# The Impact of Childhood Social Skills and Self-Control Training on Economic and non-Economic Outcomes: evidence from a randomized experiment using administrative data

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*A childhood intervention to improve the social skills and self-control of at-risk kindergarten boys in the 1980s had positive impacts over the life course: higher trust and self-control as children; increased social group membership, education, and reduced criminality as young adults; and increased marriage and employment as adults. Using administrative data, we find this intervention increased average yearly employment income by about 20% and decreased average yearly social transfers by almost 40%. We estimate that $1 invested in this program around age 8 yields about $11 in benefits by age 39, with an internal rate of return of around 17%.*

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While observational evidence shows a strong association between non-cognitive skills in childhood and favorable adult outcomes (Borghans et al. 2008, Almlund et al. 2011, Duckworth et al. 2012, Vergunst et al. 2019), no controlled study has yet demonstrated that a program targeted exclusively to non-cognitive skills has an impact on adult outcomes, and, in particular, on economic outcomes. By using administrative tax return data, this paper estimates the long-term impact on both labor market and non-economic outcomes of an intervention at school entry (age 7-9) that aimed reduce behavior problems by improving self-control and social skills for boys from low-income neighborhoods in urban Montreal in the 1980s. At-risk boys who were participants in the Montreal Longitudinal Experimental Study (MLES) were randomly assigned to either a treatment group, which was invited to participate in a two-year long social skills and self-control training program, or a control group, which did not have access to the training but still had access to standard programs and resources available to Montreal public school children. During the training, boys were coached on social skills and self-control, covering topics such as how to invite another child to play with you, how to ask "why", and what to do when you get angry.

We begin by evaluating how this intervention impacted non-cognitive skills as adolescents and young adults. Researchers collected detailed longitudinal data from these cohorts on socio-emotional development during childhood, adolescence, and young adulthood. While previous studies focused on aggression trajectories (Lacourse et al. 2002, Vitaro et al. 1999, 1999, 2012, 2013), we pursue a more agnostic and comprehensive approach in this study. We construct new behavior scales to measure the initial and later impact of the intervention on all the observed non-cognitive and cognitive skills from ages 10 to 17. Using intention to treat estimates, we find large impact of the intervention on the two main non-cognitive skills targeted by the training program: "self-control" skills (Aggression Control[1] and Attention Control) and "pro-social" skills (Trust), but no impact

[1] In this paper we use capitalized terms ("Trust" and "Self-Esteem", for example) to distinguish the skills estimated and used econometrically in this paper from the general concepts ("trust" and "self-esteem").

on other non-cognitive skills such as Self-Esteem, Altruism or Friendliness. The intervention had no impact on IQ, and no initial impact on grades or school performance, which is consistent with the nature of the program. However, during the late adolescent period, the treatment students did have improved grades, reduced grade repetition, and reduced special education class assignment, suggesting that the initial boost in non-cognitive skills led to positive impact on school performance. By using self-reported survey in young adulthood (age 20 and 24), we also find that treatment subjects were more likely to report be members of a social group, which is consistent with the increase in social skills during adolescence.

We then estimate the impact of the intervention on long-term labor market outcomes as well as available non-income outcomes (marriage, household composition, group membership, charitable donations, tuition expenditure). Measuring long-term impacts through surveys, especially economic and financial impacts, is difficult due to attrition and problems with recall[2], and consequently reliable estimates are rare.[3] In partnership with Statistics Canada, we matched subjects to tax returns from ages 20-39 and find large and significant positive impacts on marriage, contributions employment insurance and professional groups, and employment and income. The treatment raised annual income from employment by $ 5,708 CAD per year from ages 20-39, equivalent to a 20% increase. In addition to individual labor market returns, we find a reduction in social transfers to the treatment group. On average, the control group benefited from social transfers of some kind for 3.9 years during the 20-year period, whereas the treatment group received social transfers for 2.8 years on average (the respective average annual amounts of transfers were $ 2,436 CAD in the control group and $ 1,507 CAD in the treatment group). The treatment group was also more likely to be married. These results are robust to several specifications.

[2] For example, in this study, by age 26 about half of the original MLES sample could not be contacted.

[3] 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 (Kautz et al. 2014). 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 (Durlak et al. 2011).

We provide an estimate of the dollars in benefit compared to dollars in cost and estimate the internal rates of return (IRR) of the program using our results on economic outcomes as well as previous estimates on school completion and criminality (Boisjoli et al. 2007; Vitaro et al. 2012). We estimate that $1 invested in this program yields about $11 in benefits by age 39, with an IRR of 17%. Even when income effects are excluded and only reduced taxpayer expenditures (reduced crime, additional education spending and social transfers as adults) are considered, the program's benefits equal its costs by the time individuals are in their mid-20s.

This study makes several contributions to the existing literature on the long-run impact of childhood interventions.[4] First, while several recent shorter-term evaluations of programs that target non-cognitive skills have had encouraging results, there is little evidence on the long-term impact of non-cognitive skill interventions on adult earnings and economic outcomes. Recent studies, focused on growth mindset and goal-setting (Dobronyi et al. 2019, Alan et al. 2019, Yeager et al. 2019), emotional and social competence (Domitrovich et al. 2007; Conduct Problems Prevention Research Group 2010)[5], prosociality (Kosse et al. 2019), automaticity during high school (Heller et al. 2017) or patience in early adulthood (Blattman et al. 2017), have shown promising short- to medium-term effects (on grades or crime).[6] This evidence base still needs long-term follow-ups to assess whether the effects persist into adulthood, or if new effects may only become apparent in adulthood. One longitudinal randomized evaluation that shares some

[4] While several studies show the relationship of non-cognitive skills to adult economic outcomes, many studies are based on non-experimental longitudinal studies and cannot address the issue of causality (see Duckworth 2011, and Moffitt et al. 2011, for surveys on the role of self-control; and Duckworth and Seligman 2005, Diamond et al. 2007, and Pingault et al. 2014, for the relationship of self-control to academic achievement in non-experimental studies). Recent studies also find an association between trust and income at the individual level from social surveys (Butler, Guiliano and Guiso 2016).

[5] The PATHS (Promoting Alternative Thinking Strategies) model and curriculum have been tested using several methodologies and in a variety of contexts (see Stanley 2019), but long-term impacts have not been measured.

[6] Many of the previous longitudinal experiments focused on the early-childhood period (see Almond and Currie 2010, Heckman 2006 for early childhood as critical period for the formation of cognitive skills and the meta-analysis of childhood programs by Kautz et al. 2014 and Durlak et al. 2011). Several recent studies, including Blattman et al. 2017 and Heller et al. 2017 show that the window of intervention may be wider than previously thought, and our paper supports this finding.

characteristics with the MLES is the Cambridge-Somerville Youth Study, where at risk boys were given tutoring and medical care, as well as interventions supportive of non-cognitive skills (for example, Boy Scouts) but not specific non-cognitive skill training. Long term follow up was mostly self-reported and focused on health and criminality, and there was no long-run evaluation on adult economic outcomes.[7] The long-run administrative data is of critical importance to demonstrate economic impact and allows us to compare the benefits to the costs of the program.

Second, the well-known interventions that do have long term follow-ups on adult economic outcomes, and which have been critical to generating support for early intervention, are not able to disentangle the impact of targeting non-cognitive skills versus other skills. This is because of the nature of the interventions: they simultaneously fostered cognitive skill development, for example the Abecedarian program (Campbell et al. 2002, Campbell et al. 2014) and Head Start (Currie and Duncan 1995, Ludwig and Miller 2007); or combined non-cognitive training with cognitive training or tutoring, for example the Perry Preschool Program (Heckman et al. 2010), the Fast Track Program (Bierman et al. 2013), or Project STAR (Chetty et al. 2011); or included health intervention, for example the Nurse-Family Partnership (Howard and Brooks-Gunn 2009) and the Jamaican Supplementation Study (Gertler et al. 2014).[8]

Third, there is a direct contribution with respect to the MLES program itself. While previous studies focused on secondary school completion and criminality (Boisjoli et al. 2007; Vitaro et al. 2012), this paper provides evidence that the program has large and persistent effects on both labor market outcomes, increasing employment income for participants and reducing their dependence on social transfers, and positive impacts on

[7] This program had a negative impact on participants at age 30 on juvenile crime (Hawkins et al. 1991, Hawkins et al. 2008, McCord and McCord 1959). Children with behavioral problems were grouped together for treatment, and this may have given rise to the stigma or negative influence of peers that led to the unintended negative consequences. The fact that the disruptive children were mixed with pro-social peers might thus be an important element of the MLES. Other possible sources of the negative impact are that the counselors introduced middle-class values that did not m lived experience, or over-dependence on the counselors, so that when the program ended the treatment subjects were left adrift.

[8] Several researchers have presented evidence that much of the large benefit of early childhood interventions is likely to be due to improvements in skills that are not measured by grades or IQ tests – suggesting a very important role for non-cognitive skills (Heckman et al. 2013; Chetty et al. 2011).

non-economic outcomes such as marriage, participation in employment insurance and professional groups during adulthood. We also provide an overall cost-benefit analysis of the program. Finally, we discuss the impact on cognitive and non-cognitive skills during adolescence and young adulthood to interpret these findings, while previous studies focused on aggression trajectories.

This paper is organized as follows. Section 2 provides background on the MLES, the experimental design and data. Section 3 analyses the impact of the MLES program on adult economic outcomes. Section 4 provides the cost benefit analysis, and Section 5 concludes.

## I. The Montreal Longitudinal Experimental Study (MLES)

### *A. Experimental Design and Timeline*

Figure 1 shows the timeline of the experiment and data collection. Kindergarten teachers of 53 schools in low SES areas of Montreal, Canada, were asked to rate the behavior of their male students at the end of the 1984 school year with the Social Behavior Questionnaire (Tremblay et al. 1987). Almost all (87%) of the teachers provided ratings for a total of 1,161 boys. To create a homogenous sample, only participants whose parents were Canadian born with French as a first language and 14 years or less of schooling were included in the longitudinal study, which reduced the number to 1037 boys. The setup of the MLES is summarized in Tremblay et al. (2003) and McCord et al. (1994).

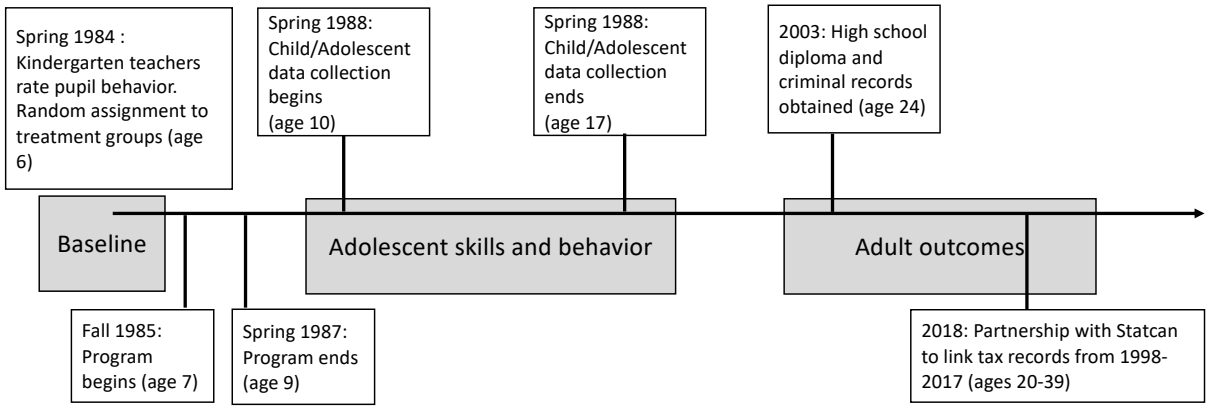


FIGURE 1. TIMELINE OF THE EXPERIMENTAL STUDY

*Notes:* Figure shows the timeline for the MLES study including experimentation, data collection, and matching with Statistics Canada administrative data.

The disruptiveness scale of the Social Behavior Questionnaire was used to identify the at-risk boys for the intervention. The scale measured the frequency of physical aggression, oppositional behavior, and hyperactive behavior (Cronbach alpha = 0.93). Boys with a score above the 70th percentile for that sample of kindergarteners (N = 250) on this disruptiveness scale were considered to be at high risk of later antisocial behavior (Vitaro et al. 2005). Boys with a score below the 70$^{th}$ percentile are considered "Non-disruptive" and are used as a reference group in this paper. The 250 participants who formed the "Disruptive" group were randomly assigned to a treatment (69 boys) and a control group (181 boys) by drawing names from a box.

### *B. Intervention Program*

The intervention program was implemented over a 2-year period, from ages 7 to 9 (Grades 2 and 3). The main element of the intervention consisted of direct training on social skills and self-control to children. The experiment drew on randomized and non-randomized studies of children on emotional regulation and impulse control, social-information processing and how to interpret other's intent (Cartledge and Milburn 1980; Kettlewell and Kausch 1983; Michelson et al. 1983; Schneider and Byrne 1987; Weiss et al. 1992; Dodge 2003, 2013). The training sessions were conducted at school (outside the classroom), in groups of four to seven children, of which one or two would be the treatment participants, and the rest would be boys identified by their teachers as highly pro-social. This arrangement was intended to provide positive role models for the treatment participants and avoid stigmatizing the treatment participants. The sessions were held once a week for 45 minutes, during lunch or after school. There was also a teacher training component and a parent component, presented in detailed in Appendix G, but as discussed below, it is unlikely that these elements were major drivers of impact.

During the first year, nine sessions of social behavior training were offered.

Sessions included topics such as how to invite a bystander to play, how to ask "why", how to give a compliment, and how to help. The second year included 10 sessions of self-control strategies (Camp et al. 1977; Goldstein et al. 1980). Some stimulus situations for these sessions were how to react to teasing, how to react when angry, and what to do if other children refuse to play with you. For each situation, the children reviewed ways to define the problem, identified the intentions of the other person (perspective taking abilities), analyzed their feelings if they were in the role of the victim, suggested different action plans to solve the problem, anticipated their consequences, selected one action plan and, finally, gave positive reinforcement to themselves for their work.[9]

Verbal instructions, coaching, modeling, behavior rehearsal, and positive (verbal and material) reinforcement were all used. Children were encouraged to use their newly learned skills before the next training session. At the following meeting, the children were congratulated for having performed their new skills in the interim. Teachers and parents were informed through one-page letters of the new skills learned by the children during each session. They were encouraged to praise the child for using these new skills as often as possible.

Two full-time university-trained child-care workers, one a psychologist and one a social worker carried out the training and support activities. The team was coordinated by a fifth professional who worked on the project half time. See Appendix A for a more detailed explanation of the program, as well as Tremblay et al. (1992).

[9] For example, one session covered Self Control. The facilitator introduced the topic and talked about situations where children would be upset and might make an angry outburst, like a spilled glass of milk or some other kind of disappointment. The facilitator then modeled a situation: he has been playing tag, and he just got tagged and is now out. He's upset because he is the first person to be tagged out, and he's angry and disappointed. The facilitator demonstrates how children can respond in this situation: noticing clues in his body that he is going to lose control (clenching fists, feeling hot), he thinks about what happened to make him feel this way (he got tagged first, is worried other kids will laugh at him), he chooses a way to avoid making an angry outburst (count to ten, move away, say to himself "calm down", breathe), and then he acts and praises himself. The facilitator then invites children to perform additional role-plays based at school (one child bumps another's desk and their pen falls), at home (someone suddenly turns off the TV because it's time for dinner) or while playing (a friend takes a ball that was dropped). Together, the group makes observations about what the actors are doing, how they are following the steps, and gives feedback. At the end of the session, the facilitator fills out a workbook with the children to explain how they can practice self-control until the next session ("homework").

### *C. Data*

**Adolescent data:** Measures of different skills in adolescence were collected from ages 10 and 17 years. Previous studies on this sample have examined measures of disruptiveness and have found that participants are on different trajectories of disruptive behavior (Tremblay et al. 1991, 1995, Vitaro et al. 2012). We pursue a more agnostic and comprehensive approach in this study. We investigate the initial and later impact of the intervention on all the observed non-cognitive and cognitive skills by exploiting all the measures for which we have balanced observations every year from ages 10 to 17. We break this period into early and late adolescence and analyze data that are available from both periods: early adolescence (ages 10 to 13, or 1988-1991), and late adolescence (ages 14 to 17, or 1992-1995). The year 1992 was chosen as the break year because it is in this year that the treatment and control groups begin to diverge in rates of grade repetition.

Our identification of skills is based on exploratory factor analysis using the entire ELEM sample (1037 subjects), though results do not differ when the treatment group of 69 subjects is excluded, making it unlikely that treatment assignment is driving the identification of the skills. Combining all data available and averaging over the years available, we use factor analysis to examine how the factors combine into groups potentially measuring the same latent variable. We include the original questions that were used to identify the at-risk disruptive sub-sample, which were repeated each year, and questions from several well-known psychological inventories (Jesness and Wedge 1983; Kovacs 1983; Marsh 1990; Rosenberg 1965; Lacourse et al. 2002; Tremblay et al. 1992). We use individual questions from the different measuring instruments rather than the original scales themselves. This approach allows for the possibility that individual questions might cluster together effectively and allows subject-reported and teacher-reported data to be used together when possible.[10] In fact, all the skills that we identify,

[10] While there are likely to be discrepancies between teacher and student reports, we still expect them to be related when they are measuring the same trait and, moreover, to provide different points of data collection that cannot be obtained from the same informant (that is, the subjects can tell us how they feel and what they do, while the teachers can tell us how they behave in the class). If this is not the case – and there is evidence of discrepancies between the teacher and student reported data - then sensible groupings of variables from these two sources will not emerge.

except for Altruism, include both teacher and student reported variables that have sufficiently high alphas when grouped together. Full details for each skill are given in Appendix D.

We identify two skills that are related to self-control, based largely on the behavioral dimensions used to identify the at-risk 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 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, Friendliness, Altruism and Self-Esteem.

*Trust* measures generalized trust: it includes variables on whether the subject trusts others, strangers, the police and teachers or whether it is better not to trust anyone (self-reported). This measure also includes beliefs about the trustworthiness of others, with variables on whether the subject assumes that a bump from another child is intentional (self and teacher reported) and is inconsiderate of others (teacher-reported). This measure of Trust is in line with the recent literature defining trust as a belief about cooperation outside the inner family circle, to be distinguished from social networks (see the synthesis by Algan and Cahuc 2014, and the seminal works by Banfield 1958 and Putman 2000). *Friendliness* measures close relationships with family and friends. It includes variables on whether the subject gets advice from his best friend, cares about whether other people like him, and how much he spends time with friends. This social skill is more related to the ability to build social networks in the social capital literature, and in fact, displays a low correlation with *Trust* (see Table S 12 in Appendix D). *Altruism* measures voluntary altruistic and compassionate behavior (whether the subject tries to stop others from fighting, invites a child who is left out to play, helps injured children, volunteers to put things away, congratulates others, shows sympathy), and *Self-Esteem* measures feelings of value and self-worth.

For cognitive skills and school performance, verbal IQ was tested when the subjects were around 13 years old using the Sentence Completion Test (Lorge 1950), and we have

data on yearly grades in Math and French, as well as whether the subject had repeated a grade or was assigned to a special education class each year.

The correlation matrix for the different skills is given in Table S 12 of the Appendix D. In general, the skills are not highly correlated, and correlation varies over time, for example the correlation of Altruism to Trust is 0.11-0.19 and the correlation of Friendliness to Trust is 0.06-0.23. Conceptually, these three skills fall under the umbrella of pro-social skills, but they are not derived from the same scale, nor are they measuring the same behavior. The highest correlations are between Trust and Aggression Control (0.6) and Self-Esteem and Attention Control (0.6).

**Young adult data:** When participants were 23 and 28 years old (2001 and 2006), the MLES administered questionnaires with detailed economic and social questions. In order to maximize sample size and simplify the presentation of the data, we focus on data that are available in both years and take the average over the two waves when possible (percent of years employed or in school fulltime, percent of years receiving social transfers, and group membership). We include two variables that are available only one year: voting and volunteering. While we rely on the tax data for the estimates of the economic impact of the MLES, we also present the percent of years that the subjects report being occupied fulltime in either work or school.

In addition to this questionnaire data, as in Boisjoli et al. (2007) and Vitaro et al. (2012) we use administrative data from Quebec on whether each subject had received a secondary-school degree, and police records to identify the number of criminal offenses on record for each subject, collected when the participants were around 24 years old, to perform our cost-benefit analysis.

**Adult data:** In partnership with Statistics Canada, we have linked the longitudinal data to data from tax returns from 1998-2017. The linkage process is documented in Findlay et al (2018). Overall, 97.5% of the sample was linked to tax records for at least one year. See Appendix E for additional information. Tax returns contain information on

individual income from employment, including self-employment income, social transfers (and the number of years with non-zero income from employment or non-zero transfers), employment insurance benefits, contributions to professional organizations, marriage and household composition, donations to charity, disability, home ownership, and other measures.[11]

### *D. Validity*

To test for valid randomization, we carry out a balance check. Table 1 shows the baseline values of the two groups for several critical variables measured prior to randomization. There are statistically significant differences on 3 variables: initial anxiety measures, age of father at birth of subject and prestige of the mother's employment (at the 10% level). The fact that there are some differences does not mean that the selection process was non-random. It is not surprising to find imbalances for a handful of variables, especially given that the small sample size and that many variables were tested for differences. A joint significance test of all the baseline variables to treatment was not significant (p=0.34). There is no reason to believe that the randomization protocol was violated, and it is likely that these differences arose by chance. Since there is a chance that these variables might impact the skills and outcomes we wish to examine, we control for them in a robustness check.[12]

TABLE 1. BASELINE CHARACTERISTICS AND RANDOMIZATION CHECK

| | Control | | Treatment | | | | |
|---|---|---|---|---|---|---|---|
| | Mean | Standard deviation | Mean | Standard deviation | Observations | Treatment minus Control | p-value |

[11] The tax returns do not contain information on the detailed source of earnings. They generally have insufficient information on family composition for analysis of, for example, childbearing.

[12] There is some missing data in the baseline variables, and so controlling for any imbalances reduces sample size. An imperfect solution to this problem is to impute as zero those control variables only for the specifications that control for differential baseline characteristics between groups to avoid loss of sample: father's age (33 observations) and prestige of the mother's work (31 observations). We include a dummy equal to one when the value is imputed. Note that this imputation is limited to the baseline variables, and not to outcome variables, and that results that include these baseline controls are not materially different from those that do not, whether or not imputation is done.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Age | 6.03 | 0.3 | 5.97 | 0.29 | 250 | 0.05 | 0.2 |
| Attended Pre-school | 0.21 | 0.41 | 0.19 | 0.4 | 250 | 0.02 | 0.71 |
| Age of mother | 23.99 | 4.18 | 24.01 | 4.71 | 248 | -0.02 | 0.97 |
| Age of father | 26.9 | 5.34 | 28.28 | 5.33 | 217 | -1.38 | 0.1 |
| Mother education | 9.97 | 2.23 | 9.9 | 2.28 | 248 | 0.07 | 0.83 |
| Father education | 9.7 | 2.45 | 9.93 | 2.42 | 220 | -0.24 | 0.52 |
| # of children in HH | 0.97 | 0.9 | 1.07 | 0.8 | 249 | -0.1 | 0.42 |
| Adversity index | 0.43 | 0.24 | 0.43 | 0.27 | 249 | 0 | 0.96 |
| Mother works | 1.73 | 0.45 | 1.78 | 0.42 | 245 | -0.05 | 0.42 |
| Father works | 1.21 | 0.41 | 1.2 | 0.41 | 197 | 0.01 | 0.86 |
| Mother job prestige | 36.03 | 11.02 | 33.16 | 10.13 | 221 | 2.87 | 0.08 |
| Father job prestige | 35.19 | 9.58 | 35.22 | 9.83 | 209 | -0.03 | 0.99 |
| Initial Aggression | 14.51 | 4.78 | 14.62 | 4.58 | 250 | -0.11 | 0.86 |
| Initial Anxiety | 3.55 | 2.73 | 4.26 | 2.82 | 250 | -0.71 | 0.07 |
| Initial Opposition | 5.62 | 2.19 | 5.81 | 1.93 | 250 | -0.19 | 0.53 |
| Initial Prosociality | 6.52 | 4.79 | 6.99 | 4.51 | 250 | -0.47 | 0.49 |
| Initial Combativeness | 3.53 | 1.59 | 3.48 | 1.54 | 250 | 0.05 | 0.83 |
| Initial Inattention | 4.19 | 2.35 | 4.19 | 2.18 | 250 | 0.01 | 0.99 |
| Initial Hyperactivity | 2.79 | 1.21 | 2.96 | 1.19 | 248 | -0.16 | 0.35 |
| Initial Antisociality | 0.99 | 1.11 | 1.21 | 1.23 | 249 | -0.21 | 0.2 |

*Notes:* Data from MLES baseline data collection, 1984 (prior to randomization and program implementation). A joint significance is not significant (p=0.34).

Compliance was not complete. Some families (78 out of 250) from both the treatment and the control groups refused to participate in some elements of the study but were included in the longitudinal data collection. The rate of non-participation was the same across groups. These participants are included in the analysis as belonging to their initially assigned treatment groups (intention-to-treat analysis). The issue of compliance is discussed in detail, including balance checks, in Appendix B.

For the skills measured in adolescence, attrition is lower in early adolescence (less than 10% for most behavior outcomes) than in later adolescence (around 15%). Appendix C presents rates of attrition for the different adolescent variables and the p-value of the

difference between treatment and control. In no case are the attrition rates significantly different in treatment and control groups. However, attrition is high for the young adult survey data. Our confidence in the results from the young adult survey data is bolstered by the fact that employment estimates from the self-reported data are very close to the employment data from the tax data, and a falsification exercise presented in the appendix which suggests that any bias from attrition would be in the opposite direction of our results.

For adult economic outcomes using tax data, 98% of the at-risk disruptive sample was matched to administrative tax records for at least one year during the period 1998-2017. There is no significant difference in overall linkage rates between treatment and control groups: 97% of linkages for the treatment group were successful and 100% for the control group (see Findlay et al 2018 for a detailed description of the linkage process and rates). During that period, data for individuals may be missing for a given year (most years had an 83% match rate, though this is lower when subjects are younger). Even though other populations may have yearly match rates over 90%, recall that this sample was originally selected from low SES areas in Montreal, and the treatment and control groups come from the most at-risk disruptive children of the original sample. Given the characteristics of this group (for example, a high school graduation rate of less than 50%) lower match rates than the general population are unsurprising. See Table S13 and Appendix E for further details on the administrative data. On average, subjects within the at-risk disruptive sample are missing two out of the seventeen years. The robustness checks in Table S16 of Appendix F use different methods of dealing with unmatched years, including imputation, and the results are unchanged.

## II. Impact of the program

### *A. Specifications*

In Tables 2 - 5, we estimate the impact of the program using a simple comparison of means. We report p-values calculated using a permutation test of the difference in means,[13] and we also provide the coefficient estimate from a regression controlling for initial treatment group imbalances.[14] We estimate intention to treat effects. Robustness checks are presented in Appendix F. When discussing results, we compare the size of the impact to the difference between the disruptive (those randomized into treatment and control) group and the non-disruptive group to provide some idea of the scale of impact.

While our sample size is small, we have power to rule out reasonable sized effects in almost all cases where we find no effect. In Tables 2 – 5, we include a minimum detectable effect equal to 1.65 times the standard error of the estimate of the difference between the treatment and control group.

### *B. Adolescence and Young Adulthood*

Table 2 shows the treatment impacts during early adolescence, Table 3 shows the treatment impacts in late adolescence, and Appendix D shows the distributions by group for the different skills. Overall, we find that the treatment has a significant impact on non-cognitive skills, but no measurable impact on verbal IQ. School performance is only improved in late adolescence, potentially due to improved non-cognitive skills. This is consistent with the intervention's targeting of non-cognitive skills only.

[13] In the permutation test (also referred to as a randomization or re-randomization test), the treatment group assignment was randomly re-assigned 5000 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.

[14] Note that 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 necessary, and they do not substantially change the treatment estimate or the standard errors (Appendix F). There were 78 schools in 1984, and the average number of children in the experimental group in each school is three. In 29 of the schools, there is only one child per school. The fixed effect specification in the appendix includes the entire ELEM sample so that there are sufficient observations to estimate fixed effects without dropping observations.

TABLE 2. EARLY ADOLESCENT OUTCOMES

| | Control mean | Treatment mean | Non-disruptive minus Control | Treatment minus Control | Detectable effect (absolute value) | p-value Treatment minus Control | Treatment effect on averages (OLS) | Observ-ations |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Trust | 0.00 | 0.15 | 0.30 | 0.16 | 0.11 | 0.02 | 0.18 | 243 |
| | (0.03) | (0.06) | (0.04) | (0.07) | | | (0.07) | |
| Aggression Control | -0.01 | 0.14 | 0.41 | 0.15 | 0.13 | 0.05 | 0.14 | 248 |
| | (0.04) | (0.07) | (0.04) | (0.08) | | | (0.08) | |
| Attention Control | -0.01 | 0.15 | 0.37 | 0.16 | 0.14 | 0.06 | 0.17 | 248 |
| | (0.04) | (0.07) | (0.05) | (0.08) | | | (0.08) | |
| Sociability | 0.01 | -0.07 | 0.13 | -0.08 | 0.11 | 0.27 | -0.04 | 248 |
| | (0.03) | (0.07) | (0.04) | (0.07) | | | (0.07) | |
| Self Esteem | 0.00 | 0.03 | 0.20 | 0.03 | 0.12 | 0.68 | 0.04 | 232 |
| | (0.04) | (0.06) | (0.04) | (0.07) | | | (0.08) | |
| Altruism | 0.00 | -0.11 | 0.11 | -0.11 | 0.18 | 0.32 | -0.09 | 248 |
| | (0.06) | (0.09) | (0.06) | (0.11) | | | (0.11) | |
| Verbal IQ | 8.57 | 8.54 | 0.61 | -0.03 | 0.61 | 0.95 | 0.18 | 204 |
| | (0.19) | (0.35) | (0.19) | (0.37) | | | (0.39) | |
| Grades | -0.01 | 0.11 | 0.39 | 0.11 | 0.23 | 0.42 | 0.17 | 220 |
| | (0.07) | (0.12) | (0.08) | (0.14) | | | (0.15) | |
| Special education | 0.21 | 0.20 | -0.12 | 0.00 | 0.08 | 0.96 | -0.03 | 250 |
| | (0.02) | (0.04) | (0.02) | (0.05) | | | (0.05) | |
| Years held back | 0.26 | 0.26 | -0.16 | 0.00 | 0.09 | 0.96 | -0.04 | 250 |
| | (0.03) | (0.04) | (0.02) | (0.05) | | | (0.05) | |
| Ever held back | 0.40 | 0.39 | -0.20 | 0.01 | 0.11 | 1.00 | -0.05 | 250 |
| | (0.04) | (0.06) | (0.03) | (0.07) | | | (0.07) | |

*Notes :* Standard errors in parentheses. Each cell of column (1) provides the mean of the control group, and column (2) the mean of the treatment group. Column (3) provides the raw difference between the non-disruptive and the control group, column (4) the raw difference of the treatment and control group (ITT), column (5) gives the minimum detectable effect using a one-sided t-test (1.65*SE of column 4), column (6) gives the p-value of the T-C difference using a permutation (randomization) test. Column (7) is the conditional treatment effect from an OLS regression controlling for baseline differences between the treatment and control groups, with robust standard errors. Column (8) gives the number of observations in the treatment and control groups. The non-disruptive group is composed of those children who scored below the 70th percentile of anti-social behavior on the initial questionnaire in 1984. This non-disruptive group did not participate in the randomized evaluation and serves as a reference group.

**Non-cognitive skills:** Treatment has a significant impact on non-cognitive skills: in early adolescence, Aggression Control is higher in the treatment group (0.15 standard

deviations, p= 0.05; about 37% of the difference between the disruptive and non-disruptive sub-samples), Attention Control is higher (0.16 standard deviations, p=0.06; about 43% of the difference between the disruptive and non-disruptive sub-samples), and Trust is higher (0.16 standard deviations, p=0.02; about 52% of the difference between disruptive and non-disruptive). The intervention had no measurable impact on Altruism[15] or Friendliness, contrasted with the effect on Trust, suggesting social skills should be decomposed from a more general measure of pro-sociality.

We find persistent effects of the intervention on non-cognitive skills. In late adolescence, treatment had a significant impact on Aggression Control (0.19 standard deviations, p=0.04; 70% of the gap between the disruptive and non-disruptive sub-samples) and Trust (0.18 standard deviations, p=0.04; 69% of the gap between disruptive and non-disruptive sub-samples).

**Cognitive skills and school performance:** In early adolescence, there is no impact on IQ scores, grades, repeating a grade, or being placed in a Special Education Class. This is in line with what might be expected given the nature of the program: a focus on social abilities and self-control and an absence of intervention on cognitive skills. However, in contrast to the early adolescent period, in late adolescence we observe a large impact on school performance: Grades (0.22 standard deviations, p=0.10; almost half of the gap between disruptive and non-disruptive sub-samples), the likelihood of ever having repeated a grade (15 percentage points, p=0.03; 65% of the gap between disruptive and non-disruptive sub-samples) and years in Special Education (10 percentage points, p=0.11; 40% of the gap between disruptive and non-disruptive groups).

[15] We cannot exclude differences in Altruism smaller than the difference between the non-disruptive and the control groups, in part because the difference between the non-disruptive and control groups is quite small. The small size of this initial difference, however, combined with the estimate of altruism in the treatment group as being lower than that in the control group, supports our contention below that increased altruism is unlikely to be a channel of impact.

TABLE 3. LATE ADOLESCENT OUTCOMES

| | Control mean | Treatment mean | Non-disruptive minus Control | Treatment minus Control | Detectable effect (absolute value) | p-value Treatment minus Control | Treatment effect on averages (OLS) | Observ-ations |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Trust | -0.04 | 0.14 | 0.25 | 0.18 | 0.14 | 0.04 | 0.19 | 213 |
| | (0.05) | (0.07) | (0.04) | (0.09) | | | (0.09) | |
| Aggression Control | -0.01 | 0.17 | 0.27 | 0.19 | 0.15 | 0.04 | 0.15 | 213 |
| | (0.05) | (0.07) | (0.04) | (0.09) | | | (0.09) | |
| Attention Control | 0.00 | 0.04 | 0.25 | 0.04 | 0.15 | 0.65 | 0.00 | 210 |
| | (0.05) | (0.07) | (0.05) | (0.09) | | | (0.09) | |
| Sociability | 0.01 | 0.02 | 0.12 | 0.01 | 0.11 | 0.83 | 0.05 | 213 |
| | (0.04) | (0.06) | (0.04) | (0.07) | | | (0.07) | |
| Self Esteem | -0.01 | -0.01 | 0.13 | 0.00 | 0.11 | 0.98 | 0.01 | 202 |
| | (0.03) | (0.06) | (0.04) | (0.07) | | | (0.07) | |
| Altruism | 0.00 | -0.04 | -0.02 | -0.04 | 0.20 | 0.74 | -0.08 | 199 |
| | (0.06) | (0.11) | (0.07) | (0.12) | | | (0.13) | |
| Grades | -0.01 | 0.21 | 0.44 | 0.22 | 0.22 | 0.10 | 0.27 | 215 |
| | (0.07) | (0.11) | (0.08) | (0.13) | | | (0.13) | |
| Special education | 0.46 | 0.36 | -0.25 | -0.10 | 0.10 | 0.11 | -0.14 | 248 |
| | (0.03) | (0.05) | (0.03) | (0.06) | | | (0.06) | |
| Years held back | 0.60 | 0.50 | -0.26 | -0.10 | 0.10 | 0.12 | -0.14 | 249 |
| | (0.03) | (0.06) | (0.03) | (0.06) | | | (0.06) | |
| Ever held back | 0.77 | 0.62 | -0.23 | -0.15 | 0.10 | 0.03 | -0.17 | 249 |
| | (0.03) | (0.06) | (0.04) | (0.06) | | | (0.07) | |

*Notes :* Standard errors in parentheses. Each cell of column (1) provides the mean for the control group, and column (2) the mean of the treatment group. Column (3) provides the raw difference between the non-disruptive and the control group, column (4) the raw difference of the treatment and control group (ITT), column (5) gives the minimum detectable effect using a one-sided t-test (1.65*SE of column 4), column (6) gives the p-value of the T-C difference using a permutation (randomization) test. Column (7) is the conditional treatment effect from an OLS regression controlling for baseline differences between the treatment and control groups, with robust standard errors. Column (8) gives the number of observations in the treatment and control groups. The non-disruptive group is composed of those children who scored below the 70th percentile of anti-social behavior on the initial questionnaire in 1984. This non-disruptive group did not participate in the randomized evaluation and serves as a reference group.

Table 4 provides the estimates for impact in young adulthood on self-reported questionnaire data and administrative data. We find beneficial impacts on self-reported economic outcomes and group membership, and we confirm previous research finding impacts on crime and schooling.

TABLE 4. YOUNG ADULT OUTCOMES

| | Control mean | Treatment mean | Non-disruptive minus Control | Treatment minus Control | Detectable effect (absolute value) | p-value Treatment minus Control | Treatment effect on averages (OLS) | Observ-ations |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Group membership | 0.22 | 0.38 | 0.13 | 0.16 | 0.11 | 0.02 | 0.15 | 159 |
| | (0.03) | (0.07) | (0.04) | (0.07) | | | (0.08) | |
| Percent of years occupied fulltime | 0.77 | 0.83 | 0.06 | 0.06 | 0.09 | 0.25 | 0.11 | 153 |
| | (0.03) | (0.04) | (0.03) | (0.05) | | | (0.06) | |
| Percent of years receiving transfers | 0.14 | 0.10 | -0.07 | 0.04 | 0.07 | 0.39 | -0.05 | 153 |
| | (0.02) | (0.03) | (0.02) | (0.04) | | | (0.04) | |
| Post-secondary education | 0.13 | 0.07 | 0.14 | -0.06 | 0.09 | 0.40 | -0.04 | 159 |
| | (0.03) | (0.04) | (0.04) | (0.06) | | | (0.05) | |
| Voted (2001) | 0.49 | 0.48 | 0.06 | 0.01 | 0.15 | 1.00 | 0.01 | 147 |
| | (0.05) | (0.08) | (0.05) | (0.09) | | | (0.10) | |
| Volunteered (2001) | 0.38 | 0.45 | -0.08 | 0.07 | 0.15 | 0.46 | 0.07 | 148 |
| | (0.05) | (0.08) | (0.05) | (0.09) | | | (0.10) | |
| Number of crimes committed by age 24 (administrative data) | 2.15 | 1.13 | -1.47 | -1.02 | 1.21 | 0.17 | -1.09 | 250 |
| | (0.43) | (0.36) | (0.29) | (0.73) | | | (0.58) | |
| Secondary school diploma (administrative data) | 0.31 | 0.45 | 0.27 | 0.14 | 0.11 | 0.05 | 0.19 | 250 |
| | (0.03) | (0.06) | (0.04) | (0.07) | | | (0.08) | |

*Notes:* Standard errors in parentheses. Each cell of column (1) provides the mean for the control group, and column (2) the mean of the treatment group. Column (3) provides the raw difference between the non-disruptive and the control group, column (4) the raw difference of the treatment and control group (ITT), column (5) gives the minimum detectable effect using a one-sided t-test (1.65*SE of column 4), column (6) gives the p-value of the T-C difference using a permutation (randomization) test. Column (7) is the conditional treatment effect from an OLS regression controlling for baseline differences between the treatment and control groups, with robust standard errors. Column (8) gives the number of observations in the treatment and control groups. The non-disruptive group is composed of those children who scored below the 70th percentile of anti-social behavior on the initial questionnaire in 1984. This non-disruptive group did not participate in the randomized evaluation and serves as a reference group.

**Economic outcomes:** We rely on the tax data for income calculations, but we provide two estimates from the young adult surveys as supplementary evidence. Treatment subjects reported more years spent fulltime occupied in school or work and fewer years spent relying on social transfers, though neither of these findings reach thresholds of significance in the permutation test.

**Group membership:** The program increased membership in social groups as young adults (ages 21-26). Treatment subjects were 16 percentage points more likely to belong to a group (such as cultural or recreational groups) than control subjects (p=0.02).

**Other self-reported outcomes:** There was no consistent impact on post-secondary education, volunteering or voting, and the data are insufficiently precise to measure impacts on friendships, health violent behavior, stealing, fraud, depression, or self-esteem.

**Crime and School completion:** To perform the cost-benefit analysis we re-examine the impact of the program on school completion and criminal records at age 24, as these estimates are used in the cost-benefit analysis. Our analyses confirm the previous analyses concerning school completion and criminal behavior (Boisjoli et al. (2007), and Vitaro et al. (1999, 2012)). The results of the analyses for secondary school completion indicate that participants in the treatment group were 14 percentage points more likely to receive a secondary school diploma than participants in the control group. The results for criminal behavior suggest that the treatment group committed around one fewer crime per person compared to the control group.

### *C. Adult tax data: ages 20-39*

Using the administrative data from Statistics Canada, We find large impacts on adult social and economic outcomes. In particular, we find increased employment, marriage, and reduced social transfer receipt. Figure 2 provides a visualization of these impacts, along with increased school completion, compared to the non-disruptive group.

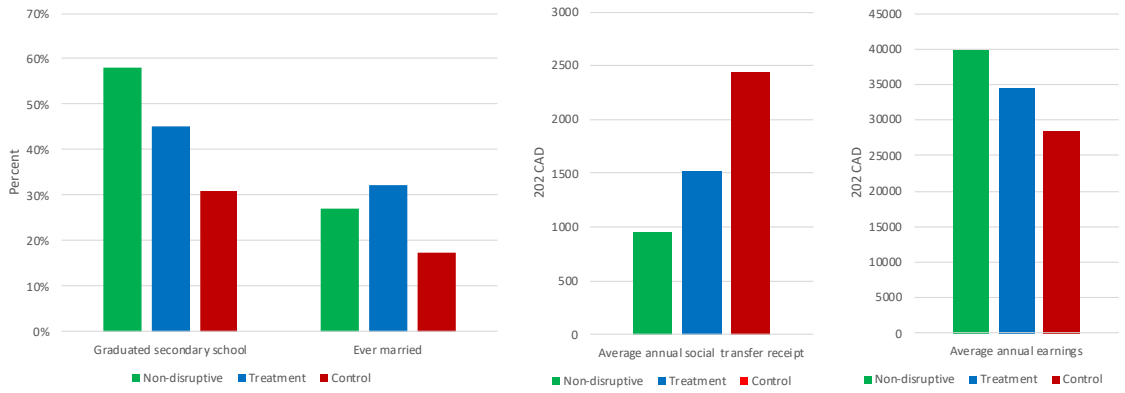



FIGURE 2. SUMMARY OF PRINCIPAL IMPACTS

*Notes:* The non-disruptive group is composed of those children who scored below the 70th percentile of anti-social behavior on the initial questionnaire in 1984. This non-disruptive group did not participate in the randomized evaluation and serves as a reference group.

**Labor market outcomes:** In Table 5 we show the impact of the intervention on employment and income. Individual income is composed of wage income, self-employment income, investment income, and other income (as well as some other very small categories not detailed here). To have a unified measure of the labor market effects, we combine wage income and self-employment income into earnings. Treatment subjects had non-zero income from employment (a proxy for employment status) for 2.2 more years than control group subjects (14.9 years in the treatment group and 12.7 years in the control group, p=0.03) about half the difference between the non-disruptive and control groups. Annual income from employment in the treatment group was 5708 $ CAD, almost 20%, higher than that in the control group ($ 28,752 vs $ 34,459, p=0.08), redressing the gap between the disruptive group and the non-disruptive group by about 50% (measured annual income from employment is not conditional on being employed). Figure 3 shows that the increase in income from employment is not driven entirely by participation: the entire distribution of the treatment group (red) is shifted towards the non-disruptive group (green). As with other tax variables, the raw differences tend to be lower in point value than the estimates adjusting for treatment group imbalances or the other robustness checks given in the appendix. In the interest of providing a conservative estimate, we focus on the raw differences and use those values in the cost-benefit analysis. As shown in column 8 of

Table 5, most of the differences are statistically significant after controlling for covariates and group imbalances.

TABLE 5A. ADULT OUTCOMES

| | Control mean | Treatment mean | Non-disruptive minus Control | Treatment minus Control | Detectable effect (absolute value) | p-value Treatment minus Control | Treatment effect on averages (OLS) | Observ-ations |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Household income | 61480 | 69950 | 25534 | 8469 | 8568 | 0.11 | 12172 | 245 |
| | (2611) | (4855) | (3881) | (5193) | | | (5532) | |
| Individual income | 35027 | 40050 | 9571 | 5023 | 5090 | 0.10 | 7010 | 245 |
| | (1569) | (2813) | (1963) | (3085) | | | (3241) | |
| Years with any employment income | 12.7 | 14.9 | 3.1 | 2.2 | 1.6 | 0.03 | 2.8 | 245 |
| | (0.5) | (0.8) | (0.5) | (1.0) | | | (0.9) | |
| Employment income | 28752 | 34459 | 11180 | 5708 | 5442 | 0.08 | 8091 | 245 |
| | (1681) | (2996) | (2079) | (3298) | | | (3414) | |
| Years contributing to unemployment insurance | 11.9 | 13.9 | 2.9 | 2.0 | 1.6 | 0.04 | 2.7 | 245 |
| | (0.5) | (0.8) | (0.5) | (1.0) | | | (0.9) | |
| Contributions to unemployment insurance | 419 | 489 | 126 | 70 | 72 | 0.11 | 102 | 245 |
| | (23) | (37) | (24) | (44) | | | (44) | |
| Years receiving social benefits | 3.9 | 2.8 | -2.1 | -1.1 | 1.3 | 0.16 | -1.7 | 245 |
| | (0.4) | (0.5) | (0.4) | (0.8) | | | (0.7) | |
| Amount of social benefits | 2436 | 1507 | -1488 | -929 | 817 | 0.06 | -1322 | 245 |
| | (277) | (333) | (225) | (495) | | | (425) | |

*Notes:* Each cell of column (1) provides the mean for the control group, and column (2) the mean of the treatment group. Column (3) provides the raw difference between the non-disruptive and the disruptive group, column (4) the raw difference of the treatment and control group (ITT), column (5) gives the minimum detectable effect using a one-sided t-test (1.65*SE of column 4), column (6) gives the p-value of the T-C difference using a permutation (randomization) test. Column (7) is the conditional treatment effect from an OLS regression controlling for baseline differences between the treatment and control groups, with robust standard errors. The non-disruptive group is composed of those children who scored below the 70th percentile of anti-social behavior on the initial questionnaire in 1984. This non-disruptive group did not participate in the randomized evaluation and serves as a reference group. Those who scored above the 70th percentile were randomized into either the treatment or control groups. Employment income includes self-employment.

TABLE 5B. ADULT OUTCOMES, CONTINUED

| | Control mean | Treatment mean | Non-disruptive minus Control | Treatment minus Control | Detectable effect (absolute value) | p-value Treatment minus Control | Treatment effect on averages (OLS) | Observ-ations |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Years contributing to professional org | 5.9 | 7.8 | 1.5 | 2.0 | 1.5 | 0.04 | 2.5 | 245 |
| | (0.5) | (0.8) | (0.6) | (0.9) | | | (0.9) | |
| Amount contributed to professional org | 226 | 332 | 65 | 106 | 86 | 0.04 | 130 | 245 |
| | (26) | (50) | (31) | (52) | | | (57) | |
| Ever reported married | 0.18 | 0.32 | 0.10 | 0.15 | 0 | 0.01 | 0.16 | 245 |
| | (0.03) | (0.06) | (0.04) | (0.06) | | | (0.07) | |
| Charitable contributions | 9 | 43 | 26 | 34 | 25 | 0.03 | 31 | 245 |
| | (2) | (24) | (16) | (15) | | | (22) | |
| Children | 0.79 | 0.81 | 0.19 | 0.02 | 0 | 0.87 | 0.02 | 245 |
| | (0.06) | (0.08) | (0.06) | (0.11) | | | (0.10) | |
| Tuition | 109 | 98 | 103 | -11 | 92 | 0.86 | 6 | 245 |
| | (31) | (35) | (38) | (56) | | | (42) | |
| Home deduction | 0.01 | 0.01 | 0.01 | 0.00 | 0 | 0.75 | 0.00 | 245 |
| | (0.00) | (0.01) | (0.00) | (0.01) | | | (0.01) | |
| Pension contributions | 672 | 674 | 514 | 3 | 350 | 0.99 | 53 | 245 |
| | (119) | (143) | (164) | (212) | | | (194) | |

*Notes:* Each cell of column (1) provides the mean for the control group, and column (2) the mean of the treatment group. Column (3) provides the raw difference between the non-disruptive and the disruptive group, column (4) the raw difference of the treatment and control group (ITT), column (5) gives the minimum detectable effect using a one-sided t-test (1.65*SE of column 4), column (6) gives the p-value of the T-C difference using a permutation (randomization) test. Column (7) is the conditional treatment effect from an OLS regression controlling for baseline differences between the treatment and control groups, with robust standard errors. The non-disruptive group is composed of those children who scored below the 70th percentile of anti-social behavior on the initial questionnaire in 1984. This non-disruptive group did not participate in the randomized evaluation and serves as a reference group. Those who scored above the 70th percentile were randomized into either the treatment or control groups. Employment income includes self-employment.

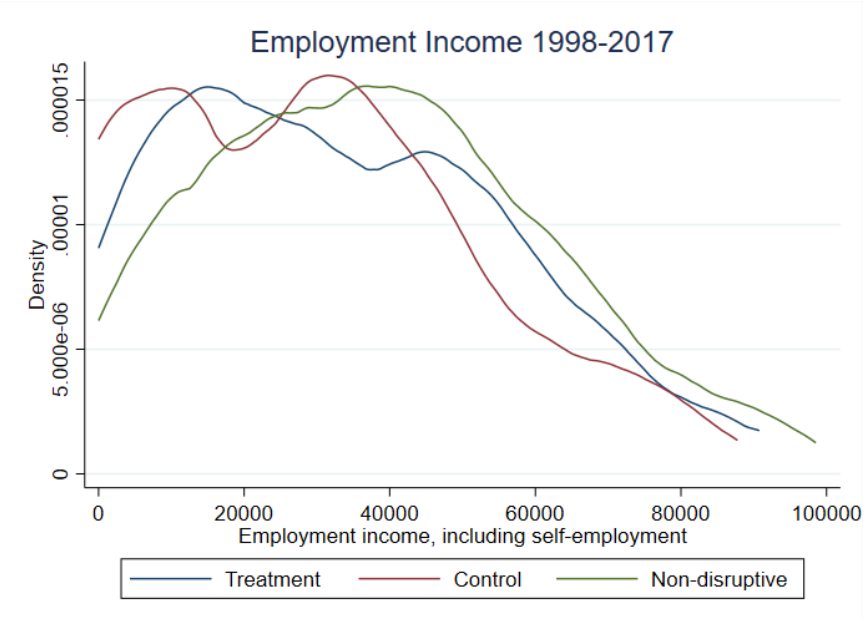



FIGURE 3. AVERAGE ANNUAL EMPLOYMENT INCOME

*Notes:* Figure shows the distribution of average annual employment income, including self-employment, for the control, treatment, and non-disruptive groups. The non-disruptive group is composed of those children who scored below the 70th percentile of anti-social behavior on the initial questionnaire in 1984. This non-disruptive group did not participate in the randomized evaluation and serves as a reference group.

**Insurance outcomes:** Treatment subjects relied less on social transfers. Over the entire period, yearly social transfers were $ 929 (almost 40%) lower than in the treatment group (p=0.06) and the gap with the non-disruptive group reduced by about 60%. Treatment subjects were also more likely to contribute to unemployment insurance than the control subjects, which is likely a direct result of higher employment due to automatic contributions.

**Social outcomes:** Treatment subjects also paid $106 more in dues each year to a professional organization such as a union ($332 vs $226, p=0.04), and did so for almost 2 more years than control subjects (7.8 vs 5.9, p=0.04). Treatment subjects were also 15 percentage points more likely to ever have been married (32% vs 18%, p=0.01), including common-law couples.

The above impacts are all robust to different specifications and tests presented in Appendix F. In contrast, the impact on charitable donations is not robust and we do not find any impacts on tuition deductions, home deductions, pension contributions or childbearing. The lack of robust impact on charitable donations, compared to the positive impact on participation in professional groups reinforces the distinction between altruism and trust as separate skills. Here, we see that the intervention targeted at social skills, increased trust, but not altruism, in childhood and adolescence, and group membership, but not volunteering, in young adulthood, and labor market outcomes, but not charitable giving, in adulthood.

Overall, treatment subjects live in households with higher income. This is partly due to composition, as they are more likely to be married, but more than half of the difference is due to higher individual earnings. In raw differences, the households of the treatment group reported 8,569 $ CAD more in total income. Of this, 5,023 $ CAD comes from increased individual income for the treatment individuals.

### *D. Discussion of the channels of impact*

The main component of the experiment was to expose the treated children to a 2-year training in self-control and social skills, and the training included direct contact with children with well-developed skills. It is possible that the treatment effect could be due to peer effects, linked to role models or friendships developed during the training that have persisted through their lives, rather than to the social skills training. We cannot completely disentangle those two aspects of the program; however, the pattern of impact suggests that the training was of primary importance. The skills that were the most changed were those that the training directly targeted (and are also highly correlated to economic outcomes). There was no impact on Friendliness, which could be expected if interactions with pro-social peers were responsible for behavior changes. Finally, if the interactions with the pro-social peers were critically important, we might have expected an initial impact in grades and school performance consistent with previous studies looking at peer effects on educational performance (see Sacerdote 2011 for a review). The fact that we do not find

an initial impact on grades - and only an impact in the later adolescence – is better explained by reducing the disruptive behavior that can be a barrier to schooling than peer effects. Note that school performance is correlated to Attention Control as shown in Appendix Table S 12.

The experiment also included two components with parents and teachers. Appendix Table S22 shows that there was no impact on parent behavior, so parents did not dramatically change their parenting in response to the parent training. The teacher training was quite light to begin with (two meetings) and only half the teachers agreed to participate, suggesting that the teacher training is not the primary driver of impact.

The sequence of impacts over the life course – non-cognitive skills in childhood, school performance in adolescence, then lower criminal behavior, higher earnings, and increased insurance as adults - is consistent with the hypothesis that improved non-cognitive skills lead to improved adult outcomes, even in the absence of changes in cognitive skills. While the intervention looks "small" based on hours of training involved, the large effect on adult economic outcomes is consistent with the observed magnitude of the impact on non-cognitive skills during adolescence, and with the literature showing the strong associations between self-control and social skills with economic outcomes. Several longitudinal studies following cohorts from childhood to adulthood have shown that self-control is highly correlated with wealth and labor market outcomes (Moffit et al., 2010, Duckworth et al. 2012, Vergunst et al. 2019). It has been also established that social skills, and especially trust and perspective taking (e.g., the ability to attribute mental states to others based on their behavior that was an important component of the training program on social skills) display very high returns on the labor market (Deming, 2017). Those findings are also consistent with the growing literature on the impact of trust on community or country-level economic outcomes.[16] Finally, while we cannot isolate the causal effect

[16] There is a flourishing literature on the importance of the contribution of trust to community or country-level outcomes, such as income per capita (Knack and Keefer 1997, Algan and Cahuc 2010), formation of large organizations (La Porta et al. 1997), trade (Guiso et al. 2009), state intervention (Aghion et al. 2010). Recent studies show 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 (Chetty et al. 2014). Our paper is also related

of self-control and social skills, we provide suggestive evidence about which skills are more closely connected to which outcomes in the Appendix G. Aggression Control is more closely connected to reduced crime, Attention Control is more closely connected to school performance and secondary school completion, and Trust is most closely connected to labor market outcomes and group membership. Increased marriage in the treatment group is not well explained by any of the skills.

## III. Cost-effectiveness, Benefit to cost ratio, and Rate of Return

To provide information about the adult impact of investment in childhood behavioral interventions, we compare the cost of the intervention to the impact of the intervention under a set of conservative assumptions and provide a sensitivity analysis. Under these assumptions laid out in Appendix H, the total program cost per offer was around $ 10,855 per offer in 2020 CAD.[17] In terms of overall program costs and potential target populations, recall that the impact estimates are based on the group that was initially targeted, that is, the most disruptive 30% of boys from low SES schools, so the total cost of a program based on this evaluation and its target group would be of a smaller magnitude than one that targeted the entire population or a larger group.

We first calculate how much each increment of benefit "cost" with this intervention. For example, how much would it cost, using this program, to avert one crime? Or, how much would it cost to bring one more student to high school graduation? This type of estimate of cost-effectiveness measures the effectiveness of a program in terms of the cost of attaining a desired outcome and is simply the size of the impact in countable terms (e.g.,

to a growing literature on the formation of trust either through teaching practices (Algan et al. 2013) or long-run historical events (Nunn and Wantchekon 2011, Guiso et al. 2016)

[17] Our estimate of the total cost per person using 2011 data on salaries was $9,327 in 2011 CAD. To provide a figure closer to current policy budgets, we convert to 2020 CAD using the average inflation rate over this period (1.7%) and do not account for the time value of money. This yields $10,855 in 2020 CAD, which is an estimate of what it would cost in 2020, per offer, to implement such a program. This contrasts with the comparison of costs and benefits, which must account for the time value of money (using a 3% discount rate), and so the cumulative cost of the program varies over time. This is because, for the cost-benefit analysis, we take the cost of foregoing other possible uses of money into account. We also use the discount rate of 3% on benefits, both monetary and non-monetary, to reflect this time preference. The sensitivity analysis in Appendix H provides estimates under 2% and 5% discount rates.

the total number of crimes avoided) divided by the total cost. This estimate does not rely on monetizing the value of the outcomes and can be used for making comparisons between programs that have similar policy goals (Dhaliwal et al. 2013). Table 6 provides the main results on cost-effectiveness for criminality, education, and labor market outcomes. We estimate that the cost of averting one crime through this program was a bit over $5,000,[18] enabling one more student to achieve secondary school graduation cost approximately $59,000, ensuring that one student never repeated a grade was $63,000, avoiding one year of special education assignment was $19,000, avoiding one year of social transfer receipt was almost $10,000, and ensuring one more year of employment (non-zero earnings) cost around $5,000.

TABLE 6. COST-EFFECTIVENESS ANALYSIS

| | Cost in 2020 CAD |
|---|---|
| Program per offer | $10 855 |
| Averting one crime | $5 112 |
| One more secondary diploma | $58 675 |
| Avoiding ever repeating a grade | $62 745 |
| Avoiding one year of special education | $19 111 |
| One less year of relying on social benefits | $9 868 |
| One more year of employment | $4 934 |

*Notes:* Treatment effects taken from raw differences, using a discount rate of 3%. The cost of the program does not account for the time value of money, but is the estimate of the original cost of the program, accounting for inflation. We assume that the treatment effect on crime fades by 10% per year and disappears at age 35.

However, the program impacted several outcomes at once, and to measure the returns of the program it is useful to consider a measure that compares the cost and benefits overall, and monetizing each benefit of the program is one way to do this. As detailed in Appendix H, we use several data sources to monetize the value of the benefits to the individual and to society, in order to provide a comparison of those benefits to the cost of

[18] This method of estimating cost-effectiveness does not depend on the value of the outcome. Here, we answer the question, "Using this program, how much does it cost to avoid one crime?" Further below, we try to value the program in terms of the value of the outcome. For example, in Appendix H we estimate a lower bound cost to society per crime of $1,912 in 2003 CAD based on administrative data police costs. Thus the cost of avoiding one crime using MLES crime is $5,000, and the benefit of avoiding one crime is the cost (a lower bound) that society will not have to pay ($1,912). In isolated case of crime, the cost/benefit analysis may not be favorable (though our social cost of crime estimates are a lower bound), but in our analysis below we include all benefits to give a more accurate picture of the returns to our program.

the program. Using a discount rate of 3%, we calculate the net present value of the costs and the stream of benefits in each year. The benefits of the program considered for the analysis are the reduced educational cost (reduced grade repetition and special education assignment), reduced crime (arrest and court costs), reduced social transfers, and increased employment income. Another benefit is increased secondary school graduation, but we do not monetize this separately from the crime and employment income results. We also do not consider other, more difficult to monetize, benefits such as the social benefit of employment or the cost of crime to victims. The details of the empirical approach are presented in Appendix H. We provide results until age 39 (the last year for which we have tax data available).

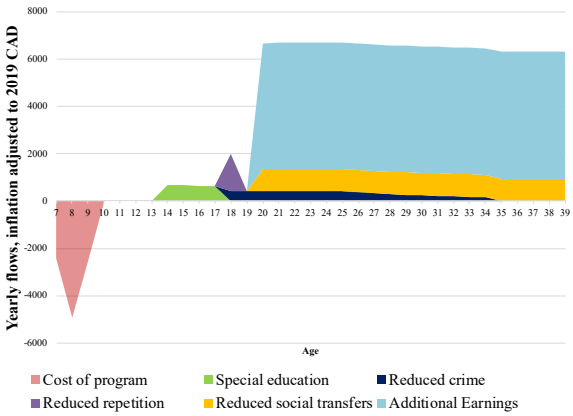
TABLE 7. ESTIMATED RETURNS TO INTERVENTION AT AGE 39

| | | |
|---|---|---|
| Cost per offer (including time value of money) | | $12 814 |
| All benefits, including income, discounted | | |
| Benefits | | $141 996 |
| IRR | | 17% |
| For every dollar spent, total benefits are | | $11 |
| Breakdown | % from education savings | 4% |
| | % from crime savings | 5% |
| | % from increased earnings | 80% |
| | % from social transfer savings | 12% |
| Taxpayer benefits only, discounted | | |
| Benefits | | $28 909 |
| IRR | | 8% |
| For every dollar spent, social benefits are | | $2 |
| Breakdown | % from education savings | 17% |
| | % from crime savings | 23% |
| | % from social transfer savings | 60% |

*Notes:* Table shows comparison of discounted cumulative costs and benefits at age 39. All cost and benefit figures use a 3% discount rate. Overall benefits includes increased earnings, reduced social transfers, reduced schooling costs (repetition and special education) and reduced criminality. Taxpayer benefits exclude increased earnings as a benefit.

Table 7 presents the results under the base case (a 3% discount rate). If benefits end at age 39, then there is an overall benefit of 11 dollars per dollar invested. Policymakers may also be interested in the taxpayer benefit, that is, excluding the labor market returns to the individual. Considering only financial expenditures avoided by the taxpayer, each dollar spent yields 2 dollars in reduced spending if benefits stop at age 39. Considering only social benefits, taxpayer investments would have been recouped by age 24. For a visualization of the incurred costs and benefits by age, see Figure 4, showing that until early adulthood the benefits primarily come from reduced crime and schooling expenditures. We begin counting labor market benefits and reduced social transfers at age 20, and they rapidly overtake other benefits in size. This underscores the importance of long-run follow up as net benefit to society is negative for the first decade after the program. A sensitivity analysis for different discount rates is presented in Appendix H. Note that this estimate is a lower bound, given that we do not include benefits that have value but are difficult to monetize (such as the cost of crime to victims) and do not include benefits beyond age 39.

Panel A. Cost and benefit flows until age 39




Panel B. Net accumulated social expenditure benefits

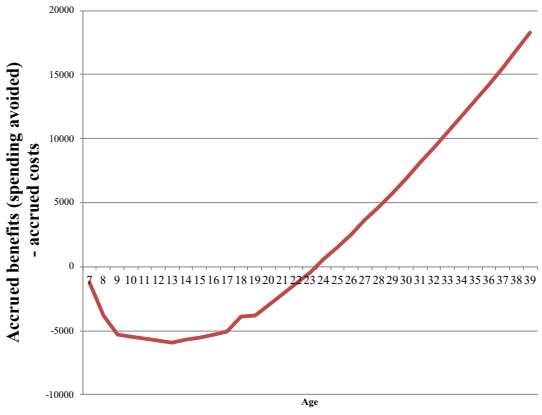



FIGURE 4. COST AND BENEFIT FLOWS OVER TIME

*Notes:* The left-hand figure shows the estimated costs and benefits per year from the program, by type of flow. Note that since the employment income benefit impact presented in the paper is an average over time, the benefit is blocky. The right-hand figure shows the net accumulated social benefits over time (that is, accumulated benefits minus accumulated costs at each age), which becomes positive around age 24.

Finally, to provide policymakers with an idea of the potential policy value of programs like this one, we provide an estimate of the overall internal rate of return of the intervention program in this sample.[19] We calculate an IRR of 17% if the labor market benefits are included, and 8% if only taxpayer benefits are considered.

Our estimates are of roughly equivalent magnitude to programs that included cognitive and non-cognitive elements. Chetty et al (2011) use data from Project STAR to estimate that children randomly assigned to classrooms with more experienced teachers (with no targeted training or special curriculum) had a USD$1,093 increase in yearly earnings at age 27. This is equivalent to USD$1,257 in 2020, which is equivalent to 1,684 $CAD in 2020. Based on our tax data, we estimate that the increased earnings from age 20 until age 39 are about 5,023 $CAD (Table 5). Our estimates are roughly three times as high as the Chetty et al estimates, which is unsurprising given that the "intervention" in Project STAR was not targeted to children most likely to have behavioral problems and was also much lighter. A closer program to MLES is the Perry Preschool Program, where Heckman et al (2010) estimate an annualized rate of return of 7-10%. If we assume that for MLES costs of around 10,800 $CAD were incurred at age 10, and total benefits at age 39 were 142,000 $CAD, this yields an annualized return of just under 9%. In addition, in the Perry Preschool Program, boys had increased income of around 20%, which is very close to our finding here.

## IV. Conclusion

[19] Note that periods of cost and periods of benefit do not overlap, so the internal rate of return is the rate of return that solves the following equation:

$$\sum_{t=0}^{t=2} \frac{-C_t}{(1+IRR)^t} + \sum_{t=7}^{T} \frac{B_t}{(1+IRR)^t} = 0$$

where $T$ is equal to 32 (up to age 39).

Working with a unique dataset matched to administrative tax records, we find that an intensive prevention program for disruptive boys conducted in low-socioeconomic neighborhoods of Montreal in the 1980s and focused exclusively on non-cognitive skills led to improvements over the life course. We find statistically and economically significant increases in adult economic (employment, wages and reduced social expenditure) and non-economic outcomes (marriage, and social group membership).

To understand the magnitude of these effects on adult outcomes, we evaluate how the program affected cognitive and non-cognitive skills during adolescence and early adulthood. We show that this intervention significantly increased self-control and trust in early adolescence, and that these effects persisted and were followed by subsequent improvement in education and social outcomes during late adolescence and early adulthood.

This finding is an important and unique contribution to the literature: in no other case has a randomized prevention trial for disruptive children at school-entry been linked to adult administrative data, providing causal evidence that investment in non-cognitive skills in early elementary school can have substantial individual and social benefits in adulthood.

While the large positive impact on the life trajectories of at-risk children is itself sufficient to justify public support of prevention programs such as the program we evaluated, they are also attractive from a strict efficiency perspective. Under a variety of reasonable and conservative assumptions, investments in childhood yield reasonable returns for taxpayers in terms of reduced expenditure on schooling (special education and grade repetition), delinquent behavior, and social transfers. The taxpayer may also have an interest in fostering better labor market integration and higher earnings for individuals. When increased earnings are considered, the returns to the program are very large.

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