Title: The Impact of Non-Cognitive Skills Training on Academic and Non-academic Trajectories: From Childhood to Early Adulthood

Authors: Yann Algan[1]*, Elizabeth Beasley[2], Frank Vitaro[3], Richard E. Tremblay[3,4]

Affiliations:

[1] Department of Economics, Sciences Po, Paris.

[2] LIEPP, Sciences Po, Paris.

[3] University of Montréal.

[4] University College Dublin.

*Correspondence to: yann.algan@sciencespo.fr

Abstract: Non-cognitive skills are closely associated with adult socio-economic success. However, it is unclear whether interventions targeting those skills, rather than cognitive skills, can improve adult outcomes. It is also unclear whether interventions after early childhood can have lasting effects. We show that an intervention focused solely on non-cognitive skills at age 7 can change the lifetime trajectories for children with deficits of non-cognitive skills, increasing self-control and trust in adolescence, improving education achievement, and outcomes in early adulthood such as criminality, education, employment and social capital. We show that improvements in trust and self-control explain much of the impact on education and young adult outcomes, and argue that social skills are an important but neglected aspect of non-cognitive skill development. Using conservative assumptions in a simple framework, we estimate that, as a lower bound, $1 invested in this program yields about $14 in benefits over the lifetime of the participants.

One Sentence Summary: Non-cognitive skill training at school entry boosts school and adult outcomes, suggesting causal role for self-control and trust.

Main Text:

Introduction

If schools seek to make people more prosperous and productive by improving cognitive skills, should they also explicitly teach non-cognitive skills? Substantial evidence shows that non-cognitive skills like self-control, motivation, and sociability are strongly associated with favorable school, economic and social outcomes (*1-3*). Information on whether interventions in elementary school can promote non-cognitive skills, and subsequently increase positive adult outcomes, is critical, especially for children who arrive at school with low levels of social skills and self-control. These children are more likely to struggle in school, have behavior problems, and be locked into poverty in adulthood. Providing more equal opportunities for all children may not depend only on training in math and reading, but also in self-control and socialization.

Much of the large benefit of early childhood interventions may be due to improvements in non-cognitive, rather than cognitive, skills. Experiments measuring the impact of investments in early childhood cognitive development, such as the Abecedarian project, the Perry Preschool program, Head Start or Project STAR, suggest that a substantial part of the powerful long-term impact of these programs is due to increases in skills that are not measured by grades or IQ tests – suggesting a very important role for non-cognitive skills (*4*,*5*). It is important to know what these critical non-cognitive skills are, whether it makes sense to target them directly, and whether the window for intervention is wide or narrow – in

other words, is elementary school "too late"? It is well established that early childhood is a critical period for formation of cognitive skills, non-cognitive skills (*6,7*) The last question is particularly important for countries and communities where pre-school education is not universal, and so behavioral issues may not even be identified until children begin primary school.

This paper provides evidence on these questions by estimating the impact on lifetime trajectories of a randomized non-cognitive skills training program at school entry for disruptive kindergarten boys from low socioeconomic environments. The intervention consisted of a 2-year program aimed at enhancing self-control and social abilities beginning at age 7. We examine the impact of the training on the development of a large set of non-cognitive skills and school performance throughout adolescence, and connect these improvements with existing evidence on adult crime and education outcomes and new evidence on adult economic and social capital outcomes. We also provide a cost-benefit analysis of the program. The rich dataset allows us to crack open the "black box" of non-cognitive skills (*8*). It also sheds new light on our understanding of skill formation (*7,9,10*), the malleability of non-cognitive skills at school entry, and the triggering effect of non-cognitive skills on latter academic achievement.

We find that, in the early adolescent period, the intervention increased aggression control, attention-impulse control, and trust. During the late adolescent period, the intervention improved school performance, aggression control, and trust. For impacts in early adulthood, we present new data showing that the treated group is more likely to be active fulltime in either work or school from ages 17-27, and to belong to a civic or social group. This result complements earlier findings (*11*) showing higher rate of secondary-school graduation for the treatment group and a large reduction in criminal behavior. We then present evidence that the changes in late adolescent school performance and young adult outcomes are explained by changes in behavior in the early adolescent period. We provide suggestive evidence on the amount of the impact on each outcome that can be explained by changes in Self-Control (Aggression Control and Attention-Impulse Control) and Trust.

The results from our decomposition exercise are directly connected to the debate in the literature about the interaction between non-cognitive and cognitive skills, the contribution of non-cognitive skills to cognitive outcomes, school performance, and their related outcomes in adulthood (*12-13*). In particular, our results are related to non-experimental longitudinal studies showing a strong association between childhood self-control and a variety of adult outcomes such as school achievement, health and criminality. The crucial contribution of this study is that there is an exogenous variation in self-control levels thanks to the randomization of the intervention, whereas most evidence on the effect of self-control is based on non-experimental longitudinal studies, which cannot address the issue of causality (*14-20*).

This study differs from the rich and growing literature on childhood development programs in terms of the characteristics of the target population, length of follow up, the strength of causal identification and the content of the program.

This study targets a different population than most previous studies: children with high deficits in social skills at school entry. The selection criterion of previous studies was mostly based on low IQ or low-income status of parents or neighborhoods. In addition, it provides evidence of effectiveness of an intervention after early childhood, and there is little evidence on the effectiveness of non-cognitive skills training during the early elementary school years (*12,21*). As discussed above, understanding whether the window of intervention is narrow or wide has significant policy relevance. Programs have begun either at birth, like the Nurse-Family Partnership (*22*) or the Abecedarian Program (*23,24*), or before the age of

3, like the Jamaican Supplementation Study (*25*), the Perry Preschool Program (*26*), or the Head Start program (*27,28*).

This study has a longer follow-up than most previous studies. The few interventions that have targeted elementary school students generally do not have long-term follow-up on both the development of skills during adolescence and later adult outcomes (*12*). In a meta-analysis of 213 school-based emotional learning programs, only 15% of those programs have a follow-up that lasts beyond 6 months, and the other ones have very short follow-up programs compared to the MLES (*21).* A randomized experiment of non-cognitive skill training in Chicago is close to ours (*29*) but not enough time has elapsed since implementation to measure long-term impacts. The Seattle Social Development Project had unusually long-term follow up that showed positive impacts (*30-33*). The MLES program is different because it was randomized at the individual level, providing an opportunity for strong causal inference, and because it has a broader set of observations and more frequent assessments of a large variety of non-cognitive skills from childhood to early adulthood.

The content of the MLES curriculum targeted only non-cognitive skills while most of the other early childhood interventions taught cognitive skill development (like Abecedarian and Head Start) or combined non-cognitive training with cognitive training (Perry Preschool). In this way, the MLES is also different from the Fast Track program (*34, 35*) which included both academic tutoring and social skills training. The Cambridge-Somerville Youth Study evaluated training in non-cognitive skills with a long-term follow-up, which actually had a negative impact on participants at age 30 (*36*), but the program intervention was itself fundamentally different from that of the MLES: children with behavioral problems were grouped together for treatment, and this may have given rise to the stigma or negative influence of deviant peers that led to the unintended negative consequences.

Several dimensions of the MLES program have been discussed in the psychology literature, but it is generally centered on testing measures of and theories on the development of aggressiveness during adolescence (*37-44*) and primarily relies on scales designed to test one psychological construct at a time. Other studies have examined the long-term effect of the MLES on secondary completion and crime (*11, 45*).

In this study we investigate the impact of the intervention on a wide set of non-cognitive and cognitive skills at adolescence, using new data that has not been previously presented or analyzed. We take a much broader approach to identify the development of all potential cognitive and non-cognitive skills during adolescence rather than restricting the analysis to aggressiveness. This approach opens the way to identifying new, potentially important, skills, which are then systematically examined for their relationship to academic achievement during adolescence and adult outcomes using the same framework. While we do find some skills related to disruptiveness as addressed in previous papers, there are notable differences, and we add several new skills: new data includes information on school performance and grades, trust, altruism, self-esteem, friends, and parent behaviors. We also extend the analysis of these diverse cognitive and non-cognitive skills as potential pathways for impacts on education, crime, economic and social outcomes and provide suggestive evidence on the relative contribution of several different skills.

Experimental Design and Data

In the spring of 1984, the Montreal Longitudinal Experimental Study (MLES) evaluated 1037 boys at the end of kindergarten (age 6) in low socio-economic areas. From this original sample, 250 boys were targeted for the experiment based on teacher ratings of disruptive behavior. These 250 boys were randomly assigned to either participate in the social skills/parent training program or to be part of the control group. Detailed information on the behavior rating, sampling, timeline of the intervention and data collection, and information

on tests for the balance of baseline characteristics are given in Supplementary materials section B.

The intervention took place when the participants were between 7 and 9 years old. Over the two-year period, 19 sessions were carried out at school by a team of professional childcare workers, a social worker and a psychologist. The sessions were carried out in small groups, which included one disruptive boy and 3 or 4 non-disruptive boys. The first year was dedicated to social behavior training, such as how to ask a question, how to say no politely, or how to join a game. The second year was dedicated to self-control, such as how to react to teasing or when angry, or how to identify the intentions of others. The parents received a home-based training program in child rearing to review and reinforce the lessons taught at school at home. The control group did not have access to this program but had access to all of the standard programs and resources available to the Montreal public school children in this period. Detailed information on the program is given in Supplementary materials section A.

Of those assigned to the treatment group, 67% agreed to participate. Table S 2 shows the difference between compliers and non-compliers on a number of baseline variables. Differences are significant at the 10% level or higher for 4 out of 21 variables tested: Prestige of mother's job, age of mother, initial aggression and initial fighting. Our estimates control for those baseline characteristics. Our preferred specification uses Intention to Treat (ITT) estimation, so that all subjects assigned to the treatment group are considered treated for the analysis, and we provide Treatment on Treated (TOT) estimates in Table S 20 for completeness.

Following the two-year intervention program between ages 7-9 (1985-1987), the MLES collected detailed longitudinal data for two decades on the boys' development during adolescence, including self-reported behavior, grades, relationship with parents, and teacher-reported behavior, and later adult outcomes at ages 17-27, including educational achievement, crime, employment and social capital from both administrative data and self-reported surveys. The surveys took place around age 20-21 and 26-27. The MLES also collected the same longitudinal data for the boys who were not identified as disruptive during the kindergarten year (N= 787). We use data from both groups as the comparison between the experimental group and the non-disruptive group helps understand the size of the impact by estimating to what extent the intervention helped the disruptive boys "catch up" to the non-disruptive. Details on constructing the measures are given in Supplementary materials section C.

We identify two skills that deal with self-control, based largely on the behavioral dimensions used to identify the disruptive sub-sample in kindergarten: Aggression Control, that is, control over aggressive behavior towards persons or towards property (such as fighting, bullying, and destroying objects), and Attention-Impulse Control, that is, control over impulsive behavior in tasks that require self-control (sitting still, remaining on task, focusing). We also identify four additional skills: Trust, Friends, Altruism and Self-Esteem. Trust includes trust in others and also the ability of perspective taking, that is, to understand the intentions of another person. Our measure of trust is thus more subtle than a definition where someone may be "trusting" if they simply tend to believe what other people tell them, it is more closely related to someone's attitude toward the intentions of institutions and other people (in particular, people who are outside of their immediate social circle). Friends measures the closeness of relationships friends, Altruism measures voluntary altruistic and compassionate behavior, and Self-Esteem measures feelings of value and self-worth. There is also a measure of social capital, Group Membership, which is the only non-cognitive skill that is measured by a single variable (whether or not the individual belong to a social group at ages 16 or 17). Figure 1 parts A, B, and C shows the distributions for the treatment and

control groups, as well as the non-disruptive sub-sample, of the Aggression Control, Attention-Impulse Control, and Trust skills.

For cognitive skills and school performance, verbal IQ was tested when the subjects were around 13 years old using the Sentence Completion Test (*46*), and we have data on yearly grades in Math and French, as well as whether the subject had been held back or was in special education class each year. Figure 1 part D shows the percent of subjects held back a grade at each age.

Full details for each skill, including all distributions, summary statistics, the number of components, the Cronbach alpha index for internal consistency, and the mean and number of observations for each sub-sample are given in Supplementary materials section C. We also measure, but find no impact on, various aspects of parent behavior, reported in Supplementary materials section E.

The adult outcome variables are measured with questionnaires and administrative data. We use self-reported data on activities from ages 17 to 27 to construct a variable of the percent of reported years from 17-27 where the subject was active full time by either school or work or both. To supplement the data on fulltime activity, we also provide estimates on whether the subject was employed at age 27, employed fulltime at age 27, and the percent of years from ages 17-27 during which the subject reported receiving government transfers, and the percent of years from ages 17-27 that the subject was inactive. We also measure the hours worked at age 27 which we use as one measure of cost-effectiveness in section 5. We use self-reported data on whether or not the subject reported belonging to a civic or social group at age 21 or age 27. We use administrative information on whether each subject had received a secondary-school level degree, whether they had a criminal record, the number of offenses and whether the offense was violent or non-violent, collected when the participants were around 23-24 years old (2003). Summary statistics on adult outcome are given in Supplementary materials section C.

For the adolescent outcomes that we focus on, in early adolescence the average rate of attrition is 4%, and in late adolescence the average rate of attrition is 10%. Adult education and criminal outcomes come from administrative data for which attrition is virtually zero. However, the primary economic and social outcomes suffer from high attrition of around 40% (38% in the control group and 44% in the treatment group) that could threaten internal validity if it is correlated with treatment. In no case is the rate of attrition statistically significant between the treatment and the control group. Supplementary materials section B details attrition rates between groups and the relationship to compliance. We discuss the strategies we use to deal with attrition below.

Impact of the program

Figures 1 and 2 show the raw differences in selected outcomes, and Tables 1 and 2 show the results from different specifications for selected outcomes, and our preferred specification is column 5 of Table 1 and column 6 of Table 2. Supplementary materials section D provides details on the specifications and detailed results for all outcomes.

In early adolescence, we find that the treatment has a significant impact on non-cognitive skills: Aggression Control is higher in the treatment group (0.15 standard deviations, significant at the 10% level, about 36% of the difference between the disruptive and non-disruptive sub-samples), Attention-Impulse Control (0.19 standard deviations, significant at the 5% level, about 57% of the difference between the disruptive and non-disruptive sub-samples) is higher, and Trust is higher (0.17 standard deviations, significant at the 5% level, about 62% of the difference between disruptive and non-disruptive). There is no impact on the other behavioral factors in early adolescence: Friendship, Self-Esteem, and

Altruism, and no impact on IQ scores, grades, being held back, or being placed in a Special Education Class.

In late adolescence, treatment has a significant impact on Aggression Control (0.18 standard deviations, significant at the 5% level, or 61% of the gap between the disruptive and non-disruptive sub-samples) and Trust (0.18 standard deviations, significant at the 5% level, or 68% of the gap between disruptive and non-disruptive sub-samples). We find no impact on Attention-Impulse Control in late adolescence. In contrast to the early adolescent period, we observe a large impact on school performance in the late adolescent period: Grades (about 0.30 standard deviations, or 74% of the gap between disruptive and non-disruptive sub-samples), percent of years Held Back (14 percentage points, 65% of the gap between disruptive and non-disruptive sub-samples) and percent of years in Special Education (14 percentage points, or 65% of the gap between disruptive and non-disruptive samples) (all significant at the 5% level). In addition, there is an increase in social group membership (10 percentage points, significant at the 5% level). The impact on social group membership is very large relative to the gap between the disruptive and the non-disruptive sub-samples, over five times as large, but this is because the gap itself is quite small. Indeed, overall group membership is low.

For crime and secondary completion, we find results equivalent to those previously reported (*11*). The treatment group committed 1.1 fewer crimes per person, significant at the 10% level, bringing the treatment group about 80% of the way "back" to the non-disruptive sub-sample. The impact is largely driven by reductions in nonviolent crimes (for example, drug offenses) rather than violent crimes. Participants in the treatment group are 18 percentage points more likely to have received a secondary school diploma than participants in the control group, significant at the 1% level (note that only 32% percent of control group participants completed secondary school). Our specification shows that the treatment reduced the gap between the non-disruptive and disruptive sub-samples by 80%.

We find a significant impact on economic performance and social capital.[1] Treatment participants were active fulltime in work or school between 8 and 12 percentage points more than the control group (the control group average is 80%), significant at the 5% level. Treatment group members were 22 percentage points more likely to belong to a social group, from 32% in the control group, significant at the 5% level. Both the impacts on percent of years active fulltime and on social capital are large and greater than the size of the gap between the disruptive and non-disruptive groups. (Recall that the impact on social group membership in adolescence also exceeded the gap between the disruptive and non-disruptive sub-samples).

Robustness Checks

There were 53 schools in 1984, with an average of 20 total participants, 2 treatment participants and 4 control participants, per school. Since randomization was carried out at the individual level (within schools), and control and treatment participants are present in each school, neither fixed effects nor clustering are required, but we present these results as robustness checks. We present a specification that includes clustered standard errors at the level of the school in 1984, and a specification that includes fixed effects at the level of the school in 1984.

Since the sample size is small, we also include the p-value from a permutation (randomization) test of the difference in means (where treatment group assignment was randomly permuted within the sample) with 2000 draws. That is, treatment group assignment was randomly re-assigned 2000 times, and the simple difference in means (or proportions) was calculated for each draw. The p-value is the proportion of draws that have a difference in means as large (in absolute value) as the difference observed in the true sample.

Attrition is marginal for observations on cognitive and non-cognitive skills during adolescence, and for secondary-school completion and criminal records that are based on administrative data, but is a potentially important issue for the economic and social adult outcomes. We test whether attrition for adult outcomes is related to 21 variables collected prior to the program, and whether it is related to treatment conditional on those baseline variables.[2] Results are given in Supplementary materials section B. There are two variables of potential concern: father's initial work status and hyperactivity. We control for these variables in all regressions using adult data subject to attrition.

In order to get an idea of the possible direction and magnitude of any bias, we estimate the bias introduced by the same level of attrition into the estimate of the program impact on secondary school completion. Administrative data on secondary school completion is available for nearly the entire sample (242 out of 250 participants, or 97%). We use the following procedure. We create a false secondary school variable that takes the value of missing if the subject is missing data for our principal economic outcome (percent of years active fulltime) and the value of the true secondary school variable if they do have economic outcome data. In this way we mimic the level of attrition in the adult questionnaire data in the administrative data (we call this the "attrited" dataset). We then estimate the impact of the program on secondary school completion using our preferred specification. We compare this to an estimate of the impact of the program using data from the entire sample (the "true" dataset). The validity of this comparison rests on the assumption that the direction of the bias induced in the secondary school completion is likely to be the same as the direction of the potential bias in the fulltime occupation data. The results presented in Table S 6 show that if we had observed the same pattern of attrition for secondary completion as we do for percent active fulltime, we would underestimate the impact. This test provides some reassurance that our results are not driven by attrition bias.

We also use inverse probability weighting (IPW) as an additional check. IPW gives higher weight to observations that are similar to those missing outcome data (*24*). This procedure cannot recover the true distribution of outcomes except under strong assumptions, but provides an additional robustness check. We use a logit specification with a dummy variable for attrition as the dependent variable and all available baseline variables as the independent variables. Using the coefficients from this estimate and the values of the baseline variables, we calculate the likelihood that each subject will attrit regardless of actual attrition status. Participants are weighted using the inverse of the likelihood of having data on the outcome, so that participants who are similar to attriters have higher weight in the estimate of program impact. We include the estimate using IPW for variables with attrition. In general, the IPW estimates of impact are slightly larger than the unweighted estimates.

Trust and Self-Control as Potential Mechanisms

We observe an impact on Self-Control and Trust in early adolescence, and subsequent improvements in school performance and young adult outcomes. To what extent are these changes explained by the initial changes in non-cognitive skills? That is, is the initial boost in trust and self-control responsible for the later improvements in school, crime, and labor market outcomes? Note that non-cognitive skills explain a higher share of the variance in later school performance than early adolescent school grades and IQ. Figure 3 shows the adjusted R-squared for regressions on school achievement, where the independent variables are IQ, the non-cognitive skills evaluated here, or both. It shows that, except for Special Education, Trust and Self-Control explain substantially more of the variance in school achievement.

Here, we investigate the potential mechanisms for explaining the impact on academic achievement and adult outcomes, following some aspects of previous approaches to estimating mechanisms (*4*). Details of the estimation strategy are given in Supplementary materials section F. We present these results with the caveat that whereas the impact of the treatment on the non-cognitive skills, school performance, and young adult outcomes is well-identified thanks to the random assignment of the treatment, the analysis presented here depends on the observed correlation of the non-cognitive skill to the outcome, which may be subject to omitted variable bias. Nonetheless, these results, combined with the fact that we find no impact on other non-cognitive skills or cognitive skills in early adolescence, and the fact that these were precisely the skills that were targeted by the program, provide strong suggestive evidence that trust and self-control are mechanisms for the longer-term impact of the intervention.

Figure 4 shows the proportion of the impact on each outcome that is explained by changes in adolescent skills, and Tables S 31 and S 32 present the estimates and p-values. Half of the impact on being Held Back is explained by changes in Attention-Impulse Control (50%, significant at the 5% level), and 20% is explained by changes in Trust (significant at the 10% level), leaving 30% of the impact unexplained though this direct impact is not significant. There is essentially the same pattern for assignment to Special Education Classes: 36% (significant at the 10% level) of the impacts explained by Attention-Impulse Control, 21% (significant at the 5% level) is explained by Trust. The unexplained impact is not significant. The impact on Grades is not explained by Trust or by Aggression Control, but about half (54%, significant at the 5% level) is explained by Attention-Impulse Control.

Changes in Attention-Impulse Control explain the greatest proportion of the treatment impact on secondary completion (about 52%, significant at the 5% level), while Trust (13%, not significant but with a p-value of 0.10) and Aggression Control (9%, significant at the 10% level) may also play a role. Recall that we found no impact of the program on IQ or grades in early adolescence, so this again underlines that non-cognitive skills, in particular Attention-Impulse Control, are important determinants of school achievement.

Changes in Aggression Control explain the greatest proportion of the treatment impact on Number of Crimes (32%, significant at the 10% level), Attention-Impulse Control and Trust explains 5% (not significant). Changes in Attention-Impulse Control and Trust explain equal shares of the treatment impact on percent active fulltime: each about 14% (Attention-Impulse Control significant at 5% and Trust at the 10% level). Changes in Trust are the only skill that explains even a part of the impact on group membership, about 6% of the impact, and it is not significant.

Cost-effectiveness and Rate of Return

While it is difficult to estimate the precise rate of return to such a program because it is difficult to monetize the benefits (for an exception, see *47*), it may be useful for policymakers to know the order of magnitude of the benefits in comparison to the costs;

We estimate the cost of the program based on known staff costs, as the principle cost of the program was the salary of the trainers and implementers, and no other particular inputs were used. The implementation team was composed of one full time social worker, two full time childcare specialists (BA level), one psychologist, and one half-time program administrator, full time over the course of the program (two years). We do not include the cost of evaluation or questionnaires. We use median reference hourly wages for these professions in Quebec in 2011 (*48*), adjusted for inflation to 1985 (half a year), 1986 (full year), and 1987 (half a year). We assume 40 hours per week paid for 52 weeks. We assume that other costs amount to 30% of salaries (photocopies, transport, training, and so on). Under these assumptions, we calculate that the total program cost per person (for two years) was around 9,500 in 2013 USD.

In order to understand how the benefits relate to the costs of the intervention, we compare the benefits to the costs in two ways. First, we calculate how much each increment of benefit "cost" under the intervention. For example, how much did it cost, using this program, to avert one crime? This type of estimate of cost-effectiveness measures the effectiveness of a program in terms of the cost of attaining a desired outcome. It does not rely on monetizing the outcomes. It can be useful for making general comparisons between programs that have similar policy goals (*49*). The detailed assumptions underlying this analysis are presented in Supplementary materials section G. We calculate that the cost of averting one crime is around 3,100 in 2013 USD, the cost of each additional secondary graduation is around 29,300 in 2013 USD. We also estimate that the cost of each repeated year avoided is 8,600 in 2013 USD and the cost of each year of special education avoided is 7,700 in 2013 USD. We calculate that each additional year of fulltime work of about 1,800 in 2013 USD.

Second, we calculate a lower bound overall rate of return based on a set of reasonable and conservative assumptions, and show the proportion of return accounted for by each type of benefit. This estimate gives a general idea of how good an investment this type of intervention is likely to be, compared to other interventions with different policy goals. Our calculation of the total monetary benefits of the program is a lower bound: it excludes monetary benefits from increased hourly wage, because calculating the benefits in increased hourly wage requires making assumptions about the replacement wage of the unemployed, increased high school graduation, and reduced social cost of crime. We calculate that each dollar invested yields about 14 dollars in net benefits over 53 years. An equivalent compound interest rate, taking into account the 53 years elapsed between investment at age 7 (the intervention) and the full benefits accrued at age 60, is around 5%. Since labor market activity accounts for a large proportion of the returns, the estimate is sensitive to the amount of labor returns, and these returns may fade over time. If labor market returns decline yearly at the rates of 5%, or 15%, the respective equivalent compound interest rates are 4% or 2%. Details of the cost-effectiveness estimates and a sensitivity analysis are presented in Supplementary materials section G.

Conclusion

We find that a non-cognitive skills training program conducted in inner-city Montreal in the 1980s with disruptive boys led to substantial improvements in non-cognitive skills in early and late adolescence, improvements in school performance in late adolescence, and

long-term positive effects early adult outcomes. We use a rich longitudinal dataset to examine cognitive and non-cognitive skills and find that the program changed levels of trust and self-control (both in terms of controlling aggression and controlling attention and impulses) in adolescence, and we provide suggestive evidence that these non-cognitive skills are the mechanisms by which adult outcomes are improved. An important result from this paper is the evidence that self-control is critically important for academic achievement as an adolescent, and directly linked to young adult outcomes.

Our findings highlight the role of trust as an important factor for explaining academic achievement and adult outcomes. A growing literature shows an important association between social capital and trust and community or country-level outcome like income per capita (*50*-*52*). A recent study shows that inter-generational mobility is highly correlated with social capital at the local level, measured by voter turn-out or civic associations in US districts (*53*). At the same time, relatively little research attention has been paid to the causal impact of trust on individual success (as compared to self-control) and the mechanisms behind the relationship between trust and aggregate outcomes are poorly understood. We present evidence at the individual level on this mechanism: higher levels of trust help people function better in secondary school and on the job market, controlling for other cognitive and non-cognitive skills. Our evidence on the important role of trust suggests that new investment should be made in refining the measure of trust in others and perspective taking, and how to build those social skills in early childhood.

The results reported in this paper demonstrate that increased investment in early childhood development programs for disruptive boys from low socioeconomic environments is likely to be an efficient and profitable public policy, especially where such programs explicitly incorporate simple strategies to foster the development of social skills. Outside of the public interest in the welfare of these children, such investments are likely to be particularly prudent in terms of cost to the education system, the welfare system, the juvenile justice system and the criminal justice system.

References and Notes:

[1] While we focus on percent active fulltime as our primary economic outcome of interest, we present impacts on related economic outcomes. We observe significant effects on percent of years 17-27 working, and employment, full-time employment, number of hours worked and hourly wages at age 27. There is no significant impact on the percent of government transfers, though the point estimates are in the expected direction. There is no impact on activity at age 21.

[2] Whether both parents were living at home; age of mother and father at birth of subject; education of mother and father; work status of mother and father; prestige of job of mother and father, number of children in the household; adversity index of household; age of subject in 1984; whether or not the subject went to pre-school; and initial scores on aggression, anxiety, opposition, pro-sociality, combativeness, inattention, hyperactivity, and anti-social behavior.

1. L. Borghans, A. L. Duckworth, J. J. Heckman, B. ter Weel. The economics and psychology of personality traits. *Journal of Human Resources* **43**(**4**), 972-1059 (2008).
2. M. Almlund, A. Duckworth, J. J. Heckman, T. Kautz, Personality psychology and economics. In E. A. Hanushek, S. Machin, L. Woessmann (Eds.), *Handbook of the Economics of Education*, Volume 4, 1-181. Amsterdam: Elsevier. (2011).
3. A. L. Duckworth, D. Weir, E. Tsukayama, D. Kwok, Who does well in life? Conscientious adults excel in both objective and subjective success. *Frontiers in psychology*, **3** (2012).
4. J. J. Heckman, L. Malofeeva, R. Pinto, P. Savelyev, Understanding the mechanisms through which an influential early childhood program boosted adult outcomes. *American Economic Review*, **103(6)**, 2052-86 (2013).
5. R. Chetty, J. Friedman, N. Hilger, E. Saez, D. W. Schanzenbach, D. Yagan, How does your kindergarten classroom affect your earnings? Evidence from project Star. *Quarterly Journal of Economics* **126(4)** (2011).
6. D. Almond, J. Currie, Human Capital Development Before Age Five. *Handbook of Labor Economics*, Volume 4, Elsvier (2010).
7. J. J. Heckman, Skill formation and the economics of investing in disadvantaged children. *Science* **312**, 1900-1902 (2006).
8. S. Bowles, A (Science-Based) Poor Kids' manifesto, (Review of Heckman James, Giving Kids a Fair Chance: A strategy that works, Cambridge MIT), *Science,* **340,** 1044-1045 (2013).
9. F. Cunha, J. J. Heckman, Formulating, identifying and estimating the technology of cognitive and noncognitive skill formation. *Journal of Human Resources* **43(4)**, 738-782 (2008).
10. F. Cunha, J. J. Heckman, S. M. Schennach, Estimating the technology of cognitive and noncognitive skill formation. Econometrica **78(3)**, 883-931 (2010).
11. R. Boisjoli, F. Vitaro, E. Lacourse, E. D. Barker, R. Tremblay, Impact and clinical significance of a preventive intervention for disruptive boys: A 15-year follow-up. *British Journal of Psychiatry*, **191,** 415-419 (2007).
12. J. J. Heckman, T. Kautz, Fostering and Measuring Skills: Interventions that improve Character and Cognition, IZA working paper (2013).
13. F. Vitaro, M. Brendgen, R. E. Tremblay. Early predictors of high school completion: The developmental interplay between behavior, motivation, and academic performance. In M. Boivin, K. Bierman (Eds.), *Promoting school readiness and early learning:*

implications of developmental research for practice (15-45, ch. 2). New York, NY: Guilford Press (2014).
14. A. L. Duckworth, The significance of self-control. *Proceedings of the National Academy of Sciences*, **108**(**7**), 2639-2640, 2011.
15. A. L. Duckworth, M. E. P. Seligman, Self-discipline outdoes IQ in predicting academic performance of adolescents. *Psychol Science* **16**, 939–944 (2005).
16. R. Diamond, W. S. Barnett, J. Thomas, S. Munro, Preschool program improves cognitive control. *Science* **318**, 1387–1388 (2007).
17. T. E. Moffitt, L. Arseneault, D. Belsky, N. Dickson, R. J. Hancox, H. Harrington, R. Houts, R. Poulton, B. Roberts, S. Ross, M. Sears, W. Thomspon, A. Caspi, A gradient of childhood self-control predicts health, wealth, and public safety. *Proceedings of the National Academy of Sciences*, **108**(**7**), 2693-2698. (2011).
18. J. B. Pingault, S. M. Côté, F. Vitaro, B. Falissard, C. Genolini, R. E. Tremblay, The developmental course of childhood inattention symptoms uniquely predicts educational attainment: A 16-year longitudinal study. *Psychiatry Research* **219**(**3**), (2014).
19. J. B. Pingault, S. M. Côté, E. Lacourse, C. Galéra, F. Vitaro, R. E. Tremblay, Childhood hyperactivity, physical aggression and criminality: a 19-year prospective population-based study. *PloS one*, **8**(**5**), e62594 (2013).
20. J. B. Pingault, R. E. Tremblay, F. Vitaro, R. Carbonneau, C. Genolini, B. Falissard, S. M. Côté, Childhood trajectories of inattention and hyperactivity and prediction of educational attainment in early adulthood: a 16-year longitudinal population-based study. *American Journal of Psychiatry*, **168**(**11**), 1164-1170 (2011).
21. J. A. Durlak, R. P. Weissberg, A. B. Dymnicki, R. D. Taylor, K. B. Schellinger, The impact of enhancing students' social and emotional learning: A meta-analysis of school-based universal interventions. *Child Development* **82** (**1**), 405-432. (2011).
22. K. S. Howard, J. Brooks-Gunn, The role of home-visiting programs in preventing child abuse and neglect. *The Future of Children,* **19**(**2**), 119-146 (2009).
23. F. Campbell, C. T. Ramey, E. Pungello, J. Sparling S. Miller-Johnson, Early childhood education: Young adult outcomes for the Abecedarian project. *Applied Development Science*, **6**, 42-57 (2002).
24. F. Campbell, G. Conti, J. Heckman, S. Moon, R. Pinto, E. Pungello, Y. Pan, Early childhood investments substantially boost adult health. *Science,* 343, 1478 (2014).
25. P. Gertler, J. Heckman, R. Pinto, A. Zanolini, C. Vermeersch, S. Walker, S. Chang, S., Grantham-McGregor, Labor market returns to early childhood stimulation: A 20-year follow up to an experimental intervention in Jamaica. *NBER Working Paper* 19185 (2013).
26. J. J. Heckman, S. H. Moon, R. Pinto, P. A. Savelyev, A. Q. Yavitz, Analyzing social experiments as implemented: A reexamination of the evidence from the High Scope Perry Preschool. *Quantitative Economics* **1(1)**, 1-46 (2010).
27. J. Currie, T. Duncan, Does Head Start make a difference? *American Economic Review*, **85**, 341-364 (1995).
28. J. Ludwig, D.L. Miller, Does Head Start improve children's life chances? Evidence from a regression-discontinuity design. *Quarterly Journal of Economics* **122**(**1**), 159-208 (2007).
29. S. Heller, H. Pollack, R. Ander, J. Ludwig, Improving social-cognitive skills among disadvantaged youth: A randomized field experiment, Univ. Chicago Working Paper (2013).
30. J. D. Hawkins, E. Von Cleve, R. F. Catalano Jr., Reducing early childhood aggression: Results of a primary prevention program. *Journal of the American Academy of Child & Adolescent Psychiatry* **30**(**2**), 208-217 (1991).

31. J. D. Hawkins, R. F. Catalano, R. Kosterman, R. D. Abbott, K. G. Hill, Preventing adolescent health-risk behaviors by strengthening protection during childhood. *Archives of Pediatrics and Adolescent Medicine* **153**(**3**), 226-234 (1999).
32. J. D. Hawkins, R. Kosterman, R. F. Catalano, K. G. Hill, R. D. Abbott, Promoting positive adult functioning through social development intervention in childhood. *Archives of Pediatrics and Adolescent Medicine* **159**(**1**), 25-31 (2005).
33. J. D. Hawkins, R. Kosterman, R. F. Catalano, K. G. Hill, R. D. Abbott, Effect of social development intervention in childhood 15 years later. *Archives of Pediatrics and Adolescent Medicine* **162**(**12**), 1133-1141 (2008).
34. Conduct Problems Prevention Research Group, Fast Track intervention effects on youth arrests and delinquency. *Journal of experimental criminology*, **6**(**2**), 131 (2010).
35. K. L. Bierman, J. Coie, K. Dodge, M. Greenberg, J. Lochman, R. McMohan, E. Pinderhughes, School Outcomes of Aggressive-Disruptive Children: Prediction From Kindergarten Risk Factors and Impact of the Fast Track Prevention Program. *Aggressive behavior*, **39**(**2**), 114-130 (2013).
36. J. McCord, W. McCord. A follow-up report on the Cambridge-Somerville Youth Study. *Annals of the American Academy of Political and Social Science*, **322**(**1**), 89-96 (1959).
37. R. E. Tremblay, E. McCord, J. Boileau, P. Charlebois, C. Gagnon, M. LeBlanc, Can disruptive boys be helped to become competent? *Psychiatry*, **54(2),** 148-161 (1991).
38. R. E. Tremblay, F. Vitaro, L. Bertrand, M. Le Blanc, H. Beauchesne, H. Boileau, Parent and child training to prevent early onset of delinquency: The Montreal longitudinal experimental study. In J. McCord, R. E. Tremblay (Eds.), *Preventing antisocial behavior: Intervention from birth through adolescence* (Guilford Press, New York, 1992), 117-138.
39. R. E. Tremblay, L. Pagani-Kurtz, L. C. Mâsse, F. Vitaro, R. O. Pihl, A bimodal preventive intervention for disruptive kindergarten boys: its impact through mid-adolescence. *Journal of consulting and clinical psychology*, **63**(**4**), 560 (1995).
40. R. E. Tremblay, The development of aggressive behavior during childhood: what have we learned during the past century. *International Journal of Behavioral Development*, **24**, 129-141 (2000).
41. F. Vitaro, D. Larocque, M. Janosz, R. E. Tremblay, Negative social experiences and dropping out of school. *Educational Psychology* **21**, 401-415 (2001).
42. E. Lacourse, S. Cote, D. S. Nagin, F. Vitaro, M. Brendgen, R. E. Tremblay, A longitudinal-experimental approach to testing theories of antisocial behavior development. *Development and Psychopathology* **14,** 909-924 (2002).
43. N. Castellanos-Ryan, J. Séguin, F. Vitaro, S. Parent, R. E. Tremblay, A multimodal intervention for disruptive kindergarten children reduces substance-use across adolescence: a randomized control trial. *British Journal of Psychiatry*, forthcoming (2013).
44. F. Vitaro, M. Brendgen, S. Larose, R. E. Tremblay, Kindergarten Disruptive Behaviors, Protective Factors, and Educational Achievement by Early Adulthood. *Journal of educational psychology*, **97**(**4**), 617 (2005).
45. F. Vitaro, E. Barker, M. Brendgen, R. E. Tremblay, Pathways explaining the reduction of adult criminal behaviour by a randomized preventative intervention for disruptive kindergarten children. *The Journal of Child Psychology and Psychiatry* **53(7),** 748-756 (2012).
46. I. Lorge, R. A. Thorndike. *The Lorge-Thorndike Intelligence Tests*. New York, NY: Houghton Mifflin (1950).
47. J. J. Heckman, S. H. Moon, R. Pinto, P. A. Savelyev, A. Yavitz, The rate of return to the HighScope Perry Preschool Program. *Journal of Public Economics* **94(1)**, 114-128

(2010).
48. Statistics Canada, Labour Force Survey (2011).
49. I. Dhaliwal, E. Duflo, R. Glennerster, C. Tulloch, *Comparative cost-effectiveness analysis to inform policy in developing countries: a general framework with applications for education*. Cambridge, MA: MIT Press (2013).
50. E. Banfield, *The Moral Basis of a Backward Society*. New York: Free Press (1958).
51. R. Putnam *Bowling Alone: The Collapse and Revival of American Community*. New York: Simon & Schuster (2000).
52. Y. Algan, P. Cahuc, Trust, Growth and Well-Being: New evidence and policy implication. Handbook of Economic Growth, vol.1, eds S. Durlauf and P. Aghion. Amsterdam: Elsevier (2014).
53. R. Chetty, N. Hendren, P. Kline, E. Saez, Where is the land of opportunity? The geography of intergenerational mobility in the United States. *NBER Working Paper*, 19843 (2014).

References for Supplementary Material

54. R. Tremblay. The Montreal longitudinal and experimental studyIn R. Tremblay (Ed.), *The Montreal Longitudinal Experimental Study: The Intervention and its Long Term Effects*,. (available at *https://gripinfo.ca*) (2010).
55. R. E. Tremblay, L. Desmarais-Gervais, C. Gagnon, P. Charlebois, The Preschool Behavior Questionnaire: Stability of its Factor Structure Between Cultures, Sexes, Ages and Socioeconomic Classes. *International Journal of Behavioral Development* **10(4),** 467-484 (1987).
56. G. Cartledge, J. F. Milburn, *Teaching social skills to children. Innovative approaches* (Pergamon Press, New York, 1980).
57. P. W. Kettlewell, D. F. Kaush, The generalization of the effects of a cognitive behavioral treatment program for aggressive children. *Journal of Abnormal Child Psychology* **11**, 101-114 (1983).
58. L. Michelson, D. Sugai, R. Wood, A. E Kazdin, *Social skills assessment and training with children*. Plenum Press, New York (1983).
59. B. H. Schneider, B. M. Byrne, Individualizing social skills training for behavior disordered children. *Journal of Consulting and Clinical Psychology* **55,** 444-455 (1987).
60. B. Weiss, K. A. Dodge, J. E. Bates, G. S. Pettit, Some consequences of early harsh discipline: Child aggression and a maladaptive social information processing style. *Child Development* **63**, 1321-1335 (1992)
61. K. Dodge, J. Godwin, Conduct Problems Prevention Research Group, Social-information-processing patterns mediate the impact of preventive intervention on adolescent antisocial behavior. *Psychol Sci* **24(4)**, 456-65 (2013).
62. K. Dodge, Do social information-processing patterns mediate aggressive behavior? In B. B. Lahey, T. E. Moffitt, A. Caspi (Eds), *Causes of conduct disorder and juvenile delinquency*. New York, NY, US: Guilford Press (2003)
63. B. W. Camp, G. E. Blom, F. Hebert, W. J. van Doorminck, Think aloud: A program for developing self-control in young aggressive boys. *Journal of Abnormal Child Psychology* 5, 157-169 (1977).
64. A. P. Goldstein, R. P. Sprafkin, N. J. Gershaw, P. Klein, The adolescent: Social skills training through structured learning. In G. Cartledge & J. F. Milburn (Eds.), *Teaching social skills to children: Innovative approaches* (Pergamon Press, New York, 1980) 249-277.
65. G.R. Patterson, *Coercive family process*. Castalia, Eugene, Oregon (1982).
66. G.R. Patterson, J. B. Reid, R. R. Jones, R.W. Conger, *Families with aggressive children*

Volume 1: A social learning approach to family intervention. Castalia, Eugene, Oregon (1975).
67. L. Bertrand, D. Reclus-Prince, Social Skills Training. The Montreal Longitudinal and Experimental Study: The Intervention Program and its Long Term Effects. In R. Tremblay (Ed.), *The Montreal Longitudinal Experimental Study: The Intervention and its Long Term Effects*, 51-74. (available at https://gripinfo.ca).
68. K. Bessell, U. Palomares, *Programme de développement affectif et social: guide pratique de niveau pré-scolaire*. Québec: Institut de développement humain (1972).
69. M. B. Shure, G. Spivack, *A mental health program for kindergarten children: Training script*. Philadelphia, PA: Hanheman Medical College (1984).
70. C. F. Jesness, R. F. Wedge, *Classifying offenders: The Jesness inventory classification system* (California Department of Youth Authority, Program Research and Review Division, 1983).
71. M. Kovacs, *The Children's Depression Inventory: A self-rated depression scale for school-aged youngsters*. University of Pittsburgh School of Medicine, Department of Psychiatry, Western Psychiatric Institute and Clinic (1983).
72. H.W. Marsh, A multidimensional, hierarchical model of self-concept: Theoretical and empirical justification. *Educational Psychology Review* **2(2),** 77-172 (1990).
73. M. Rosenberg, Rosenberg self-esteem scale (RSE). *Acceptance and Commitment Therapy. Measures Package* **61** (1965).
74. R. E. Tremblay, F. Vitaro, C. Gagnon, A Prosocial Scale for the Preschool Behavior Questionnaire: Concurrent and Predictive Correlates. *International Journal of Behavioral Development* **15(2),** 227-245 (1992).
75. American Psychiatric Association, *The Diagnostic and Statistical Manual of Mental Disorders: DSM 5* (2013).
76. M. Maric, R.W. Wiers, P. J. M. Prins, Ten ways to improve the use of statistical mediation analysis in the practice of child and adolescent treatment research. *Clin Child Fam Psychol Rev* **15,** 177-191 (2012).
77. D.P. MacKinnon, C.M. Lockwood, J. Williams, Confidence limits for the indirect effect: distribution of the product and resampling methods. *Multivariate Behavioral Research* **39**(**1**) 99-128 (2004).
78. Ministry of Justice, Quebec (2008) available at http://www.justice.gc.ca/.
79. Direction des statistiques et de l'information décisionnelle, Indicateurs de Gestion 2012-2013, Ministère de l'Éducation, du Loisir et du Sport, Quebec.
80. R. J. Sampson, J. H. Laub, Life-Course Desisters? Trajectories Of Crime Among Delinquent Boys Followed To Age 70. *Criminology*, **41**(**3**), 555-592 (2003).

Acknowledgments:

We thank Roland Benabou, Raj Chetty, Bruno Crépon, Angus Deaton, Esther Duflo, Armin Falk, Ernst Fehr, Nicole Fortin, Karla Hoff, Larry Katz, Rachel Kranton, Jens Ludwig, Mark Stabile, Philip Oreopoulos, Bob Putnam, Jean Marc Robin and Emmanuel Saez for very helpful comments. We also thank participants at the NBER Education and Crime Summer Institute 2014, NBER Political Economy workshop and seminars at Harvard, Princeton, Stockholm and Zurich University. All the data from the MLES experiment are located at GRIP: Groupe de recherche sur l'inadaptation psychosociale chez l'enfant, Hopital Sainte-Justine, Montréal Canada. The research leading to these results has received funding from the European Research Council for Yann Algan under the European Community's Seventh Framework Program (FP7/2007-2013) / ERC grant agreement n° 240923. Elizabeth Beasley gratefully acknowledges the support of the Abdul Latif Jameel Poverty Action Lab (J-PAL). The experimental intervention and the follow-up assessments were funded by grants from the Canadian Institutes of Health Research, the Social Sciences and Humanities Research Council of Canada, the Québec Research Funds for Health and the Québec Research Funds for Society and Culture.

Figure 1. Non-cognitive skills and school performance during adolescence. A, B and C show distributions for non-cognitive skills measured in early adolescence for the control, treatment and non-disruptive groups (the non-disruptive boys being those who were not disruptive in kindergarten and did not participate in the experiment as treatment or control: they serve as a normative population baseline). Kolmorgorov-Smirnov test for equality of Treatment and Control distributions gives p-value of 0.003 for Trust, 0.036 for Aggression Control, and 0.023 for Attention-Impulse Control. D shows the increasing gap in the percent of subjects held back at each age. P-value from χ^2 test between Treatment and Control groups is 0.60 at age 10 and 0.01 at age 17.

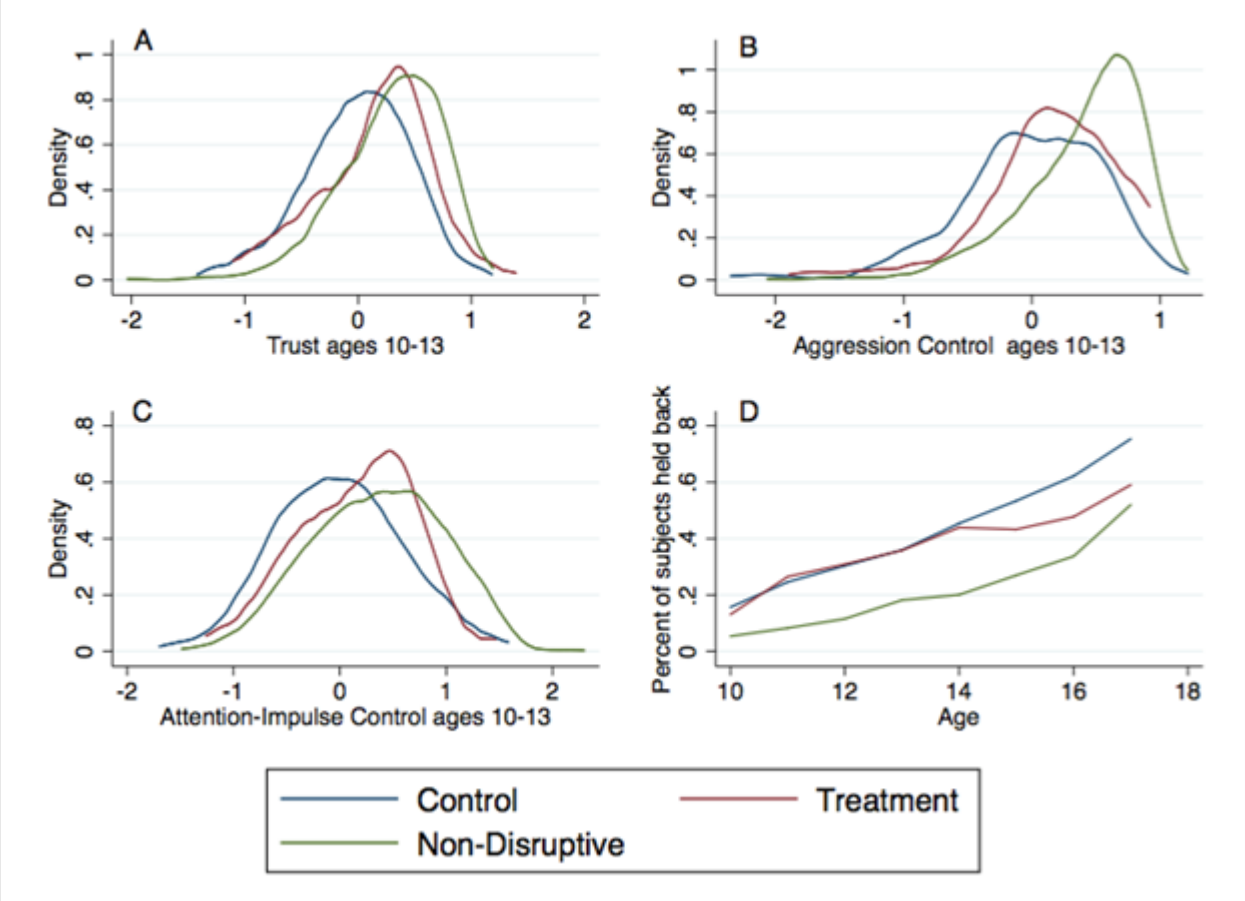

Figure 2. Young Adult Outcomes. As young adults, treatment subjects commit fewer crimes, are more likely to graduate from secondary school, are more likely to be active fulltime in school or work, and are more likely to belong to a social or civic group. The intervention closed part or all of the gap between boys ranked as disruptive in kindergarten but not treated (the control group) and the non-disruptive boys (who represent the normative population). Raw differences are significant for secondary diploma (p-value=0.04) and group membership (p-value=0.05), conditional differences (controlling for group imbalances) are significant for number of crimes (p-value=0.09) and percent active fulltime (p-value=0.03).

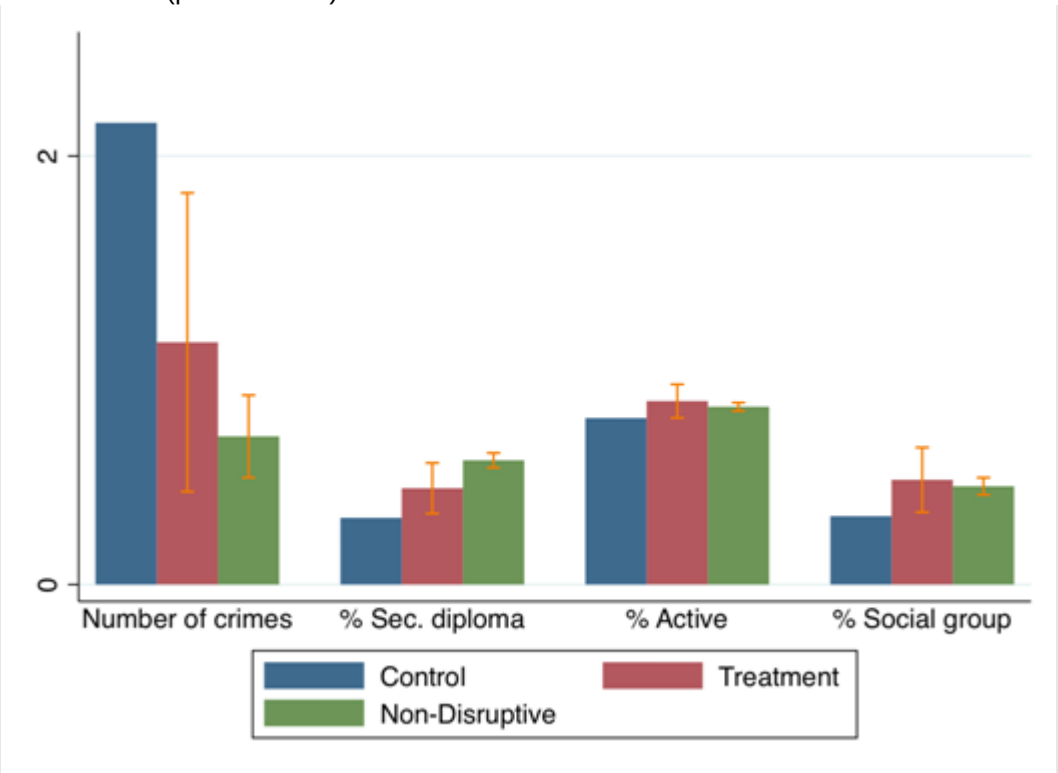

Figure 3. School achievement explained by IQ and non-cognitive skills. The non-cognitive skills measured in this paper explain a higher proportion of school performance than IQ. The bars plot the adjusted R-squared from uncontrolled OLS regressions of IQ or non-cognitive skills (Trust, Aggression Control, and Attention-Impulse Control), or both, on different measures of school achievement.

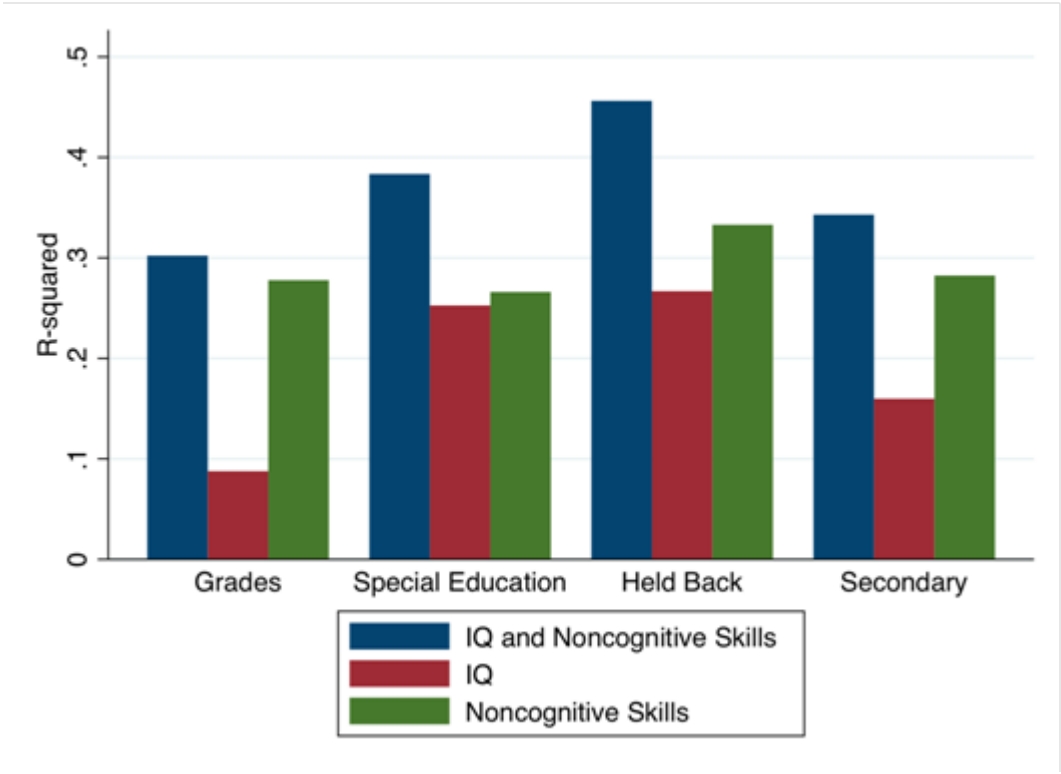

Figure 4. Proportion of impact on Grades and Young Adult Outcomes explained by Aggression Control, Attention-Impulse Control, and Trust. Increases in non-cognitive skills explain a substantial portion of the impact on several outcomes. Calculated percentages and p-values presented in Supplementary materials section F.

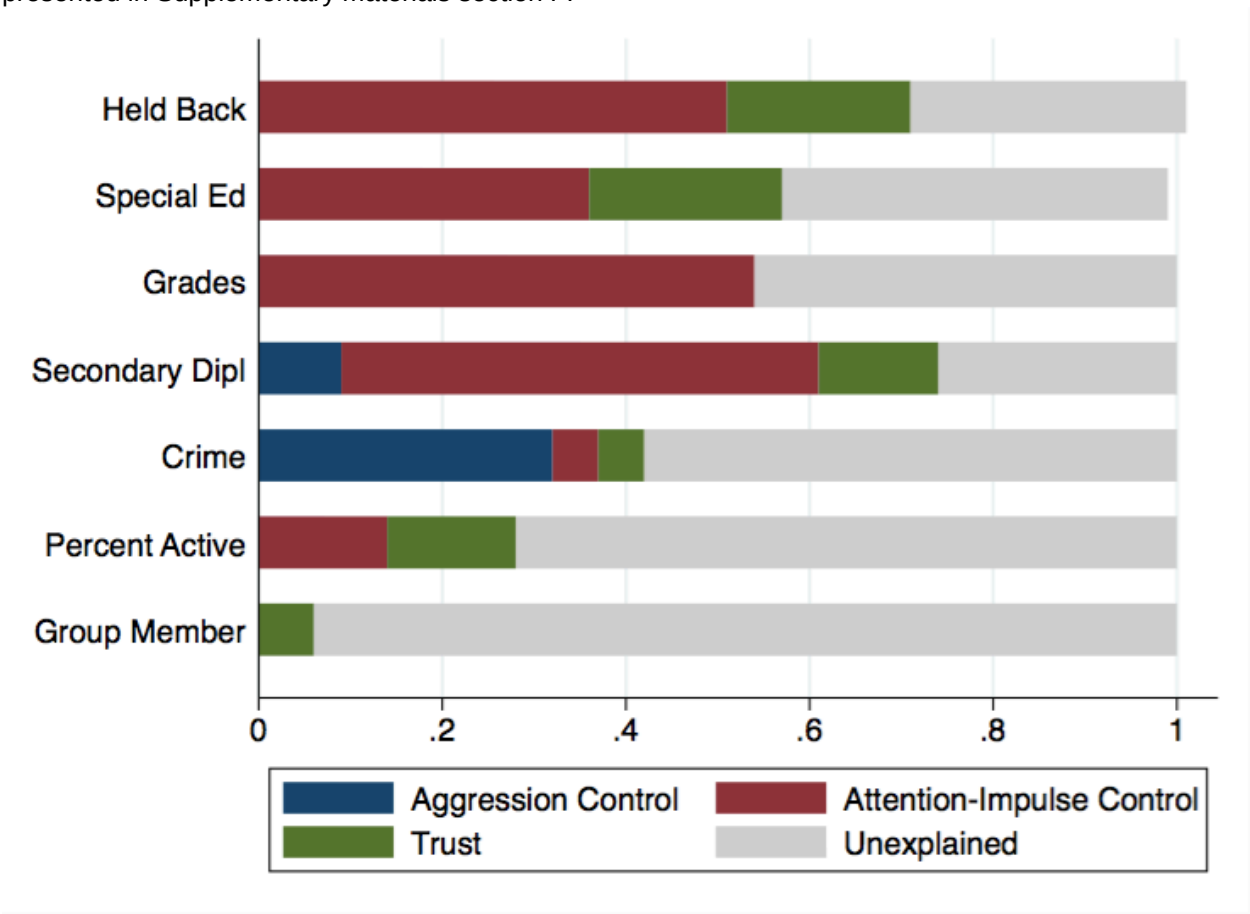

Table 1. Impact of treatment on adolescent outcomes. Each cell of column (1) gives the p-value for the raw difference between the treatment and the control group for each of the outcomes (in rows). Each cell of columns (2)-(8) give the regression coefficient of the treatment dummy variable on each of the potential mechanisms (in rows). Columns (2)-(7) include bootstrapped standard errors (with 2000 draws) in parentheses. Column (8) includes robust standard errors in parentheses. *** p<0.01, ** p<0.05, *p<0.10. Columns (1)-(3) and (9) use data from the disruptive sample only. Columns (4)-(8) use data from the entire sample. Columns (1), (2), (4), and (9) include no controls. Columns (3) and (5)-(8) include controls for imbalances between the treatment and control groups. Column (8) uses inverse probability weighting to adjust for attrition under certain assumptions. The p-values in column (9) are from a permutation test of the difference of the means (where the permuted value is treatment group) with 2000 repetitions. For details on specifications, see Supplementary materials section D.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	P-value of raw difference in means (t-test, Disruptive Sample)	Treatment Effect: Disruptive Sample	Conditional Treatment Effect: Disruptive Sample	Treatment Effect: Full Sample	Conditional Treatment Effect: Full Sample	Conditional Treatment effect with full sample, including clusters	Conditional Treatment effect with full sample, including Fixed Effects	Conditional Treatment effect with full sample, including IPW	P-value of raw difference in means (permutation test, Disruptive Sample)
Ages 10-13									
Trust	0.022	0.156**	0.181***	0.156**	0.173**	0.173**	0.177**	0.168**	0.021
		(0.0741)	(0.0700)	(0.0692)	(0.0724)	(0.0787)	(0.0705)	(0.0698)	
Agg. Control	0.054	0.154*	0.149*	0.154**	0.147*	0.147*	0.146*	0.143*	0.051
		(0.0796)	(0.0803)	(0.0782)	(0.0808)	(0.0764)	(0.0759)	(0.0783)	
Attn-Imp Control	0.058	0.160*	0.177**	0.160**	0.192**	0.192***	0.179**	0.188**	0.065
		(0.0819)	(0.0840)	(0.0786)	(0.0802)	(0.0635)	(0.0776)	(0.0826)	
IQ	0.931	-0.0319	0.188	-0.0319	0.156	0.156	0.261	0.201	0.952
		(0.398)	(0.379)	(0.393)	(0.393)	(0.376)	(0.380)	(0.382)	
Grades	0.419	0.113	0.169	0.113	0.188	0.188	0.172	0.199	0.403
		(0.144)	(0.147)	(0.143)	(0.138)	(0.127)	(0.155)	(0.144)	
Special Ed	0.951	-0.00290	-0.0316	-0.00290	-0.0227	-0.0227	-0.0174	0.00117	0.953
		(0.0472)	(0.0477)	(0.0463)	(0.0475)	(0.0489)	(0.0460)	(0.0744)	
Held Back	0.956	-0.00294	-0.0446	-0.00294	-0.0318	-0.0318	-0.0342	0.00518	0.957
		(0.0517)	(0.0528)	(0.0513)	(0.0531)	(0.0483)	(0.0557)	(0.0790)	
Ages 14-17									
Trust	0.0422	0.176**	0.199**	0.176**	0.176**	0.176*	0.200**	0.174**	0.040
		(0.0840)	(0.0865)	(0.0809)	(0.0807)	(0.0919)	(0.0797)	(0.0816)	
Agg. Control	0.038	0.186**	0.170**	0.186**	0.176**	0.176*	0.200**	0.175**	0.044
		(0.0867)	(0.0864)	(0.0773)	(0.0811)	(0.103)	(0.0850)	(0.0814)	
Attn-Imp Control	0.654	0.0412	0.0113	0.0412	0.0425	0.0425	0.0623	0.0480	0.646
		(0.0888)	(0.0872)	(0.0857)	(0.0892)	(0.0725)	(0.0887)	(0.0887)	
Grades	0.104	0.219*	0.283**	0.219*	0.292**	0.292**	0.310**	0.315**	0.098
		(0.127)	(0.132)	(0.131)	(0.127)	(0.119)	(0.124)	(0.127)	
Special Ed	0.108	-0.0989*	-0.143**	-0.0989*	-0.139**	-0.139**	-0.152**	-0.137**	0.108
		(0.0599)	(0.0656)	(0.0592)	(0.0608)	(0.0544)	(0.0636)	(0.0608)	
Held Back	0.117	-0.0963	-0.148**	-0.0963	-0.144**	-0.144**	-0.150**	-0.0218	0.124
		(0.0656)	(0.0582)	(0.0657)	(0.0594)	(0.0646)	(0.0674)	(0.0860)	

Table 2. Impact of treatment on young adult outcomes. Each cell of column (1) provides the p-value for the raw difference between the treatment and the control group for each of the young adult outcomes (in rows). Each cell of column (2) gives impact of treatment (in marginal effect) from a logit specification. Each cell of columns (3)-(9) give the OLS regression coefficient of the treatment dummy variable on each of the outcomes (in rows). Columns (2)-(8) include bootstrapped standard errors (with 2000 draws) in parentheses. Column (9) includes robust standard errors in parentheses. *** p<0.01, ** p<0.05, *p<0.10. Columns (1)-(4) and (10) use data from the disruptive sample only. Columns (5)-(9) use data from the entire sample. Columns (1), (2), (3), (5), and (10) include no controls. Columns (4) and (6)-(9) include controls for imbalances between the treatment and control groups for all outcomes and baseline variables correlated to treatment and attrition for economic and social capital outcomes (see Supplementary materials section B). Column (9) uses inverse probability weighting to adjust for attrition under certain assumptions. The p-values in column (10) are obtained from a permutation test of the difference of the means (where the permuted value is treatment group) with 2000 repetitions. For details on the specifications, see Supplementary materials section D.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	P-value of raw difference in means (t-test, Disruptive Sample)	Logit Treatment Effect: Disruptive Sample	Treatment Effect: Disruptive Sample	Conditional Treatment Effect: Disruptive Sample	Treatment Effect: Full Sample	Conditional Treatment Effect: Full Sample	Conditional Treatment effect with full sample, including clusters	Conditional Treatment effect with full sample, including Fixed Effects	Conditional Treatment effect with full sample, including IPW	P-value of raw difference in means (permutation test, Disruptive Sample)
Number of crimes 18-23	0.162		-1.024*	-1.272*	-1.024*	-1.091*	-1.091*	-1.147*		0.176
			(0.557)	(0.668)	(0.553)	(0.582)	(0.629)	(0.599)		
Secondary school diploma	0.0409	0.138*	0.138**	0.176***	0.138**	0.186***	0.186**	0.190***		0.052
		(0.0717)	(0.0684)	(0.0680)	(0.0685)	(0.0676)	(0.0753)	(0.0683)		
Economic activity and social capital										
% of years 17-27 active	0.127		0.0791*	0.126*	0.0791*	0.124**	0.124***	0.127**	0.114*	0.133
			(0.0474)	(0.0704)	(0.0473)	(0.0516)	(0.0464)	(0.0533)	(0.0583)	
% of years 17-27 working	0.056		0.0984**	0.121**	0.0984**	0.120**	0.120***	0.121**	0.0992*	0.064
			(0.0473)	(0.0600)	(0.0465)	(0.0498)	(0.0387)	(0.0531)	(0.0532)	
% of years 17-27 transfers	0.413		-0.0349	-0.0395	-0.0349	-0.0659*	-0.0659*	-0.0701*	-0.0551	0.401
			(0.0378)	(0.0427)	(0.0374)	(0.0352)	(0.0349)	(0.0372)	(0.0411)	
Employed at age 27	0.169	0.103**	0.103*	0.105*	0.103*	0.111*	0.111*	0.130**	0.130**	0.295
		(0.0469)	(0.0567)	(0.0562)	(0.0567)	(0.0590)	(0.0572)	(0.0627)	(0.0598)	
Employed fulltime at age 27	0.083	0.142***	0.142**	0.152**	0.142**	0.154**	0.154**	0.160**	0.166***	0.105
		(0.0495)	(0.0585)	(0.0682)	(0.0597)	(0.0634)	(0.0645)	(0.0667)	(0.0641)	
Hourly wage at age 27	0.277		1.829	1.802	1.829	2.737*	2.737**	2.862*	2.799*	0.264
			(1.469)	(1.704)	(1.460)	(1.585)	(1.314)	(1.645)	(1.512)	
Hours worked / week at age 27	0.048		6.431**	6.965**	6.431**	6.912**	6.912**	7.545***	7.249***	0.050
			(2.633)	(2.756)	(2.667)	(2.785)	(2.787)	(2.677)	(2.744)	
Member of group (21 or 27)	0.049	0.169*	0.169*	0.150*	0.169*	0.224***	0.224***	0.195**	0.209**	0.062
		(0.0887)	(0.0886)	(0.0885)	(0.0897)	(0.0826)	(0.0803)	(0.0848)	(0.0862)	
Member of group (16 or 17)	0.003	0.105**	0.105**	0.0870*	0.105**	0.100**	0.100**	0.0956**	0.100**	0.008
		(0.0494)	(0.0469)	(0.0493)	(0.0469)	(0.0482)	(0.0446)	(0.0463)	(0.0471)	