourhood, so these school peers also come from the same neighbourhood. The treated were asked to name up to two students from their class in the past two years whom they could recall clearly and whom they were happy for us to interview. These are more likely to have to have been prominent members of the class, rather than close friends¹⁴. Hence, it is perhaps unsurprising that the pattern of observables suggests that these classmates were better off (Tables 2 and 3). However, this pattern is unlikely to also hold for the general set of classmates as there is no reason to believe that the differences result from the choice of systematically better schools by Akanksha parents compared to parents of the community group, given the specific context of this study. All participants (treated and both comparison groups) live in the same slums and the schools they attend (there are a number of state or municipal schools) vary not in quality but mainly in terms of shifts and timings to accommodate the population they serve.

We interviewed the young people as well as their primary carers (usually mothers). The interviews were formally structured and conducted in Hindi (and occasionally in

¹² It is possible that there are systematic differences in the quality of schools attended by the neighbourhood peers and Akanksha. This becomes an issue only in the presence of selection/peer effects since in their absence, the comparison of (2) and (3) should yield an unbiased estimate of the treatment effect as well. The combination of estimates from (4), (5) and (6) and what we observe about the groups, however, should allow us to better assess the likelihood that these biases are present. See discussion in Section 6.1.

¹³We did not interview two alumni because they were unable to keep their appointments with the survey team.

¹⁴Studies of social relations between adolescents in US schools find strikingly low reciprocity in reporting of friendship within schools: for instance, Moody (1999)[39] finds, using the Add Health data, that in 55% of cases 17-18 year olds do not reciprocate when asked to name their school friends. Other work using the Add Health data-set further suggests that prominent members of the class are more likely to be named (see Stauss and Pollack (2003)[52]).

Marathi). The main aim of the interview with the young people was to measure the outcomes of interest including self-esteem and self-efficacy, as well as life evaluation and aspirations.

We follow the established way of measuring self-esteem and agency, through aggregation of responses to statements relating to a range of beliefs that reflect these skills. The measures are variations of the well-established Rosenberg Self-esteem Scale (Rosenberg (1965) [47]) and Rotter’s Locus of Control Questionnaire (1966) [45]); both have been validated in a number of studies¹⁵ to ensure that the selection of questions maps into and measures the concept that we seek to capture. The statements refer to one’s beliefs about being able to affect outcomes, cope with stress, and attain goals as well as pride in oneself, one’s achievements and background (Schwarzer and Fuchs [53]; Lambe [37]); the responses indicate the degree of agreement with the statements. The specific selection of statements used in this study have been used in other studies for measuring self-esteem and agency in adolescents in the context of India¹⁶. Table 1 shows the statements and corresponding raw average scores used to construct each of the outcome measures¹⁷. The outcome measures used in the analysis were constructed by taking a standardised average of the degree of agreement with the statements. The final indicators of esteem and self-efficacy, therefore, reflect the standardised deviation of the individual average score of all esteem/efficacy statements from the sample average.

Expected life evaluation is measured using Cantril’s ladder¹⁸. In the survey young people were asked to place themselves on the “ladder of life” ten years from now. The ladder has nine steps, where the first refers to the worst possible life for the respondent, while the ninth to the best possible life; we use the step that they place themselves on as an indicator of (future) life evaluation. The survey further includes questions about the young people’s role models which were used to construct a measure of aspirations. We asked all respondents to name up to three individuals who they consider successful, who they admire and who they know personally. Once the three role models were identified, we asked a number of questions relating to their education, wealth, and personality traits. The outcome measure used in the analysis is the number of role models named by the individual, who are wealthier than the individual and whose success the respondent thinks

¹⁵See Robins et al., (2001) [44], Bagozzi (1993) [3], Sherer (1983) [49], Tipton (1984) [56]

¹⁶The selection was based on the question used in the Young Lives Longitudinal Study across four countries including southern India. (see <http://www.younglives.org.uk/what-we-do/research-methods/household-and-child-survey>). This is a large scale study of child development, led by a team at the University of Oxford.

¹⁷The score indicates the extent to which the respondent agrees with each statement. For negative statements the higher the score the more strongly the respondent disagrees with the statement. In other words, the individual scores indicate the extent to which the respondent agrees with the “positive sentiment” of the statement.

¹⁸Life evaluation is measured using Cantril’s Self-Anchoring Scale, which has the respondent rate his or her current life on a ladder scale in which 0 is “the worst possible life for you” and 10 is “the best possible life for you.” This is the measure used in the Gallup World Poll for instance.

can be emulated within 10 years. This notion of aspirations is closely linked to that of an “aspirations window” proposed by Ray (2006) [43]¹⁹.

We also collected information on the attitudes of the young people (e.g. respondents played games designed to measure their behaviour towards risk²⁰, time preference, propensity to cooperate²¹) as well as their educational attainment (including current school level and 10th standard exam results), current employment status and earnings. We also administered the Peabody Picture Vocabulary Test (PPVT) to measure cognitive skills²².

We interviewed the main carers to collect recall data on the pre-treatment socioeconomic conditions. To this end they were asked about the environment in which the children were living a decade ago, including assets, facilities and quality of the dwelling, parental education and occupation, and extensive data on carer life evaluation, aspirations, self-esteem and self-efficacy. In addition we used the same games as with the young people to measure parental risk and time preferences. An issue of concern might be whether recall data truly captures the environment in which the children were raised or simply reflects current conditions. This is mainly a concern for the recall data on pre-treatment socioeconomic conditions, rather than parental non-cognitive skills. While, as noted in the Introduction, non-cognitive skills and preferences are considered stable in adulthood, socioeconomic conditions are likely to have changed over the treatment period. Therefore, even if our recall measure of parental non-cognitive skills reflects current conditions, evidence in the literature would suggest that for adults this would still be valid as a measure of skills at an earlier point in adulthood. For instance, in their study of stability of locus of control (closely related to our measure of self-efficacy) using an Australian longitudinal data-set, Cobb-Clark and Schurer (2013)[13] find that "...short- and medium-run changes in locus of control are rather modest on average, are concentrated among the young or very old, do not appear to be related to the demographic, labor market, and health events that individual experience, and are unlikely to be economically meaningful" While their analysis is most recent and robust, it only spans four years. However, these findings are consistent with earlier studies which span longer periods of time, such as Doherty and Baldwin (1985)[23], who find substantial stability over a ten

¹⁹Ray suggests that "the window is formed from an individual's cognitive world, her zone of "similar", "attainable" individuals"

²⁰This was based on similar attempts in ICRISAT villages by Binswanger (1981)[8] and Giné et al.,(2008)[26]. Subjects were asked to choose between lotteries, where the riskier alternatives were a mean-preserving spread of the less risky ones - they had the same expected value, but a higher variance of payoffs. These games were played for real money and were not hypothetical.

²¹Time preference was measured using a hypothetical question where respondents were asked for the minimum amount they would be willing to accept as payment today in return for not waiting for a month for a sum of Rs.100 won on a lottery.The propensity to cooperate was measured using responses to a standard public good game (played individually against the larger virtual group for real stakes). Details are available upon request.

²²The PPVT has been used in India and is a test of receptive vocabulary, administered individually. It does not test schooling in a particular language or curriculum.

year period and lack of responsiveness to changing demographic factors. In this study we are, therefore, more concerned with the validity of the recall measures of pre-treatment socioeconomic conditions than of the parental non-cognitive skill measures. To check the robustness of recall responses about pre-treatment socioeconomic conditions, we examine the difference in responses to questions that were asked to the same respondent with reference to both the past and the present, as well as the difference in responses to the same recall questions by parent and child. Overall our findings suggest that the information contained in the recall measures differs from that in the contemporaneous measures of the same dimension, and that these recall measures are likely to reflect circumstances 10 years ago. Details of these results are presented in the next section. Finally, while we cannot use these methods to check the validity of the parental non-cognitive and preference measures as controls for the young person’s pre-treatment environment, we test the robustness of the main estimates to omission of these from the matching function. These results are presented in Section 6.3.2.

In all, we conducted 300 interviews as part of this survey; 154 with the young adults and the remainder with their carers.

5.1 Sample Descriptives

Table 2 provides summary statistics for the sample showing data on basic individual characteristics, as well as pre-treatment parental and socioeconomic characteristics; Table 3 presents summary statistics for the main outcomes. The first column of Table 2 shows statistics for the pooled sample, while Columns (2) – (4) show the disaggregated statistics for the treatment and each of the comparison groups. Columns (5) and (6) show tests of differences in means.

At the time of enrollment most young people were living with mothers who had less than primary education and were not employed. On average, the fathers have higher levels of education than the mothers and the majority worked as manual labourers. The primary carers are risk-averse and impatient with a discount rate of 52% over a month’s horizon²³. The young people grew up in very modest households; about half had electricity, a third had running water, and they owned less than a fifth of the basic assets²⁴. The average age is 19 and there are (slightly) more boys than girls.

The treated are of a similar age to the community peers (by construction) and slightly older than the classmates, by about 6 months. They are as wealthy as their community peers, but are worse off than the classmates. For instance, in the pre-treatment period the classmates were more likely to live in households with running water and electricity

²³This might seem high - but recent evidence across countries using a similar question suggests that this is in the median range. (Wang et al (2010) [57])

²⁴The complete list of assets includes: gas stove, radio, television, video/dvd player, cable tv, landline phone, refrigerator, bicycle, motorbike/scooter.

and more of the basic assets. However, the carers of the three groups do not differ by attitudes towards risk and time

In short, in the pre-treatment period, the treated were materially worse off relative to the classmates. In contrast, the community peers are similar to the treated in the observed characteristics. As discussed earlier, there might be concerns about spillovers and reflection effects between Akanksha treated and their classmates. The descriptive statistics presented here suggest that the treated identified classmates who are better-off, which, if anything, would bias our results against finding a significant treatment effect relative to the classmates: as the evidence in the literature review showed, non-cognitive skills tend to be positively correlated with socio-economic background.

We now turn to discussion of the patterns in the main outcomes. Table 3 presents summary statistics for the outcomes²⁵. There are no significant differences between the treated and comparison groups in these outcomes. In contrast, there are differences between the carers of the treated and those of their peers. Pre-treatment self-esteem (based on recall)²⁶ in this group was almost two thirds of a standard deviation lower than that of carers of the classmates and community peers. A similar pattern holds with respect to self-efficacy, though the differences are smaller. We note that in addition to being worse off materially, having lower non-cognitive skills limits the ability of the carers of the treated to transmit non-cognitive skills, mitigating against finding an impact on the treated.

The treated also have similar future life evaluation to their peers. In contrast, their carers had far lower assessments of life a decade ago than those in the two comparison groups. The pattern in aspirations is somewhat different. While there are no significant differences across the groups of parents, the treated named role models who were relatively wealthier and living outside the community.

As discussed in the previous section, reliance on recall questions to capture the environment in which the child was raised may be a concern, especially in relation to recall of pre-treatment socio-economic conditions. To investigate the robustness of these questions we first compare responses from the carer and young person to the same recall questions. If the recall questions are informative then we would expect the correlation between responses of the carers and young people to be relatively high. The second half of Table 4 presents evidence that this is the case. The correlations between responses of carers and young people about assets and access to services 10 years ago are around 60 percent. We then examine the correlation between responses to questions asked with reference to the past and the present to the same respondent. We would expect the correlation between

²⁵The scores presented in Table 3 show the standardised deviation from the mean for each group, and the standardised mean score differenced across the two groups. The use of standardised measures allows for more intuitive interpretations of the magnitude of the treatment effects.

²⁶As discussed in the previous section since self-esteem and self-efficacy are stable in adulthood, a measure based on recall should not be very different from that measured contemporaneously.

these responses to be relatively low, if they truly distinguish different time-periods. The top half of Table 4 shows that this is the case. For instance, both self-reported wealth and that relative to others in the area ²⁷ has increased from ten years ago, and the correlation between responses about the present and past does not exceed 20 percent. There is also a similarly low correlation between responses to the life evaluation ladder question that carers were asked with reference to ten years ago and the present. Note that while these two checks do not provide evidence against recall or nostalgia bias, the findings are consistent with these measures being stable and containing information that is distinctive from non-recall measures of the same domain.

5.2 Estimation

We present three sets of estimates of the treatment effects in equations (4) (5) and (6). The first is a simple OLS estimate followed by two sets of matching estimates. These are estimated using nearest neighbour Mahalanobis covariate matching, using Abadie et al.’s (2001) [1] bias adjustment²⁸. In both cases we use heteroscedasticity robust estimators of standard errors. We use nearest neighbour matching because in the context of small samples it is the most conservative: it yields the lowest bias in the point estimate at the cost of the highest standard errors. Further, as discussed in Caliendo et al., (2008) [9], if the selection of close control matches is sparse, which is likely with small samples, using fewer matches improves the quality of the matches, at the cost of higher variance. In the context of this study, one-to-one covariate matching, therefore, reduces the likelihood of falsely identifying a significant treatment effect. The estimates are robust to alternative matching estimators including propensity score and multiple neighbour covariate matching (discussed in Section 6.3.2). The OLS estimates are based on the same set of controls as used in the matching functions including individual characteristics (pre-treatment), household composition, parental characteristics (risk attitudes, time preference), endowments (index of assets, ownership of house 10 years ago) as well as parental non-cognitive skills.

The first set of matching estimates offers a conservative estimate of the treatment effect controlling for a full range of covariates, including parental non-cognitive skills, as well as socioeconomic background and own attributes of the children and parents as described in Table 2 ²⁹. The second set of estimates addresses potential concerns about family

²⁷Self reported wealth is the answer to: Which of the following best describes the household you are living in raked from 1 to 6 where: 1=very rich and 6=destitute. Self-reported wealth relative to others in the area is the answer to the question: Compared to other households here, how would you describe your household at the moment ranked from 1 to 7 where 1=the richest 7=the poorest.

²⁸This is a combination of matching algorithm and weighting matrix which has been shown to perform best in small samples. This is supported by Monte Carlo simulations conducted by Zhao (2004) [58].

²⁹In addition to the key variables included in Table 2, we also include household composition and neighbourhood controls.

level unobservables that may affect both selection into the programme and outcomes. Despite our best efforts to measure parental background, own attributes and parental non-cognitive skills and norms, potential unobservables remain a concern. Hence, we also offer a difference-in-difference set of estimates where the outcomes are expressed as the difference between the child and parental (recall) measures of non-cognitive skills (rather than using these as covariates). The estimated treatment effects in these specifications reflect the difference between treatment and comparison groups, differenced between child and parent. This serves to remove biases due to unobservable family-specific effects that matter either in terms of parenting skills or the specific environment at home; this approach can also take care of family-specific unobservables related to enrollment and attendance at Akanksha³⁰. These estimates are of course only valid to the extent that the outcomes of interest are not affected by mean-reversion, and are hence offered as a comparison. It should be noted that the literature emphasises the persistence of low skills across generations and discounts the possibility of mean reversion (see Feinstein (2000) [25] Sherman (1984) [50]).

The main results also include two falsification tests to determine whether the effects that we find on non-cognitive skills are attributable specifically to the work that Akanksha does to raise these outcomes. We explore two alternative possibilities. The first is that being in Akanksha affects outcomes through simply providing children with more adult attention and time for socialising with peers. To test this we examine the effect of participation in sports clubs and youth groups, which also has these features³¹. Secondly, we explore the possibility that participation in Akanksha affects outcomes through providing children with proficiency in English, which may in turn raise their self-esteem, sense of agency, and aspirations in the context of Bombay where it is a key skill. Therefore, we also test whether attending an English medium school affects the outcomes of interest in a similar way to Akanksha. In both tests we exclude the treated from the sample and match those who receive the "alternative treatment" (youth clubs or English medium school) to those who do not in the remaining group.

Finally, the treatment effects are estimated relative to the two comparison groups, as well as relative to the two groups combined. Sample descriptives, discussed in the previous section suggest that the community comparison group is perhaps more promising than the classmate group since, at least on observables, the treated are more similar to the former. However, estimates of the treatment effect relative to the classmate group as well as of the differences between the two comparison groups offer reassuring evidence that our results are unlikely to be driven by unobservable school effects. The advantage

³⁰Effectively, in using the first-differenced specification we are assuming that common genetic endowments are a family-specific fixed factor, distinct from parental non-cognitive skills, and entering additively.

³¹This test may also address some issues of selection into Akanksha to the extent that selection into youth clubs and sports groups is on similar characteristics as that into Akanksha.

of the estimates relative to the pooled comparison group is better matching quality and, therefore, lower likelihood of matching bias. We also match classmates to the community peers and estimate a community “treatment effect”, in order to investigate whether there are systematic differences between comparison groups³².

6 Results

6.1 Self-esteem and self-efficacy

Table 5 shows the three sets of estimates of the impact of Akanksha on self-esteem and self-efficacy. Row (1) show the OLS estimates, followed by the main results in Row (2) and the difference-in-difference results in Row (3).

As with the raw differences between the treated and comparison groups in Table 3, the OLS results do not show significant differences even after controlling for a full set of pre-treatment characteristics including parental background and parental non-cognitive skills. However, once we match the groups on pre-treatment characteristics, we find strong evidence that being in Akanksha raises both self-esteem and efficacy (see Section 6.3.2 for further discussion of the differences between OLS and matched estimates). Akanksha raises self-esteem by half of a standard deviation relative to the community peers and 0.7 of a standard deviation relative to the classmates. The effect on self-efficacy is between 0.5 and 0.9 of a standard deviation relative to the classmates and community peers respectively. Importantly, in both cases, there is no statistically significant difference in outcomes between the two comparison groups suggesting that group-specific contaminants such as classmate reflection effects and community school effects are unlikely to matter³³. This is equivalent to the estimate from Equation (6) being zero, which is consistent with there being no difference between the estimates in (4) and (5). Hence, we pool the comparison groups in order to reduce the matching bias in the estimates. The treatment effect relative to the pooled groups for both self-esteem and self-efficacy is one standard deviation.

Could the strong effect that we find be misleading? The only plausible alternative explanation of the observed pattern is that the treated identify systematically "weaker" classmates and simultaneously, community peers attend worse schools that lower their

³²The matching functions relative to the two main comparison groups and the pooled group satisfy the balancing property. Consistent with the descriptive statistics discussed above the quality of matching is somewhat better relative to the community than the classmate comparison group. Section 6.3.2 offers further discussion.

³³It also suggests that unobservable selection effects, to the extent that they are correlated with school choice, are unlikely to affect our findings. For example, if particular parents choose to enrol children in Akanksha because they valued their child’s later opportunities more, they are also likely to have been more careful in choosing schools. However, as the impacts controlling (and not) for school choice do not differ, this suggests that this type of selection effect is not a concern.

skills to the level of the "weak" classmates. This would result in the overestimation of the Akanksha effect relative to both groups, while showing no difference between these groups. However, the descriptive statistics show convincingly that, if anything, Akanksha identify "stronger" classmates, from better-off backgrounds and with parents with higher non-cognitive skills. Furthermore, there is no reason to believe that there are systematic differences in the quality across schools attended in these communities (see Section 5). Hence, we conclude that the pattern of results is much more likely to be indicative of a positive impact of Akanksha, rather than biases in the comparison groups.

Further, these results do not appear to be driven by parent/family level unobservables as the impact persists in the difference-in-difference estimates (row (3)) for both self-esteem and self-efficacy. The two sets of pooled estimates are similar in magnitude suggesting that reversion to the mean is unlikely to be relevant³⁴.

We now turn to the falsification tests (Columns (5) and (6)) which support the hypothesis that the impacts we find are attributable specifically to the work Akanksha does to build self-esteem and self-efficacy (note that we omit the treated when making this comparison). Neither participating in youth clubs, nor attending an English medium school has effects on these skills that are comparable to the impact of Akanksha³⁵. The share of those attending youth clubs is 28% for classmates and higher, at 38% for community peers. More classmates attend English medium schools (similar to the treated by construction) at 30% while only 14% on average of the community peers do so.

6.2 Expected Life Evaluation and Aspirations

As explained earlier, life evaluation and aspirations are best regarded as measures of future well-being and ambition, and as such are distinct from measures of the non-cognitive skills, self-efficacy and esteem³⁶.

The raw differences suggested no difference in expected life evaluation between the treated and comparison groups, but higher aspirations among the treated (Table 3). Matching on covariates we find that the higher aspirations persist and life evaluation is now also significantly higher (Table 6). Since there is no significant difference between the two comparison groups, we focus on the treatment effect estimates relative to the pooled comparison group (Column(4)). The effect on expected life evaluation is three-quarters of a step, equivalent to just under half of a standard deviation increase. Similarly, Akanksha has a positive effect on aspirations; more of the attainable role models named

³⁴To the extent that outcomes are affected by reversion to the mean, the estimates in the main specification (matching on parental self-esteem and self-efficacy) will be smaller than the difference-in-difference specification. These differences are negligible in the pooled specification where estimates converge.

³⁵Oddly, attending an English medium school has a significant negative effect on self-efficacy in the difference-in-difference specification, this does not hold in the main specification.

³⁶This implies that discussions about mean reversion are not relevant here.

by the treated are both wealthier and "attainable" (i.e. respondent thinks they can be emulated within 10 years). The size of the effect is equivalent to two-fifths of a standard deviation. As mentioned previously, this measure of aspirations reflects the notion of "aspirations window", proposed by Ray (2006) [43].

There are more grounds for caution in interpreting these results than those for the previous outcome measures. Firstly, the effect relative to the community comparison group is only marginally significant (at 11 percent) in the main specifications, though the significance and magnitude of this effect increase substantially with a differenced measure of the outcome. Secondly, as with life evaluation, there is fluctuation in the size of the treatment effects relative to the pooled comparison group, ranging between 0.4 -1.0 standard deviation.

As before, there is no evidence to suggest that the effect on life evaluation works through channels other than the content of Akanksha's programme; being in a youth club and attending an English medium school do not have a significant effect on this outcome. However, attending an English medium school has a positive effect on aspirations, suggesting that English proficiency can have an important effect on who one knows and looks up to.

6.3 Other robustness checks

6.3.1 Omitted variables

Throughout this paper, we ensure robustness of our analysis and findings in various ways. Our empirical and estimation strategies aim to eliminate a number of potential sources of contamination of the estimated effects. We selected two comparison groups that are similar along two key unobservable dimensions – neighbourhood and school environments. We use a rich set of covariates to match treatment and comparison groups including parental non-cognitive skills, and offer alternative estimates that control for some parental and household level unobservables through differencing. We verify that the effects we find are attributable to the work that Akanksha does with the participants rather than some other features of the programme by testing the effects of alternative treatments.

We now allow for the possibility that despite the careful treatment of possible sources of bias, there are, nevertheless, some omitted unobservable variables which violate the conditional independence assumption (CIA - our *Assumption 1*). We do this by conducting sensitivity analysis as in Ichino et al., (2008) [34]. This is part of the Rosenbaum and Rubin (1983) [46] family of tests, which allow us to examine the sensitivity of the estimates to "likely" omitted variables that affect both selection and outcome. These tests introduce a confounding variable into the matching set and test the sensitivity of the results to this confounder. We examine a range of possible confounders but our strongest test is obtained by examining the potential effect of having omitted unmeasured cognitive

ability - perhaps the treated simply had higher cognitive ability? As explained further below, we use a generated variable with a distribution similar to that of the PPVT scores as the measure of this omitted variable.

Table 7 presents the estimates of the treatment effect obtained by adding a selection of confounders to the matching set. The first four columns of each sub-table (1a-4a and 1b-4b) show the proportions of observations for which the binary variable takes the value of one in each of the four groups, denoted by p_{ij} , where i is a treatment indicator and j is an indicator of whether the outcome is above the mean. The effect of the confounder on the outcome (the outcome effect Λ) is estimated using a logistic regression (Columns 5a and 5b). The table presents the odds ratio of the estimated effect of the confounder on the probability of being treated (the selection effect Γ , in Columns 6a and 6b). The key results are in Columns 7a and 7b, which show the estimated treatment effect with the confounder.

The first row of the table shows the estimated treatment effects using propensity score radius matching with no confounder. The remaining rows introduce a range of confounders with different distributions; in all cases except the PPVT score (last row of the table) the parameters of the confounders are set to follow the distributions of covariates included in the main matching function.

Overall, the estimated treatment effects of Akanksha on self-esteem and self-efficacy are very robust to possible violations of the CIA. We introduce a range of confounders with various distributions and find that the variation in the size of the treatment effects does not exceed 0.04 of a standard deviation, irrespective of the direction and magnitude of the selection and outcome effects of these confounders. The statistical significance of the estimates also remains constant.

The last row of the table introduces a confounder which is not, in fact, in the matching set - the results of the PPVT test (a measure of cognitive achievement). This variable is not in the matching set as the test was administered during the survey (post-treatment) and is unlikely to constitute a valid control for pre-treatment differences between the groups, not least as the Akanksha treatment may well have had an impact on PPVT as well. However, to the extent that the PPVT score, at least in part, proxies inherent cognitive ability, introducing a confounder with a similar distribution is a way of testing the sensitivity of our findings to omitted controls for unobservable cognitive ability. We use the raw PPVT scores to construct a binary variable indicating whether an individual scored above the mean. In effect, we have an artificially-generated binary variable, based on the distribution of PPVT, to test whether there is any information in cognitive ability (even if affected by the programme, it should also contain information about pre-programme ability) that could potentially bias our estimates. The results suggest that the estimated treatment effects of Akanksha on self-esteem and self-efficacy hold even in the presence of omitted controls for ability. This is particularly encouraging in the

context of a non-randomised evaluation, as selection into treatment on ability is a valid concern that cannot be fully controlled for. In addition, the distribution of the binary PPVT indicator is such that it has a positive impact on both selection into treatment and the outcomes. As discussed by Ichino et al., (2008) [34], omitted variables that follow this distribution are of particular concern since they may bias the results towards finding a falsely significant effect. Reassuringly, we find that in this case omission of such a variable would have no inflationary effect on the estimates of treatment effects on both self-esteem and self-efficacy³⁷.

6.3.2 Sensitivity of Estimates

Our choice of matching estimator, set of covariates included in the matching function and variance estimator are all motivated by "best practice" suggestions in the literature. As noted in Section 5.2 nearest neighbour covariate matching achieves the most conservative estimates in the context of small samples, trading off lower bias for higher variance. We use the heteroscedasticity robust variance estimator derived by Abadie and Imbens which, as they show, yields reliable standard errors even with a small sample (Abadie and Imbens, 2008 [2])³⁸. Finally, we ensure that the matching functions satisfy the balancing property as a way of ensuring that the included covariates are relevant, even though this is not a requirement for covariate matching. The balancing graphs³⁹, presented in Figure 2 show that while there is variation in how similar the groups are depending on the comparison group, in all cases, the majority of score intervals have non-zero densities within non-treated distributions. In line with the descriptive statistics discussed in Section 5.1, Akanksha alumni are more similar to the community peers than the school matches. The pseudo-R² declines after the matching for all of the models, and the likelihood-ratio test of the joint insignificance of the covariates before the matching is rejected in all of the models and cannot be rejected after the matching in all of the specifications (Table 8).

Before concluding, we conduct some additional checks for the sensitivity of the estimates to common support as well as the matching estimator and function used.

Table 9 shows estimates of the treatment effect applying various trimming methods to exclude observations with few/no close matches in the comparison group: trimming 5% of observations at the extremes of the distribution (Dehejia and Wahba, 1999,[21]), trimming

³⁷We also examined the sensitivity of the estimated treatment effects of Akanksha on our measures of life evaluation and aspirations. As before, the magnitude of the estimates remains stable. These results are available on request.

³⁸Their starting point for this work is that classical bootstrapping produces biased variance estimates for nearest neighbour matching. A key feature of their variance estimator is that it uses the closest match within the set of units with the same treatment indicator (Abadie and Imbens, 2008 [2]).

³⁹While conceptually common support is a relevant issue in the context of nearest neighbour covariate matching, it can be directly measured only in propensity score matching through comparison of the distributions of the estimated propensity scores for the treatment and control groups. Similarly, balancing tests are relevant in propensity score matching to check whether the propensity score matching procedure balances the distributions of the relevant variables (Caliendo and Kopeinig, 2008,[9]).

observations in any interval for which the density within the non-treated distribution is below a 5 and 10% thresholds (Smith and Todd, 2005,[51]), and calculating Lechner non-parametric bounds (see Caliendo and Kopeinig, 2008,[9] for a discussion of these methods). Overall, this sensitivity analysis suggests that any bias in the estimates driven by lack of close matches does not alter the main findings. The majority of the estimates, including those of the lower bound, are similar in significance and magnitude to the main estimates. For instance, focusing on our strongest effects, the magnitude of the Lechner lower bound estimate of the Akanksha impact relative to the pooled comparison group on self-esteem is about 17% lower than the main estimate, and 5% lower than the main estimate for the impact of self-efficacy; both remain significant at the 1% level (Column (3), Table 9).

Tables 10 and 11 examine the sensitivity of estimates to the matching estimator used. Rows (1), (2), (6) and (7) show the main OLS and nearest neighbour matching estimates, while the rest of the rows show estimates using alternative estimators. We first focus on the two alternative matching estimators - propensity score kernel matching and multiple nearest neighbour matching. In small samples, especially if they are relatively heterogeneous, these estimators may yield more biased estimates than the single nearest neighbour matching as there is a greater risk of worse matches being used in the estimation. Overall, neither set of alternative estimates greatly alters the main conclusions. In the majority of cases (8 out of 12) the propensity score estimates, like the main nearest neighbour estimates, are positive, significant and do not differ in magnitude from the main estimate by more than about a quarter of a standard deviation⁴⁰; further they tend to be lower than the nearest neighbour estimates (discussed further below) The most similar pattern is observed in the propensity score and nearest neighbour estimates using the community peer comparison group. Like the nearest neighbour estimates, the kernel propensity score matching estimates are positive and significant for all of the four outcomes and, if anything, tend to be greater than the nearest neighbour estimates. Since the community matches are the most similar comparison group (on observables), it is not surprising that estimates of the treatment effect relative to this group are somewhat less sensitive to the matching estimator used. The estimates using three nearest neighbours are even more similar to the main estimates. Only one out of the twelve estimates is not statistically significant, the majority are within 0.1 of a standard deviation of the main estimate, and all are either equal to or slightly lower than the main estimate.

We now turn to the third alternative set of estimates in Tables 10 and 11, using the fully interacted linear model (FILM), presented in rows (3) and (8). Here we investigate the noticeable differences in the magnitudes of the OLS and matching estimates of the

⁴⁰In fact, none of the propensity score estimates differ from the main estimates by more than a quarter of a standard deviation, but in 4 out of 12 cases, the propensity score estimate is not statistically significant.

treatment effect (on all outcomes except aspirations window) in Tables 5 and 6 . The robustness of the estimates to the matching estimator used suggests that this difference is unlikely to just reflect some quirk of the matching estimator used. Theoretically, the main differences between the OLS and matched estimators are that the latter get closer to comparing "like with like" (common support) and do not impose a functional form. There is some evidence that common support may in part drive the difference. Although, as discussed above trimming does not change the estimates much ⁴¹, the balancing graphs (Figure 2) do show evidence of heterogeneity in treatment and control groups and estimates from alternative matching estimators, which utilise more matches from the control group and, therefore, are likely to be more affected by common support issues, tend to be closer to the OLS estimates than the main estimates. In all, however, the evidence on the importance of common support is not strong enough to suggest that it is the main driver of the differences.

What clearly appears to matter, however, is the differences in functional form flexibility between the OLS and matched estimates. The fully interacted linear model offers an intermediate step between matched and OLS estimators in that the treatment dummy is interacted with each variable in the set of covariates, which increases the flexibility of this model relative to OLS. We would expect that if the difference between the matched and the OLS estimates in our analysis is at least partly due to a lack of functional form restrictions in the former, the FILM estimates should be moving closer to the matched estimates. This is what we find. Tables 10 and 11 show that out of the nine cases where the OLS estimates are substantially lower than the nearest neighbour estimates⁴² , seven follow the expected pattern with FILM estimates (rows (3) and (8) in Tables 10 and 11) that are higher and more precisely estimated⁴³ than the OLS estimates but still lower than the nearest neighbour matched estimates.

Before concluding the sensitivity analysis, we examine the robustness of the main findings to the covariates included in the matching function ⁴⁴. These include recall measures of the pre-treatment conditions (asset index, parental education and employment status, gender and non-cognitive skills of the primary carer) and fixed individual characteristics (such as age, gender,). We discuss the validity of the recall measures in Section 5, where we show evidence consistent with the validity of the measures of pre-treatment socio-economic conditions (Table 4), and discuss literature which suggests that validity

⁴¹ Additionally, the OLS estimates for the trimmed sample are very close in magnitude and precision to the OLS estimates for the whole sample (results available on request).

⁴² These are estimates of the treatment effect relative to the three comparison groups (classmates, community peers and pooled) for self-esteem, self-efficacy and expected life evaluation (Tables 5 and 6).

⁴³ The FILM estimates are significant in five out of the seven cases in which nearest neighbour estimates are statistically significant while the OLS ones are not.

⁴⁴ As noted above, all the specifications used in the main analysis satisfy the balancing property, which we use as a way of being systematic and rigorous about the covariates chosen, even though this approach does not directly validate the choice of matching function for covariate nearest neighbour matching.

of recall measures of parental non-cognitive skills should not be a big concern, as they are likely to be stable in adulthood. Nevertheless, in examining the robustness of our estimates to the choice of covariates, we start by testing robustness to the exclusion of these from the matching function. Results are presented in Table 12⁴⁵; the first row of results in this table shows the main estimates and is followed by estimates using the same matching method (nearest neighbour covariate matching) but excluding all measures of parental non-cognitive skills and preferences from the matching function. Most of the estimates obtained are similar in size and significance to the specification with the full set of covariates, with the exception of the estimated effect on self-esteem, which remains statistically significant but is about half of the magnitude of the main estimate.

The remainder of Table 10 shows estimates using two additional specifications - one with a minimal set of covariates⁴⁶ and one with an extended set of covariates⁴⁷. The scope for altering the matching function while maintaining joint balance of all covariates is limited with a small sample. We, therefore, use entropy balancing - an estimator which ensures that pre-specified balance conditions are satisfied by re-weighting treatment and control groups in the first stage to satisfy the conditions before estimating the treatment effect using the re-weighted sample in the second stage (Hainmueller and Xu, 2012 [31]). Unsurprisingly, given the small sample, the magnitude of the estimated treatment effects is sensitive to the covariates included in the matching function. However, the great majority of the alternative estimates (7 out of 8) remain positive and significant. Estimates for the extended model are within half of a standard deviation of the main estimates, while estimates using a more restricted set of covariates are up to 0.7 of a standard deviation lower than the main estimates. The noticeable reduction in the size of the estimated effects in the model with the minimal set of covariates is consistent with the disadvantage that Akanksha alumni experience relative to the control groups, especially with respect to parental non-cognitive skills, according to the descriptives (Tables 2 and 3). Exclusion of these from the matching function therefore results in an under-estimation of the treatment effect.

In sum, despite the small sample, the main conclusions of significant positive effects of the Akanksha intervention on non-cognitive skills seem robust to the matching estimator and matching function used. Sensitivity analysis further suggests that flexibility of the functional form assumptions in the matching estimators render these more appropriate than the OLS estimator.

⁴⁵We restrict our analysis to the pooled sample which gives us more leeway for adjusting the matching function while continuing to ensure, as in the main results, that the matching passes the balancing test.

⁴⁶This set includes the pre-treatment asset index and employment status of primary carer, as well as the young person's age and gender.

⁴⁷This set now includes additional controls for wealth indicators (such as access to water and electricity and dwelling ownership), whether the child lived with a single parent 10 years ago, and additional measures of less observable parental characteristics including attitudes to local service provision and trust in local community.

7 Non-cognitive skills and outcomes: Beyond cognitive skills

There is a vast literature in economics and other social sciences establishing the importance of cognitive skills for child development and future outcomes⁴⁸. A relevant question is whether non-cognitive skills matter *in addition* to these; below, we offer some descriptive evidence to suggest that this is so in our data. We have data on Standard 10 exam results and wages received by those who are employed. The correlations between these and non-cognitive skills are shown in Figures 2 and 3. It might be argued that in settings with low educational achievement, the measurement of cognitive skills is sufficient to explain variation in socioeconomic outcomes. We examine this below by comparing the relationship of non-cognitive skills to outcomes, both with and without the control for cognitive achievement measured by the PPVT test⁴⁹. We ask whether non-cognitive skills have any role in affecting outcomes, once we control for cognitive achievement. Arguably, if the variation in non-cognitive skills that is correlated with cognitive skills is simply noise, then the measurement of cognitive skills might be considered sufficient. We show below that this is not the case.

Figures 2a and 2b show the associations between examination results in Standard 10, self-esteem and self-efficacy. These are two-way lowess graphs with the examination results (going from Fail to First Class) on the Y-axis and each of the skills on the X-axis, obtained using partial linear regressions. Consistent with the existing literature there is a strong positive association between examination results and both self-esteem and self-efficacy. In the sample, children with higher self-esteem are more likely to have attained better exam results; the same holds with respect to self-efficacy. The point to note here is that these results hold even controlling for cognitive achievement. This supports the hypothesis that non-cognitive skills matter independently from cognitive achievement. These correlations also hold with respect to wages (Figures 3a and 3b), although only 44 percent of the sample were in work at the time of the survey.

Overall, the link between non-cognitive skills and adult outcomes widely reported in the economics and psychology literatures is supported by the associations in our data.

8 Conclusions

This study contributes to the literature on how non-cognitive skills are formed, and in particular whether initially poor non-cognitive skills can be remedied later in childhood. It investigates the impact of an NGO that offers informal education to children from

⁴⁸References include Murnane et al., (1995) [40] for the USA and Connolly et al., (1992) [14] for the UK. For a review of the literature for developing countries see Glewwe (2002) [27].

⁴⁹The simple correlation between psychosocial skills and the PPVT scores is about 0.18.

slums in Bombay. The NGO concentrates on raising non-cognitive skills and aspirations of children who join the programme at the start of primary school and stay both in school and in the programme until they complete secondary school. We use a cross-cutting design in order to obtain estimates of the impact of the programme.

We find a remarkably strong and robust effect of the intervention on both self-esteem and self-efficacy: being in the programme raises both by about one standard deviation. We also examine the impact on life evaluation and a measure of aspirations. The effects of the intervention on life evaluation is about half of a standard deviation, while that for aspirations is lower at 0.4. The size and significance of the estimated impacts are robust to alternative estimators and sets of covariates, especially the results on self-esteem and self-efficacy. Simulations show that the estimates are insensitive to the introduction of potential confounders. We also examine plausible alternative channels for the effects that we find, using enrolment in programmes such as youth clubs and English medium schools. These are found to have no effect.

This is a non-randomised evaluation of a long-term intervention and thus warrants a careful and thorough examination of robustness of estimates. Arguably, if skill accumulation could be meaningfully measured over the short horizon it would have been possible to evaluate the impact of this intervention using a short-term intervention, possibly in a randomised framework, at some point during childhood. However, the evidence from the psychology literature suggests strongly that these skills are unstable in early childhood and adolescence and are most stable (and hence measurable) between early adulthood and middle age (see Trzesniewski et al., (2003, 2009) [54] [55]) while, at the same time, they are generally formed throughout childhood. This presents particular challenges to designing any evaluation design, and our focus on a long-term intervention on measurable outcomes in early adulthood is at least sensitive to these concerns. We aim, therefore, to offer a robust and persuasive method of evaluating the long-term impact of this intervention addressing, in particular, concerns about selection into and out of the programme. While the programme did not target specific children with high or low potential, parental choices were key for enrolment. The combination of extensive controls for parental pre-treatment background and non-cognitive skills and the use of matching techniques control for observable confounders. The careful choice of comparison groups allow us to control for and distinguish between the role of unobservable neighbourhood and school confounders. The comparison between the different control groups also shows that selection into Akanksha based on unobservable criteria correlated with school choice is unlikely to have been a problem in the evaluation. The robustness analysis also demonstrates that any selection into the programme based on ability is unlikely to have had a substantial effect on the findings. While selection issues cannot be entirely ruled out, none of the findings suggest that the treated were more predisposed to higher non-cognitive skills ex-post without the programme.

The purpose of this paper was to ask whether interventions to raise non-cognitive skills can be effective. The evidence provided here suggests that they can be and, combined with what we already know about the importance of the broader set of skills for key socioeconomic outcomes, offers firm grounds for interventions targeting non-cognitive skills. It has always been clear that such skills matter - what is less certain is whether they are malleable over time. Our evidence suggests that there are reasons for optimism.

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Figure 1: Study Design

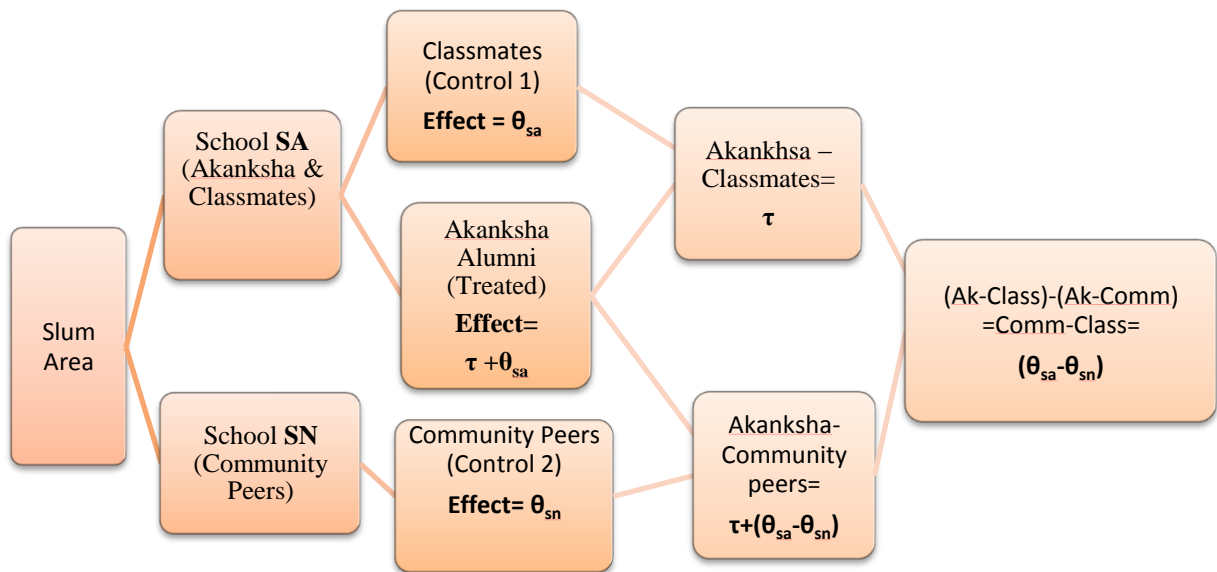


Table 1: Self-esteem and self-efficacy statements: raw scores

Characteristic	Statement	Mean score
Self-esteem (children)	I feel proud to show my friends or other visitors where I live	3.2 (0.97)
	I feel proud of the job the main breadwinner in my family did when I was at school	3.7 (0.62)
	The job I do makes me feel proud	3.8 (0.67)
	I am proud of my past achievements at school	3.6 (0.76)
	I am not comfortable with/feel shy around members of the opposite sex*	2.7 (1.2)
	My parents/guardians felt proud to show friends or other visitors where we lived	3.3 (0.92)
	My parents/guardians were ashamed of their clothes *	3.5 (0.79)
	My parents/guardians felt proud of the job they did	3.1 (0.96)
	My parents/guardians were embarrassed by/ashamed of the work they had to do, or by the fact that they had no job*	3.4 (0.88)
My parents/guardians were proud of my achievements at school	3.7 (0.70)	
Self-esteem (parents)	I felt proud to show my friends or other visitors where I live	2.9 (1.1)
	I felt proud of the job the main breadwinner in my family did	3.7 (0.69)
	I felt proud of my children/NAME	3.9 (0.41)
	The job I did made me feel proud	3.7 (0.72)
Self-Efficacy (children)	If I try hard, I can improve my situation in life	3.9 (0.34)
	It feels as if other people in my family make all the decisions about how I spend my time*	2.1 (0.90)
	I like to make plans for my future work or studies	3.6 (0.72)
	I have no choice about the work I do - I must work*	2.5 (1.2)
	Working hard will be rewarded by a better job in the future	4.0 (0.23)
	My parents/guardians believed that if one tries hard, one can improve ones situation in life	3.9 (0.25)
	My parents/guardians liked to make plans for the future	3.6 (0.64)
My parents/guardians believed that working hard would be rewarded by a better job in the future	3.9 (0.3)	
Self-Efficacy (parents)	I believed that If I tried hard, I could improve my situation in life	3.6 (0.69)
	I liked to make plans for my future work or that of my children	3.5 (0.67)
	I had no choice about the work I did - I must work*	1.7 (1.1)
	I believed that working hard would be rewarded by a better job in the future	3.8 (0.5)
	I had no choice about which school to send my NAME to*	1.6 (0.89)
	I could do little to help my child/children do well in school, no matter how hard I tried*	1.8 (1.0)

Note: * indicates negative statements. Mean score is the mean score on a scale from 1 to 4 (1- strongly disagree; 2- disagree; 3 – agree; 4 – strongly agree). The reversed score is presented for the negative statements (indicated by “*”). Standard errors in brackets.

Table 2: Basic individual, socio-economic and parental characteristics

	All (1)	Akanksha Treated (2)	Community Peers (3)	Classmates (4)	Akanksha- Community (5) Differences in means z-score	Akanksha – Classmates (6)
Individual characteristics						
Male (%)	0.58	0.58	0.58	0.59	-0.01	-0.08
Age in years	19.33 (1.74)	19.61 (1.69)	19.28 (1.93)	19.04 (1.56)	0.96	1.76*
Dwelling characteristics 10 yrs ago						
Dwelling owned (%)	0.76	0.74	0.74	0.83	-0.04	-1.08
Electricity in house (%)	0.54	0.46	0.46	0.72	-0.04	-2.66***
Water in house (%)	0.33	0.25	0.32	0.46	-0.85	-2.25***
Asset index ^a	0.19 (0.25)	0.12 (0.18)	0.12 (0.19)	0.35 (0.30)	0.09	-4.77***
Parental characteristics 10 yrs ago						
Primary carer is male (%)	0.21	0.11	0.20	0.35	-1.37	-3.00***
Mother's education: incomplete primary (%)	0.74	0.77	0.74	0.70	0.38	0.87
Mother not working (%)	0.74	0.65	0.76	0.83	-1.25	-2.01**
Father's education: incomplete primary (%)	0.42	0.46	0.54	0.26	0.87	2.04**
Father employment: manual labour (%)	0.42	0.42	0.56	0.28	-1.43	1.46
Carer attitudes						
Parental risk ^b	1.30 (1.59)	1.46 (1.73)	1.09 (1.49)	1.34 (1.53)	1.19	0.39
Parental discount rate ^c	0.72 (1.92)	0.48 (0.69)	0.54 (1.70)	0.56 (1.08)	1.06	0.05
Total Observations	154	58	50	46		

Note: Standard errors in brackets. No standard errors for binary variables reported.

^aAsset index is an average based on a list of 9 durable assets: gas stove, tv, cable, video/dvd, telephone, radio, bike, scooter, fridge.

^bParental risk is the coefficient of relative risk aversion. Obtained from risk game with 6 options (for real stakes) and risk aversion parameters computed with CRRA utilityfunction.

^cThe parental discount rate is $\rho = (100/\text{minimum accepted for payment}) - 1$ obtained from a hypothetical question.

*=significant at 10%; **=significant at 5%; ***=is significant at 1%

Table 3: Testing differences in key outcomes

	Akanksha Treated	Community Peers	Class- mates	Akanksha- Community t-stat	Akanksha- Classmates t-stat	Akanksha- Joint t-stat
	(1)	(2)	(3)	(4)	(5)	(6)
Self-esteem						
Standardised mean self-esteem score	0.11 (1.08)	-0.11 (1.01)	-0.01 (0.90)	1.06	0.59	1.01
Standardised mean parental self-esteem score	-0.37 (1.04)	0.28 (0.85)	0.28 (0.73)	-3.50***	-3.50***	-4.34***
Self-efficacy						
Standardised mean self-efficacy score	0.18 (1.06)	-0.15 (1.02)	-0.05 (0.89)	1.58	1.09	1.6
Standardised mean parental self-efficacy score	-0.16 (0.92)	-0.09 (0.92)	0.42 (0.97)	-0.38	-3.09***	-1.95**
Expected Life Evaluation						
Expected Life Evaluation (in 10 Years)	7.33 (1.90)	6.86 (1.82)	7.52 (1.50)	1.31	-0.55	0.53
Expected Life Evaluation (10 Years Ago) – Parental	2.61 (1.77)	2.56 (1.66)	3.96 (2.51)	0.16	-3.18***	-1.79*
Aspirations Window						
Aspirations Window	0.70 (0.91)	0.40 (0.81)	0.39 (0.74)	1.81*	1.87*	2.21**
Aspirations Window - parents	0.26 (0.55)	0.28 (0.57)	0.24 (0.52)	-0.15	0.22	0.03
Total Observations	58	50	46			

Notes: Standard errors in brackets.

Self-esteem and self-efficacy are mean standardised scores of items in Table 1.

Life evaluation is based on 9-step ladder, in which 1 represents the worst possible life and 9 the best possible life

Aspirations Window is the number of “attainable” role models named by respondent who are richer than them, where attainable means that the respondent believes he/she can emulate them within 10 years.

*=significant at 10%; **=significant at 5%; ***=is significant at 1%

Table 4: Exploring the validity of the recall questions

	10 years ago	Today	Correlation
Young Person Questionnaire			
Self-reported wealth	4.1 (0.92)	4.7 (0.58)	0.15
Self-reported wealth relative to others in the area	3.4 (0.96)	4.0 (0.65)	0.19
Carer Questionnaire			
Life Evaluation	3.0 (2.09)	5.1 (2.3)	0.22
Recall Question Responses by Parent and Child			
	Young Person	Carer	Correlation
Assets 10 years ago			
Asset index	0.18 (0.18)	0.13 (0.17)	0.61
Access to Services 10 years ago			
Electricity	0.64	0.54	0.59
Water in household compound	0.38	0.34	0.56

Note: Standard error in brackets, not reported for binary variables. Correlation coefficients are calculated between recall and current answer, and between young person's and carer's answer to same question.

Self reported wealth is the answer to: Which of the following best describes your household you are living in: 1=Very rich, 2=Rich, 3=Comfortable – can manage to get by, 4=Never have quite enough, struggle to get by, 5=Poor, 6=Destitute

Self-reported wealth relative to others in the area is the answer to the question: Compared to other households here, would you describe your household at the moment as: 1=The richest, 2=Among the richest, 3=Richer than most households, 4=About Average, 5=A little poorer than most households, 6=Among the poorest, 7=The poorest, 77=NK.

Life evaluation is the answers to the ladder of life question as before.

Asset index is an average based on a list of 9 durable assets: gas stove, tv, cable, video/dvd, telephone, radio, bike, scooter, fridge .

Table 5: Akanksha Treatment Effect on Self-esteem and Self-efficacy

		Akanksha – Community	Akanksha – Classmates	Community -Classmates	Akanksha - Pooled	Club- no club	English Med. School – Non
		(1)	(2)	(3)	(4)	(5)	(6)
		Akanksha treatment effect	Akanksha treatment effect	Community treatment effect	Akanksha treatment effect	Club treatment effect	English medium treatment effect
Self-Esteem levels							
OLS, robust se	(1)	0.32 (0.25)	0.38 (0.29)	-0.07 (0.21)	0.44* (0.20)	0.03 (0.22)	0.07 (0.20)
Nearest Neighbour Covariate Matching	(2)	0.52** (0.26)	0.70*** (0.28)	0.13 (0.26)	0.99*** (0.23)	-0.11 (0.24)	0.36 (0.28)
Self-Esteem Difference-in-Difference							
Nearest Neighbour Covariate Matching	(3)	1.11*** (0.29)	1.09*** (0.35)	0.17 (0.38)	0.97*** (0.27)	-0.28 (0.34)	-0.21 (0.38)
Self-Efficacy levels							
OLS, robust se	(1)	0.37 (0.26)	0.18 (0.28)	-0.11 (0.22)	0.41* (0.22)	0.06 (0.26)	-0.48** (0.22)
Nearest Neighbour Covariate Matching	(2)	0.90*** (0.28)	0.48** (0.25)	0.09 (0.22)	0.93*** (0.24)	0.01 (0.28)	-0.28 (0.28)
Self-Efficacy Difference-in-Difference							
Nearest Neighbour Covariate Matching	(3)	0.78*** (0.24)	0.67* (0.44)	-0.09 (0.32)	0.82*** (0.26)	0.07 (0.38)	-0.65* (0.41)
Total Observations		108	104	96	154	96	96

*Significant at 10%; **Significant at 5%; ***Significant at 1%; standard errors in brackets.

Self-esteem and self-efficacy are mean standardised scores of items in Table 1.

All estimators of standard errors are heteroscedasticity robust.

All specifications in this table include pre-treatment individual characteristics (age, sex), household composition, parental characteristics (risk attitudes, time preference), endowments (index of assets, ownership of house 10 years ago) as well as parental non-cognitive skills.

Table 6: Akanksha Treatment Effect on Expected Life Evaluation and Aspirations

		Akanksha – Community (1)	Akanksha – Classmates (2)	Community -Classmates (3)	Akanksha - Pooled (4)	Club-no club (5)	English Med. School – Non (6)
		Akanksha treatment effect	Akanksha treatment effect	Community treatment effect	Akanksha treatment effect	Club treatment effect	English medium treatment effect
Expected life evaluation in 10 years – Levels							
OLS, robust se	(1)	0.71* (0.42)	0.18 (0.37)	-0.66* (0.38)	0.28 (0.31)	0.23 (0.36)	0.06 (0.44)
Nearest Neighbour Covariate Matching	(2)	1.16*** (0.40)	1.09*** (0.42)	0.06 (0.42)	0.74** (0.36)	-0.34 (0.40)	0.24 (0.46)
Life evaluation – Difference-in-Difference							
Nearest Neighbour Covariate Matching	(3)	1.09* (0.68)	0.71 (0.68)	-0.11 (0.68)	2.01*** (0.53)	0.40 (1.06)	0.39 (0.81)
Aspirations Window – Levels							
OLS, robust se	(1)	0.32* (0.18)	0.41* (0.22)	-0.09 (0.16)	0.31* (0.17)	-0.01 (0.18)	0.33 (0.26)
Nearest Neighbour Covariate Matching	(2)	0.30^ (0.19)	0.52*** (0.19)	-0.13 (0.21)	0.37** (0.19)	-0.03 (0.22)	0.47* (0.26)
Aspirations Window – Difference-in-Difference							
Nearest Neighbour Covariate Matching (n=1, Mahalanobis)	(3)	0.69*** (0.28)	0.51** (0.23)	-0.28 (0.21)	0.87*** (0.20)	0.13 (0.25)	0.43 (0.30)
Total Observations		108	104	96	154	96	96

Significant at 10%; **Significant at 5%; ***Significant at 1%; standard errors in brackets. .

All estimators of standard errors are heteroscedasticity robust.

All specifications in this table include pre-treatment individual characteristics (age, sex), household composition, parental characteristics (risk attitudes, time preference), endowments (index of assets, ownership of house 10 years ago) as well as parental non-cognitive skills.

Table 7: Sensitivity analysis: Estimates of Akanksha Treatment Effects with Confounders

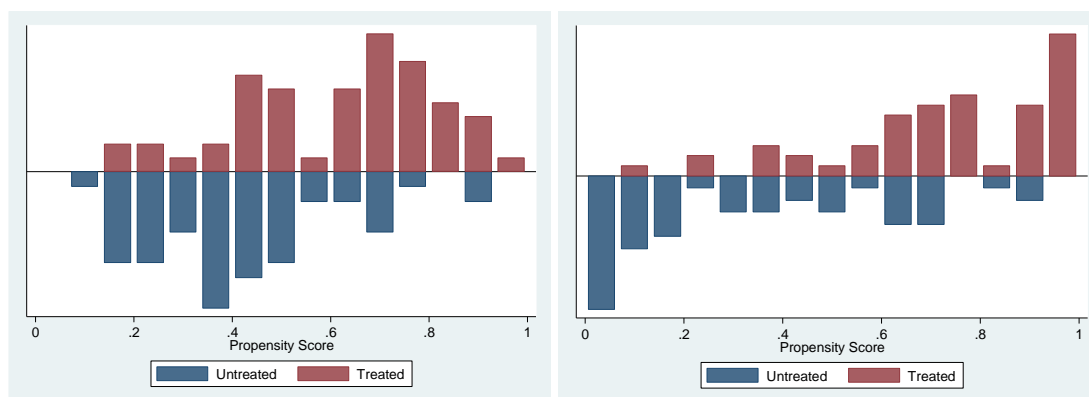
	SELF-ESTEEM								SELF-EFFICACY							
	Fraction U=1 by treatment/outcome				Λ	Γ	ATT w/ U	<i>s.e.</i>	Fraction U=1 by treatment/outcome				Λ	Γ	ATT w/U	<i>s.e.</i>
	p_{11}	p_{10}	p_{01}	p_{00}					p_{11}	p_{10}	p_{01}	p_{00}				
(1a)	(2a)	(3a)	(4a)	(5a)	(6a)	(7a)	(8a)	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)	(7b)	(8b)	
No confounder							0.66	0.24							0.45	0.20
Treatment =Akanksha and Control = joint (Community peers + Classmates) Confounder similar to:																
Owned dwelling (pre-treatment)	0.78	0.63	0.83	0.75	4.4	0.79	0.68	0.25	0.81	0.56	0.77	0.79	0.85	0.79	0.45	0.20
Electricity in dwelling (pre-treatment)	0.47	0.47	0.61	0.57	1.53	0.68	0.68	0.25	0.41	0.61	0.46	0.67	0.69	0.63	0.43	0.20
Water in dwelling (pre-treatment)	0.25	0.26	0.33	0.42	0.72	0.57	0.67	0.25	0.22	0.33	0.33	0.42	1.07	0.62	0.44	0.20
Primary carer is male	0.14	0.05	0.25	0.28	1.16	0.29	0.69	0.26	0.08	0.17	0.26	0.28	1.09	0.35	0.43	0.20
Father's education: secondary school plus	0.28	0.58	0.19	0.47	0.23	1.5	0.67	0.25	0.35	0.44	0.23	0.46	0.69	1.19	0.45	0.20
Mother nor working (pre-treatment)	0.64	0.68	0.81	0.78	3.22	0.57	0.70	0.25	0.59	0.78	0.90	0.72	5.24	0.54	0.45	0.20
Father's employment: manual labour (10 years ago)	0.39	0.53	0.42	0.43	1.29	1.14	0.68	0.25	0.49	0.33	0.51	0.37	1.69	1.19	0.45	0.20
Parent values responsibility	0.31	0.42	0.33	0.40	1.09	0.94	0.67	0.25	0.35	0.33	0.26	0.46	0.39	0.89	0.44	0.20
Parent values respectfulness	0.36	0.63	0.69	0.62	2.2	0.52	0.67	0.26	0.46	0.44	0.69	0.61	5.61	0.47	0.46	0.20
Parent values thrift	0.11	0.16	0.25	0.33	0.75	0.33	0.69	0.26	0.16	0.06	0.28	0.32	0.65	0.32	0.42	0.20
PPVT score above the mean	0.72	0.53	0.69	0.40	12.5	2.03	0.65	0.25	0.65	0.67	0.59	0.46	3.08	2.20	0.42	0.20

Γ =selection effect = odds ratio of logistic regression of confounder on probability of being treated, Λ =outcome effect = odds ratio of logistic regression of confounder on outcome, i =treatment indicator, j =outcome indicator (for continuous outcome variable – indicates whether outcome is above the mean)

Figure 2: Balancing graphs

Panel A: Untreated=community matches

Panel B: Untreated=classmates



Panel C: Untreated = pooled comparison group

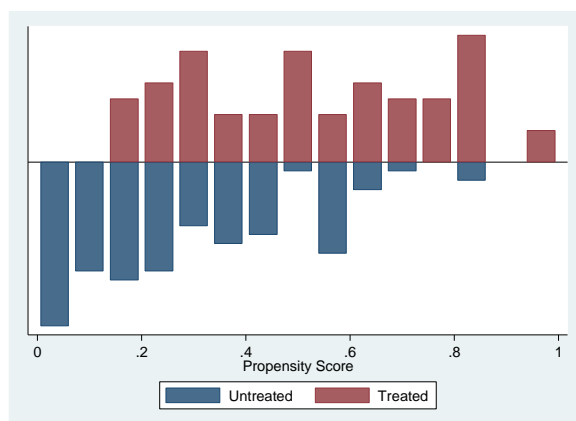


Table 8: Balancing test and common support for the main specifications

	LR chi-sq and p-values for balancing test (before matching)	LR chi-sq and p-values for balancing test (matched sample)	Off support
Akanksha-community	22.7 (0.019)	11.72 (0.39)	2 (out of 55)
Akanksha-classmates	49.5 (0.000)	9.07 (0.53)	14 (out of 55)
Community – classmates	23.14 (0.04)	8.14 (0.83)	11 (out of 50)
Akanksha – pooled	43.85 (0.000)	18.5 (0.19)	4 (out of 55)
Club – no club	33.24 (0.002)	14.6 (0.33)	7 (out of 25)
English medium – not	21.0 (0.102)	7.5 (0.91)	4 (out of 21)

Note: Balancing tests for main specifications – results presented in Tables 5 and 6. Off-support = observations with a propensity score which is higher than the maximum or lower than the minimum observations in the control groups.

Table 9 Sensitivity Analysis: Sensitivity of Estimates of Akanksha Treatment Effects to Common Support

	Akanksha – Community		Akanksha – Classmates		Akanksha - Pooled	
	(1)		(2)		(3)	
	Akanksha treatment effect		Akanksha treatment effect		Akanksha treatment effect	
Self-esteem – main result						
OLS	0.32	(0.25)	0.38	(0.29)	0.44*	(0.20)
Nearest Neighbour Covariate Matching	0.52**	(0.26)	0.70***	(0.28)	0.99***	(0.23)
Self-esteem – sensitivity to common support						
Trimmed Nearest Neighbour Covariate Matching (Dehejia&Wahba, 1999 method)	0.56**	(0.27)	0.51**	(0.26)	0.85***	(0.23)
Trimmed Nearest Neighbour Covariate Matching (5%) (Smith&Todd, 2005 method)	0.56**	(0.27)	0.62**	(0.29)	0.91***	(0.23)
Trimmed Nearest Neighbour Covariate Matching (10%) (Smith&Todd, 2005 method)	0.54**	(0.25)	0.53*	(0.27)	0.85***	(0.23)
Lechner Non-parametric Bounds (5%)	<i>Min</i>	<i>Max</i>	<i>Min</i>	<i>Max</i>	<i>Min</i>	<i>Max</i>
	0.463	0.640	0.57	0.75	0.82	0.99
Self-efficacy – main result						
OLS	0.37	(0.26)	0.18	(0.28)	0.41*	(0.22)
Nearest Neighbour Covariate Matching	0.90***	(0.28)	0.48**	(0.25)	0.93***	(0.24)
Self-efficacy – sensitivity to common support						
Trimmed Nearest Neighbour Covariate Matching (Dehejia&Wahba, 1999 method)	0.77***	(0.30)	0.49*	(0.27)	0.93***	(0.24)
Trimmed Nearest Neighbour Covariate Matching (5%) (Smith&Todd, 2005 method)	0.77***	(0.30)	0.42^	(0.27)	0.99***	(0.24)
Trimmed Nearest Neighbour Covariate Matching (10%) (Smith&Todd, 2005 method)	0.50**	(0.25)	0.40^	(0.26)	0.93***	(0.25)
Lechner Non-parametric Bounds (5%)	<i>Min</i>	<i>Max</i>	<i>Min</i>	<i>Max</i>	<i>Min</i>	<i>Max</i>
	0.688	0.927	0.33	0.50	0.88	1.12
Expected life evaluation in 10 years – main result						
OLS	0.71*	(0.42)	0.18	(0.37)	0.28	(0.31)
Nearest Neighbour Covariate Matching	1.16***	(0.40)	1.09***	(0.42)	0.74**	(0.36)
Expected life evaluation in 10 years – sensitivity to common support						
Trimmed Nearest Neighbour Covariate Matching (Dehejia&Wahba, 1999 method)	0.91**	(0.43)	0.52	(0.39)	0.55^	(0.37)
Trimmed Nearest Neighbour Covariate Matching (5%) (Smith&Todd, 2005 method)	0.91**	(0.43)	0.93**	(0.41)	0.64*	(0.36)
Trimmed Nearest Neighbour Covariate Matching (10%) (Smith&Todd, 2005 method)	1.06***	(0.43)	0.90**	(0.40)	0.45	(0.37)
Lechner Non-parametric Bounds (5%)	<i>Min</i>	<i>Max</i>	<i>Min</i>	<i>Max</i>	<i>Min</i>	<i>Max</i>
	0.88	1.17	0.81	1.06	0.54	0.83
Aspirations Window – main result						
OLS	0.32*	(0.18)	0.41*	(0.22)	0.31*	(0.17)
Nearest Neighbour Covariate Matching	0.30^	(0.19)	0.52***	(0.19)	0.37**	(0.19)
Aspirations Window – sensitivity to common support						
Trimmed Nearest Neighbour Covariate Matching (Dehejia&Wahba, 1999 method)	0.22	(0.20)	0.57***	(0.19)	0.35*	(0.18)
Trimmed Nearest Neighbour Covariate Matching (5%) (Smith&Todd, 2005 method)	0.22	(0.20)	0.46**	(0.22)	0.34*	(0.19)
Trimmed Nearest Neighbour Covariate Matching (10%)	0.14	(0.22)	0.46***	(0.20)	0.38**	(0.19)

(Smith&Todd, 2005 method)

Lechner Non-parametric Bounds (5%)	<i>Min</i>	<i>Max</i>	<i>Min</i>	<i>Max</i>	<i>Min</i>	<i>Max</i>
	0.12	0.23	0.38	0.49	0.24	0.35
Total Observations	108		104		154	

Notes: *Significant at 10%; **Significant at 5%;***Significant at 1%; Standard errors in brackets.

All estimators of standard errors are heteroscedasticity robust.

All specifications in this table include pre-treatment individual characteristics (age, sex), household composition, parental characteristics (risk attitudes, time preference), endowments (index of assets, ownership of house 10 years ago) as well as parental non-cognitive skills.

Dehejia & Wahaba method trims all observations with a propensity score which is smaller than the minimum or larger than the maximum of the opposite groups (Dehejia and Wahba, 1999)

Smith & Todd method trims observations in any interval for which the density within the non-treated distribution is below the threshold indicated in brackets (5 and 10 percent). (Smith and Todd, 2005)

See Caliendo & Kopeinig (2005) for derivation of Lechner non-parametric bounds.

Table 10 Sensitivity Analysis: Estimates of Akanksha Treatment Effects Using Alternative Matching Methods

		Akanksha – Community	Akanksha – Classmates	Community -Classmates	Akanksha - Pooled	Club- no club	English Med. School – Non
		(1)	(2)	(3)	(4)	(5)	(6)
		Akanksha treatment effect	Akanksha treatment effect	Community treatment effect	Akanksha treatment effect	Club treatment effect	English medium treatment effect
Self-Esteem levels – main result							
OLS	(1)	0.32 (0.25)	0.38 (0.29)	-0.07 (0.21)	0.44* (0.20)	0.03 (0.22)	0.07 (0.20)
Nearest Neighbour Covariate Matching	(2)	0.52** (0.26)	0.70*** (0.28)	0.13 (0.26)	0.99*** (0.23)	-0.11 (0.24)	0.36 (0.28)
Self-Esteem: alternative estimators							
FILM	(3)	0.56** (0.27)	0.50* (0.29)	0.01 (0.25)	0.55*** (0.22)	0.02 (0.23)	-0.002 (0.23)
Nearest Neighbour – 3 neighbours	(4)	0.54** (0.24)	0.71*** (0.23)	-0.06 (0.25)	0.73*** (0.20)	-0.06 (0.22)	-0.29 (0.23)
Propensity score matching	(5)	0.76** (0.29)	0.69** (0.34)	-0.08 (0.24)	0.78*** (0.24)	0.02 (0.26)	0.04 (0.26)
Self-Efficacy – main result							
OLS	(6)	0.37 (0.26)	0.18 (0.28)	-0.11 (0.22)	0.41* (0.22)	0.06 (0.26)	-0.48** (0.22)
Nearest Neighbour Covariate Matching	(7)	0.90*** (0.28)	0.48** (0.25)	0.09 (0.22)	0.93*** (0.24)	0.01 (0.28)	-0.28 (0.28)
Self-Efficacy: alternative estimators							
FILM	(8)	0.64** (0.30)	0.52* (0.30)	0.10 (0.22)	0.52** (0.23)	0.11 (0.28)	-0.48** (0.23)
Nearest Neighbour – 3 neighbours	(9)	0.71*** (0.25)	0.40* (0.24)	0.06 (0.21)	0.67*** (0.20)	0.03 (0.25)	-0.46* (0.25)
Propensity score matching	(10)	1.1*** (0.29)	0.20 (0.34)	0.05 (0.24)	0.88*** (0.24)	0.05 (0.28)	-0.39 (0.31)
Number of Observations		108	104	96	154	96	96

Note: *Significant at 10%; **Significant at 5%; ***Significant at 1%;

FILM = Fully Interacted Linear Model

All estimators of standard errors are heteroscedasticity robust. Self-esteem and self-efficacy are mean standardised scores of items in Table 1.

All specifications in this table include pre-treatment individual characteristics (age, sex), household composition, parental characteristics (risk attitudes, time preference), endowments (index of assets, ownership of house 10 years ago) as well as parental non-cognitive skills.

Table 11 Sensitivity Analysis: Estimates of Akanksha Treatment Effects Using Alternative Matching Methods

		Akanksha – Community	Akanksha – Classmates	Community -Classmates	Akanksha - Pooled	Club- no club	English Med. School – Non
		(1)	(2)	(3)	(4)	(5)	(6)
		Akanksha treatment effect	Akanksha treatment effect	Community treatment effect	Akanksha treatment effect	Club treatment effect	English medium treatment effect
Expected life evaluation in 10 years levels – main result							
OLS	(1)	0.71* (0.42)	0.18 (0.37)	-0.66* (0.38)	0.28 (0.31)	0.23 (0.36)	0.06 (0.44)
Nearest Neighbour Covariate Matching	(2)	1.16*** (0.40)	1.09*** (0.42)	0.06 (0.42)	0.74** (0.36)	-0.34 (0.40)	0.24 (0.46)
Expected life evaluation in 10 years: alternative estimators							
FILM	(3)	1.12*** (0.42)	0.25 (0.47)	-0.32 (0.37)	0.37 (0.35)	0.07 (0.42)	0.08 (0.33)
Nearest Neighbour – 3 neighbours	(4)	1.17*** (0.36)	0.68* (0.39)	-0.24 (0.35)	0.58* (0.33)	-0.035 (0.37)	0.23 (0.39)
Propensity score matching	(5)	0.99** (0.50)	0.73 (0.57)	-0.48 (0.42)	0.31 (0.39)	0.34 (0.42)	-0.52 (0.53)
Aspirations Window – main result							
OLS	(6)	0.32* (0.18)	0.41* (0.22)	-0.09 (0.16)	0.31* (0.17)	-0.01 (0.18)	0.33 (0.26)
Nearest Neighbour Covariate Matching	(7)	0.30^ (0.19)	0.52*** (0.19)	-0.13 (0.21)	0.37** (0.19)	-0.03 (0.22)	0.47* (0.26)
Aspirations Window: alternative estimators							
FILM	(8)	0.34* (0.20)	0.24 (0.24)	-0.08 (0.21)	0.26 (0.17)	0.04 (0.19)	0.23 (0.19)
Nearest Neighbour – 3 neighbours	(9)	0.24 (0.18)	0.55*** (0.18)	-0.25 (0.21)	0.32** (0.16)	0.001 (0.18)	0.41* (0.24)
Propensity score matching	(10)	0.42* (0.23)	0.42 (0.28)	-0.11 (0.18)	0.38** (0.20)	-0.12 (0.23)	0.52** (0.24)
Number of Observations		108	104	96	154	96	96

Note: *Significant at 10%; **Significant at 5%; ***Significant at 1%; Standard errors in brackets.

FILM = fully interacted linear model

All estimators of standard errors are heteroscedasticity robust.

All specifications in this table include pre-treatment individual characteristics (age, sex), household composition, parental characteristics (risk attitudes, time preference), endowments (index of assets, ownership of house 10 years ago) as well as parental non-cognitive skills.

Table 12: Sensitivity Analysis - Estimates of Akanksha Treatment Effects Using Alternative Sets of Covariates

		Self-esteem	Self-efficacy	Expected life evaluation in 10 years	Aspirations window
		(1)	(2)	(3)	(4)
		Akanksha treatment effect	Akanksha treatment effect	Akanksha treatment effect	Akanksha treatment effect
Nearest Neighbour Covariate Matching	(1)	0.99*** (0.23)	0.93*** (0.24)	0.74** (0.36)	0.37** (0.19)
Nearest Neighbour Covariate Matching (excluding non-cognitive skills)	(2)	0.41* (0.24)	0.83*** (0.27)	0.85* (0.48)	0.40** (0.20)
Entropy Balancing: Extended set of controls	(3)	0.64*** (0.23)	0.41** (0.21)	1.09** (0.46)	0.27 (0.22)
Entropy Balancing: Minimal set of controls	(4)	0.31* (0.19)	0.38** (0.19)	0.35 (0.31)	0.33** (0.15)
Number of Observations		154	154	154	154

Notes: *Significant at 10%; **Significant at 5%; ***Significant at 1%; Standard errors in brackets.

All models are estimated using the pooled comparison group.

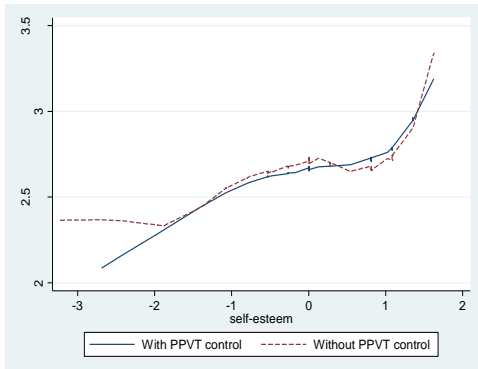
All estimators of standard errors are heteroscedasticity robust.

Extended set of controls: in addition to full set of controls included in the main model includes additional controls for wealth indicators (such as access to water, electricity and dwelling ownership), whether the child lived with a single parent 10 years ago, and additional measures of less observable parental characteristics including attitudes to local service provision and trust in local community

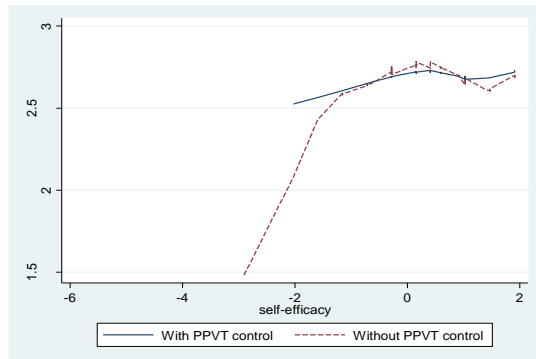
Minimal set of controls excludes all controls for parental attitudes, non-cognitive skills and preferences and only contains the asset index as a control for previous wealth, employment status of primary carer and the gender and age of the child.

Figure 3: Two-way Lowess Graphs of Standard 10 Results against Non-cognitive Skills

(a) Self-Esteem (standardised mean score)



(b) Self-Efficacy (standardised mean score)

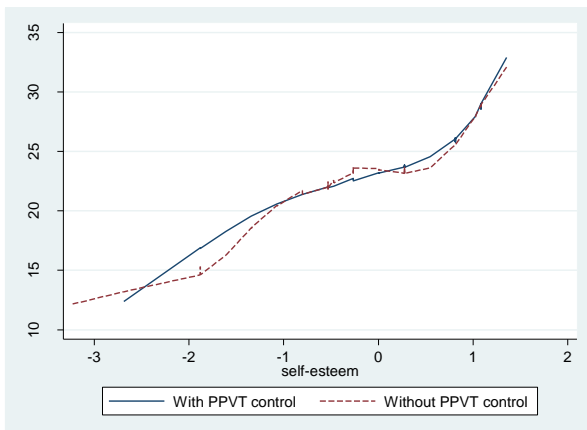


: Y-axis is

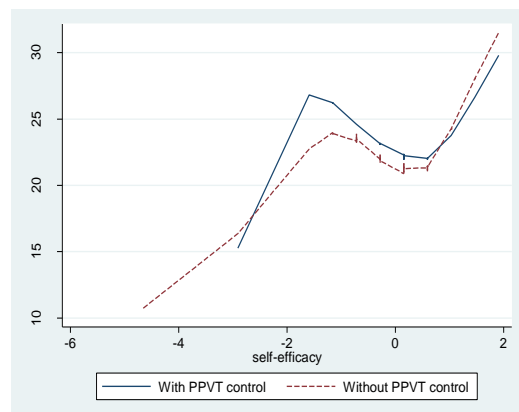
Note: Y-axis is Standard 10 exam results: 1=Fail, 2=Pass, 3=2nd Class, 4=1st class/Distinction

Figure 3: Two-way Lowess Graphs of Wages against Non-cognitive Skills

(a) Self-Esteem (standardised mean score)



(b) Self-Efficacy (standardised mean score)



Note: Y-axis is wages per hour in Rs.