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Investing in (secondary) schooling in Chile[1]

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In this paper we investigate the effects of providing information about financial aid opportunities for post-secondary schooling on educational expectations and effort at the end of primary school in Chile. We designed a DVD program in which young people from similarly poor backgrounds to the target sample discuss the relationship between effort in school and eligibility for loans and scholarships. Students in Grade 8 were randomized at the school-level into a group that watched the DVD at school, a group that received a copy of the DVD to watch with their families and a group that received no additional information. A novel aspect of this intervention is that it occurs *four years prior* to college and financial aid applications, but at the point where effort in school starts to cumulate towards an expanded set of opportunities later on. This paper has two aims. First, it evaluates the impact of being assigned to one of the DVD groups and finds that students in both groups have improved knowledge of financial aid opportunities and increase their effort in school, through reduced absenteeism. Second, we try to separate the impact of providing information to students alone from providing information to students and their parents. We evaluate whether providing the DVD to families produces greater effort gains in school than providing the DVD to students alone. We find that it does not. Our results add to a growing body of evidence that incomplete information can lead to underinvestment in human capital in the poorest families, potentially exacerbating educational and economic immobility. [257 words]

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1. Introduction

The canonical human capital model in economics has one agent, parent or child, deciding whether to invest in more schooling or not. The agent in this model maintains an eye toward the future, appropriately discounts the returns to different levels of education, compares these to the cost of using funds for tuition and then makes rational investment decisions. If all household members have the same preferences (or there is one dominant preference), then these can be represented by the utility of a single agent and the identity of the agent making the educational decision (parents or children) is irrelevant. The model predicts optimal education decisions when the agent has correct information about educational costs, is able to form expectations about income paths and does not face intra-household conflict in preferences.

New work in development and public economics suggests that this model misses some important aspects of the human capital investment decision. First, a range of experimental evidence has started to indicate that for children from the most disadvantaged families, information asymmetries and the costs of processing information may severely distort education investments. For example, Jensen (2010) evaluates the impact of providing information on returns to completing high school to 8th grade students in the Dominican Republic, and Nguyen (2008) evaluates the impact of providing similar returns to education information, with or without the use of role models, to parents of 3rd graders in rural Madagascar. Both experiments find large positive effects of providing this information on school investments as measured by school attendance, performance on tests, future enrollment and total years of education attained.[3] More broadly, several recent experiments in the USA (Oreopolous et al (2010), Sacerdote et al (2010)) provide high school graduates and/or their parents with information about and assistance with college financial aid applications. Oreopolous et al (2010) finds substantial increases in rates of college application and enrollment in families that were given additional help in filling out complex financial aid forms.

Second, development economists have accumulated sufficient empirical evidence to cast doubt on the "unitary model" of the household; this suggests that the identity of the decision-maker is relevant for the types of investments that are made.[4] A different set of experiments directly challenges the unitary model of the household which is used in modeling optimal human capital investment decisions. Recent work by Berry (2009) uses monetary and goods-incentives targeted at parents or at children to understand where incentives matter for providing effort. He finds that monetary incentives do extract effort from children

[3] In the Dominican Republic, the information provided to students translated into a 7% higher enrollment rate in high school the year after the intervention, and an average increase of 0.2 years of completed schooling four years after the intervention. In Madagascar, test scores rose by 0.2 standard deviations and absenteeism fell 3.5% points in schools provided with returns to education statistics.

[4] Duflo (2003), Rangel (2006), Martínez A. (2006).

learning to read but only for those with high productivity parents.[5] Bursztyn and Coffman (2010) implement a field experiment in Brazil's urban *favelas* to test for the existence of an agency problem in the schooling decision, where parents cannot perfectly monitor school attendance. They find that parents are willing to pay (i.e. willing to accept lower cash grants) to improve their monitoring ability.

Together, these experiments suggest that (i) addressing specific information failures and reducing processing costs for families from disadvantaged backgrounds may unlock more investments in education and (ii) that the identity of who receives information or incentives for effort matters. Our paper adds to this growing literature of the effects of access to information on educational investments.

In this paper, we evaluate the impact of an intervention we designed to provide 8th grade students and some of their parents with information about the availability of and eligibility for financial aid opportunities for post-secondary education in Chile. We produced a 15 minute informational DVD entitled "*Open the Box*" ("*Abre la Caja*") that collected together the tertiary education experiences of 13 adults from poor backgrounds. These "life stories" provided clear details on the importance of effort and good performance in high school for eligibility for post-secondary study: mainly through the vehicle of access to scholarships and loans. The overall message provided by the program was "effort now increases opportunities later in these specific ways".

Our intervention has two treatment groups to investigate the effect of parents and students learning this new information: one targeted at students alone and another targeted at families. In the first treatment group we show the DVD to students at their school in school time, whereas in the second treatment group we give students a DVD copy to share with their families.

Our project is novel in two ways: first, because much of learning is cumulative (e,g. Carneiro and Heckman, (2002) emphasize the importance of poor college preparation rather than credit constraints in limiting educational opportunities for disadvantaged youth), because high school performance is important for financial aid eligibility and college admissions, and because about two third of Chilean students are forced to choose a high school and a type of study after the end of 8th grade, we designed our intervention to target children four years *before* the relevant time for college and technical school applications. This differs from the financial aid assistance experiments that assist families once their children are graduating from high school. Our approach is also different to recently popular conditional

[5] A third novelty of our intervention is its implementation through a DVD program. This standardizes the presentation of the message to students thereby improving the fidelity of the treatment implementation. Moreover, if the program proves effective, this is something that could be easily and cheaply replicated and distributed, in contrast to having role model-type speakers provide information to schools.

cash transfer programs that incentivize current school attendance and/or performance by providing monetary rewards very close to contemporaneously, either to parents or to children. In this project, we ask: does exposure to information about future opportunities affect information sets, expectations and effort in school now.

A second distinguishing feature of our work is that we learn more about whether and how children and their parents make joint decisions about educational investments, and whether parent knowledge about future opportunities can generate larger improvements in child investment in learning. Even if children know that future opportunities entail effort costs now, they may not be sufficiently forward-looking to justify the effort cost. Hence, parent "principals" may be able to better extract child "agent" effort, if they are also aware of the payoffs to effort exerted now and if they can induce/monitor their children school effort.

Our experiment takes place in Chile, a middle income country that has succeeded in achieving universal primary school enrollment and has recently made huge leaps forward in getting children through secondary school education. Yet, the country still faces the challenge of broadening access to post-secondary education. Figure 1 shows sharply increasing post-secondary school participation with family income; the steep gradient is also familiar among researchers of the US educational system and it highlights an important and unanswered question: how to increase access to and uptake of post-secondary schooling for young people from the poorest backgrounds? This question is particularly relevant in Chile, given that country's low income mobility, high income inequality and recent increases in financial aid opportunities for tertiary education. These three features of the Chilean economy may make information asymmetries even more severe among the most vulnerable households, distorting their education decisions the most.

In this version of the paper, we present results on the impact of being randomized into the student-treatment versus the family-treatment on a set of student level intermediate outcomes collected at the follow up survey and from schools administrators (absenteeism recorded by schools). We find that immediately after viewing the DVD, children in the student treatment group report significantly higher rates of wanting to study after high school and for more years, and there are large increases in the fraction of kids reporting that financial aid resources will come from scholarships and from loans. Three to four months after the intervention, about two thirds of this information "wears off"; students in both the *Student* group and the *Family* treatment are equally and 3-4% more likely than students in the control group to report that they will finance post-secondary studies using loans. However, students in the *Family* treatment also report significantly higher rates of using scholarship finance; these students also score higher on a test of information provided by the DVD, relative to those in the *Student* treatment and in the control group, and are less likely to report not knowing how they will finance their post-secondary studies. There are small effects of the intervention on higher education expectations (which we discuss reasons for below) and significant and large reductions in the self reported and school-reported number of days absent from school for both the *Student* and *Family* level treatment (0.6 and 0.7 days respectively). This decrease in absenteeism, our primary measure of student effort, is not significantly different across *Student* and *Family* treatments.

In order to interpret the effect of being assigned to the *Family* treatment, we discuss the impact of selection into which students watch "*Abre la Caja*" at home on the interpretation of our results. Students in the *Family* treatment that chose to watch the DVD are different from those that chose not to do so and we find evidence of treatment effect heterogeneity. We compare the two treatment effects (*Student* vs *Family*) for students likely to select into watching the DVD by (1) instrumenting for DVD watching within the *Family* group and (2) reweighting observations in the *Student* treatment to make them more similar to those students in the family treatment who chose to watch the DVD. Taking this potential source of heterogeneity into account, we still cannot reject that the watching the DVD at school and watching the DVD at home have the same effect.

Our finding that parents do not seem to have an additional impact on educational investments (measured by absenteeism) is important. We show that parents in the *Family* group watched the DVD and learned from it, but this information did not seem to have a differential effect on child outcomes. We propose one potential explanation for this: that parents lack information about their children's behavior which is difficult to monitor. We show that across all groups, parents have the same information about absenteeism behavior of their children at follow up, which is an underestimate of actual attendance.[6]

The implications of our work are twofold. First, a lack of information about how to pursue further education may be an important constraint to raising effort in school, especially for students in the middle and high range of ability as measured by grades in school. Second, when parents have difficulty in monitoring certain types of effort in school, providing children with the information needed to make better human capital investment decisions may be more appropriate.

The paper begins by outlining relevant details of the Chilean education system and goes on to describe the nature of our intervention: sample selection, randomization and the characteristics of the student- and family-treatments. We discuss our empirical strategy, defining the parameters of interest and discussing

[6] This finding is in line with Bursztyn and Coffman (2010) result, where parents are willing to pay for information about their children absenteeism.

how selection into who watches the DVD in the *Family* treatment affects the interpretation of our results. We then present summary data, the immediate effects of being shown the program within student treatment schools and then our main results.

2. Background to Chile's education system

Since school vouchers were introduced nationwide in Chile in 1981, the country became the only example of a national education market. This has made it the focus of several recent papers related to school outcomes and school choice.[7] These papers outline the details of primary and secondary education in that country and so we only mention aspects of the system important to our study here, and spend more time on choices related to post-secondary education.

In Chile, 12 years of education is compulsory, and children start school at age 6. The first eight years of schooling are primary education, and the last four are high school, or secondary education. In Grade 8, students are between 13 and 14 years of age. Although they may not directly think about what they will be doing four years later, the system is structured such that if they do not consider their long-term possibilities at this point, they have a very good chance of screening themselves out of the possibility of continuing with higher education, either at the college or vocational school level. Given the voucher system, students can move between publicly funded schools, as long as they are accepted, and the system is designed to incentivize schools to make quality improvements in response to enrollment-driven demand. Students can, in principle, always choose to move to a better school if they want.[8]

One of the choices that students face comes after two years of high school. In Grade 10, they must choose between scientific-humanistic (HS) education and technical-professional (TP) education. The HS curriculum covers core subjects including language, mathematics and science, while the TP curriculum is targeted towards vocational skills. Since many schools terminate at Grade 8 (75% of the schools in our sample only have grades up to the 8th), and most secondary schools offer only one of HS or TP studies, the choice of which high school to attend from Grade 10 onwards essentially entails a choice about what path of study to take: HS or TP. This choice of HS or TP is relevant later on, when young adults are completing high school and deciding whether and how to further their studies at the post-secondary level. 84% of enrolled university students in 2009 had an HS high school training, and almost 60% of the TP students at post-secondary schools also came from HS backgrounds

[7] Urquiola and Verhoogen (2009) examine the role of parental sorting across schools in affecting class size and class composition variables; Hsieh and Urquiola (2006) and Bravo, Mukhopadhyay and Todd (2010) examine the direct impact of the Chilean school voucher system on schooling and labor market outcomes.
[8] There is a discussion regarding the existence of selection by the schools, which would limit the availability of students to move across schools. See Gallego and Hernando (2009).

Another choice that students must make, together with their parents, is whether to apply for and attend a free municipal school, a private-subsidized (private voucher school) that may charge additional school fees, or a private paying secondary school that does not accept government subsidies in the form of school vouchers. In Chile, it is generally acknowledged that private paying schools educate the most socio-economically advantaged, the private subsidized schools attract middle-income families and municipal schools cater for the poorer sections of society.[9] In our study, we only focus on students who attend municipal or private voucher schools.

Students have the option to study beyond high school in three different types of institutions: Technical Training Centres (*Centros de Formación Técnica* or CFTs, 2 years of study), Professional Institutes (*Institutos Profesionales* or IPs, 4 years of study) and traditional Universities (usually 5 years of study). To continue to post-secondary education, students must be academically eligible to enter college or vocational schools and able to finance their studies. Academic eligibility can take two forms: for most vocational training schools, high school grades (Grades 9-12) are considered. For college entry, students must write a general college readiness exam (like the SATs) called the PSU (*Prueba de Selección Universitaria*) exam at the end of Grade 12. Since 2006, this exam has been free for students at municipal schools. Scores on the PSU affect both college entry probability and financial aid eligibility (a minimmm score of 475 points is required for both). These cutoffs are provided to students during "*Abre la Caja*". Tuition costs are variable but high: in 2003, the annual cost of one year of public university tuition was 141% of annual household income for households in the lowest decile of the income distribution. One year of technical college tuition was 89% of annual household income in this lowest decile. These tuition costs put post-secondary schooling out of the affordable range for most families at the bottom of the income distribution in Chile.

Even though students from poor backgrounds have seen a doubling of high school graduation rates between 1990 and 2006, those who attend municipal schools still face significant barriers to pursuing post-secondary education. Municipal school pupils who sit the university entrance test are least likely to pass it, and least likely to get the highest scores. The inability to score reasonably well on this exam means that students from the poorest backgrounds are often unable to clear the first hurdle for financial assistance (OECD report, World Bank, 2009). Hence, decisions about HS or TP high schools, about public or private subsidized high schools, and about how hard to work in school to achieve high grades and prepare for the PSU are all decisions that happen earlier than the end of high school.

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[9] See Hsieh and Urquiola (2006) for a discussion of how the introduction of private voucher schools led to a flight of the better students from the public school sector. In 2008, only 7% of students were enrolled in private (no voucher) schools.

In the past 25 years, high school graduation rates have increased from 46% (in 1995) to 73% in 2006 (OECD, 2009). However, access to tertiary education remains a significant challenge to education policy-makers in the country. Progression into post-secondary education in Chile is strongly skewed by socioeconomic status. Although 18% of Chilean young adults aged 25-34 have completed some form of tertiary education (about the same as in Mexico), Figure 1 shows that most of these graduates will come from the upper income deciles in the country: In 2006, only 12.7% of 18-25 years old from the first income decile are in tertiary education, comparing to 53.3% of the top income decile (CASEN, 2006).

The unequal distribution of post-secondary education in Chile is said to contribute to continued intergenerational immobility. Despite strong positive economic growth throughout the 1990s and early 2000s, Chile's intergenerational correlation coefficient (for education) is 0.64, the third highest in Latin America (Hertz et al, 2007). This lack of educational mobility in the context of a country with a large income concentration (the Gini coefficient is 0.54 (MIDEPLAN, 2006)) increases the importance of finding policies to improve tertiary education access. As consequence, financial aid (scholarships) have increased from USD 40 million in 2000 to USD 173 million in 2007 (OECD, 2009), and have expanded from covering only college students to covering technical education at the post-secondary level.

However, finding the information that is required to make informed decisions about school investment is often difficult. There are many options of scholarships and loans for tertiary education, partial or full, with private or public funds, covering fees or food or transport or all. Each scholarship/credit has different requirements and benefits. Information about these scholarships is not easily accessible: we found 11 types of public scholarships, four types of loans, plus scholarships funded by private universities and donors. Eligible students must navigate the system to obtain these benefits.[10] It was only at the end of 2009 that the Ministry of Education compiled a document that organized all of these financial options for tertiary education.

3. Experimental design

i) *Description of intervention*

The intervention provides students with information about how effort and good grades in school open up opportunities down the line for further study, primarily by making it possible to apply for scholarships and/or government loans. We developed and produced a 15 minute informational DVD entitled "Open the

[10] In 2007, 660,000 were studying in tertiary education: 68% in college, 20% in IP and 12% in CFT. It is estimated that approximately 13.8% of enrolled students have some type of scholarship, and 26.4% pay their studies with loans (World Bank, 2009).

Box" ("*Abre la Caja*") that collected together the tertiary education experiences of 13 adults (5 women and 8 men; 9 professional and 4 technical careers) from poor backgrounds. These individuals talked about how, by working hard at school and becoming eligible for financial aid, they were able to finance their education in college or at technical college. Their studies enabled them to become (among other things) civil engineers, graphic designers, chefs, social workers, lawyers, TV communicators, economists and psychologists. These "life stories" provided clear details on the importance of high school achievement and PSU performance for acceptance to post-secondary education and for scholarships and loan eligibility, as well as on the availability of academic scholarships and non-merit-based student loans. The DVD emphasized financial aid availability for all types of post-secondary study (not just college).

In addition, we also developed a website that provided the same information (www.abrelecaja.cl) in more detail (both the life stories and the financial aid eligibility criteria). Students and parents in the treatment groups were able to access this website, and login access was restricted to participants in these groups. We do not use information on website usage in this paper.

We have three treatment groups: the control group, and two treatment groups. In treatment group A (*student group*) the DVD was shown at the school, and in treatment group B (*family group*) a DVD copy was given to students to share with their families. Treatment was randomized at the school level (described in more detail below).

At baseline (July and August 2009), Group A schools were given the baseline student questionnaire (self-responded, in class), after which all children in the grade 8 class were shown the informational DVD in a group. At the end of the program, a short follow-up questionnaire was completed by the students. Group B schools were given the student baseline questionnaire (self-responded, in class) and each student was given their own DVD to take home to their parents. All students in both groups also received a postcard advertising the project webpage and explaining how to login to the website. Control group schools (C) were given the baseline student questionnaire (self-responded, in class) but were not provided with any additional information. The maximum amount of time we spent in each class was 90 minutes. Each child in every school was also asked to take home a parent questionnaire and return it to school the following week at which time our enumerators collected these surveys. To incentivize return of the survey, we ran a lottery in which students who returned their parent surveys had the chance to win a computer. [11]

[11] To address the potential selection problem of the returned parent questionnaire, we randomly assigned (at the school level) the number of time the enumerators contacted and visited the school to pick up parent's questionnaires (one, two or three times).

At the follow-up in November and December 2009, we revisited all schools and administered a self-responded student questionnaire, with many of the same questions as in the baseline. We also asked students to take home another parent questionnaire and return it the following week, again for the chance to win a computer.[12]

ii) Nature of the Treatments

The program we designed was primarily an informational intervention, however because of the way in which the information was delivered (through "role models"), there is an aspect of motivation and inspiration inherent in the messages. We also expect most students to be aware of the fact that earnings increase with education level. This is a very different context to the Dominican Republic, where Jensen (2010) explains that students in rural areas are unlikely to be exposed to successful individuals from which they may infer the returns to education relationship; and also different to the situation facing Grade 3 students in rural Madagascar, where Nguyen (2008) show that less than half of the parents have completed primary school. We therefore defined the intervention around two critical elements affect the educational choices of children of poor backgrounds in Chile: the lack of knowledge about loans and scholarships opportunities and lack of role models.

To provide some evidence that students in our sample have fairly accurate perceptions of average returns to education as well as their own idiosyncratic returns, we asked two variants of two simple questions to get at what students expected to earn in a specific professional job (doctor) and in a specific technical job (computer programmer).[13] In order not to contaminate our treatment groups by priming them to think about returns to education, we asked these questions of two halves of the control group:

Control group 1

- What do you think your monthly earnings would be if you were a doctor?
- What do you think your monthly earnings would be if you were a computer programmer?

Control group 2

- What do you think someone who is a doctor earns each month?

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[12] A nation-wide teacher strike occurred just before our follow-up survey. However, by the time we went into the field, all teachers were back at work and we were able to re-contact all schools from the baseline to continue participation at follow-up. Only one school refused to participate in the follow-up, leaving us with data from students in 226 schools.

[13] A growing literature presents ways of eliciting returns to education information from students (see Dominitz and Manski, 1997; Avery and Kane (xx), Rouse (xx), Jacob and Wilder (xx)). We piloted methods of asking this information from Grade 8 students in a paper and pencil survey, and found the most sophisticated methods were also the most confusing to students. We then settled on the questions outlined in the text.

- What do you think someone who is a computer programmer earns each month?

In Figure 2, we show three bars that represent the (median of) reported returns to education for the average student (red line) as answered by control group 2, the (median of) reported returns to education for the student themselves (green line) as reported by control group 1, and the actual (median) returns to a technical and a college degree taken from the CASEN (2008) data. The graph indicates that student expectations of earnings for typical careers with higher and lower levels of education are not far from actual returns, and that there are no systematic differences between representative and idiosyncratic expected earnings. The information in this figure suggests that manipulating information about returns to education may not have been useful among this set of students and that any effects that we see from the treatment are unlikely to operate through changes in the expected returns to education.

In contrast to what they know about returns to education, our Chilean students are much less likely to have picked up details on how individuals from backgrounds similar to theirs managed to pursue an education. Our program, providing a standardized message to students, emphasizes the importance of good high school preparation, produced by effort at school, for eligibility and access to financial aid for future studies.[14]

Even though the message is standardized, the student and family treatments represent fundamentally different treatments. Students in Group A are all required to watch the DVD in class, while students in Group B have the option of watching the DVD at home, with their parents (or others, or alone). Therefore, we are interested in both whether being exposed to the program *at all* has an impact on educational expectations and inputs, and also whether being exposed to the program in the family setting has a differential impact on educational expectations and inputs.

One interpretation of B is that it is a scaled down version of A, since not all kids watch the DVD. This is not necessarily the case if there are heterogeneous treatment effects. We will show that the probability of watching the DVD in B group is positively correlated with school grades. If, for example, the treatment only has an effect on children with high grades it is possible that both treatments will have the same effect.

We cannot separate out the importance of providing information about financial aid from the importance of the "role model" effect; however, we interpret exposure to the program as exposure to a message of

[14] One could be concerned that the DVD was providing students with additional information on the link between good grades at high school and a higher probability of being accepted in to college. Appendix Figure 1 shows that even at baseline, students have a good understanding of the relationship between higher grades and a higher likelihood of entry in to college, abstracting from financial concerns.

"You too can do it" (an alternate to the "default behavior" of no further study) that provides students with no direct information about returns to education, and a lot of information about how to improve chances of eligibility for study and how to overcome the hurdle of tuition.[15] We do show some evidence from the follow-up survey on how much information kids and their parents retained.

iii) Sample selection

The project was conducted among schools in the Metropolitan region of Chile. The region includes the city of Santiago and the provinces of Chacabuco, Talagante, Cordillera, Maipo, and Melipilla. We used the list of urban schools present in the 2007 Grade 8 SIMCE database to define our sampling frame.[16] All schools are required to have 8th grade students write this exam, so this list represents the universe of schools that existed in Chile in 2007. It would not include any new schools that opened up between 2007 and 2009. To focus on children who we might expect to show the largest response to the provision of financial aid information, we selected all schools in the two lowest income groups (as defined by government administrative records) that had at least one grade 8 class with at least 20 children enrolled in the class.[17] These criteria left us with over 400 schools, all of which were approached to be part of our experiment.[18]

We hired a call center to contact the schools in our sampling frame and asked them to join our study. A total of 227 schools were reached and agreed to participate. Some fraction of schools declined to participate while others were unable to be reached by phone. The less than 100% participation of schools has implications for external validity of our study, which we discuss at the end of the paper.[19]

iv) *Randomization and application*

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[15] Dynarski and Scott-Clayton (2006) discuss different "default options" that teenagers from poor and rich socioeconomic backgrounds may face.
[16] SIMCE is the national system of evaluation of learning results administered by the Chilean Ministry of Education. The tests evaluate competence in and minimum knowledge of the basic school curriculum. The test it is applied to all students in the country; starting in 2006, the test has been applied yearly to 4th grade students, and every other year to 8th and 10th graders. We use the 8th grade results from 2007 in this paper for stratification.
[17] Schools are classified into one of five socioeconomic groups using four variables: a) father's years of education, b) mother's years of education, c) monthly income and d) Vulnerability Index (IVE) of the school. The first three are collected as part of the SIMCE parent's questionnaire. The last one is calculated yearly by the "Junta Nacional de Auxilio Escolar y Becas" (JUNAEB). (SIMCE, 2008)
[18] In sample selection, we also approached three schools in the next highest income class, in order to make up our sample size. All of our results are robust to the exclusion of these three schools
[19] We considered the school cluster in the power calculation. The sample allows us to obtain a power of 80%, with a significance level of 5%,, a minimum detectable effect size of 0.2 standard deviation, intra-cluster correlation of 0.27, cluster size of 30 and a R^2. The intra-cluster correlation, cluster size and R^2 were computed with previous SIMCE data.

We were concerned about information spillovers at the grade-level and so chose to randomize the two treatments across schools. Before the baseline and randomization, we visited each school to obtain consent from principals for participation in the project, class lists of all Grade 8 students (larger schools had more Grade 8 classes) as well as Grade 7 final scores and math scores for these students.[20] After obtaining these lists from each school, we grouped schools into five strata by SIMCE 2007 score and randomly assigned schools to one of three groups within these strata: treatment A, treatment B and control group. Within each school we randomly selected which Grade 8 class was going to participate in the study, School principals were then contacted to set up a day for our first visit; all principals were given a standard script in which it was explained that our study would investigate future education and career plans of students and that students would be given more information about future opportunities for study, either during baseline or during in 2010, depending on our budget.

4. Empirical framework

We use the framework of Rubin's causal model to describe the parameters of interest and outline our two-part approach to estimating the effects of providing financial aid information on outcomes.

i) Estimating the impact of the Student treatment (A) and the Family treatment (B): Intent to Treat

Let A_i be an indicator for whether student i receives the student treatment, where $A_i = \{0; 1\}$. Similarly, define B_i as the family treatment, where $B_i = \{0; 1\}$. Let Y_i^0 be the potential outcome for students when they do not receive any treatment, Y_i^A be the potential outcome for students when they receive program A and Y_i^B the potential outcome when they receive program B. The actual outcome for any student can be represented as $Y_i = (1 - A_i - B_i)Y_i^0 + Y_i^A A_i + Y_i^B B_i$, where only one of Y_i^A, Y_i^B or Y_i^0 is observed depending on which group the student is assigned to. The average difference in outcomes for students in program A and in control schools is:

$$ITT^A = E\left[Y_i^A \middle| A_i = 1, B_i = 0\right] - E\left[Y_i^0 \middle| A_i = 0, B_i = 0\right] \tag{1}$$

$$= E\left[Y_i^A \middle| A_i = 1, B_i = 0\right] - E\left[Y_i^0 \middle| A_i = 1, B_i = 0\right] + \left\{E\left[Y_i^0 \middle| A_i = 1, B_i = 0\right] - E\left[Y_i^0 \middle| A_i = 0, B_i = 0\right]\right\}$$

Two schools (65 students) were unable to provide us with grades; it was sometimes difficult for school principals to quickly collate this information for us. In addition, some schools provided us with incomplete or illegible grade lists. This means that some students do not have grade 7 scores, even though their school provided us with this information. For these students, we impute a grade 7 score using the class mean grade 7 score and create an indicator variable to capture that this value is imputed. All regressions that control for grade 7 score also control for the grade 7 score imputed indicator.

The first two terms represent the Intent to Treat (ITT^A) effect. Since all students watch the DVD in the *Student* treatment, this also represents the Average Treatment Effect on the Treated (*ATET*) or the Treatment Effect on the Treated (*TOT*). The second two terms in brackets capture the difference in outcomes for students in program *A* if had not been treated with program *A*. Random assignment of schools to treatment groups ensures that there is no selection into the treatment and so the average outcome among students in the control group is a valid counterfactual for untreated outcomes among students assigned to program *A*, implying that the last two terms are equal.

Equivalently, for program *B*, we can write:

$$ITT^B = E\left[Y_i^B \middle| A_i = 0, B_i = 1\right] - E\left[Y_i^0 \middle| A_i = 0, B_i = 0\right] \qquad (2)$$

$$= E\left[Y_i^B \middle| A_i = 0, B_i = 1\right] - E\left[Y_i^0 \middle| A_i = 0, B_i = 1\right] + \left\{E\left[Y_i^0 \middle| A_i = 0, B_i = 1\right] - E\left[Y_i^0 \middle| A_i = 0, B_i = 0\right]\right\}$$

where the randomization ensures that $E\left[Y_i^0 \middle| A_i = 0, B_i = 1\right] - E\left[Y_i^0 \middle| A_i = 0, B_i = 0\right] = 0$ and we are left with an unbiased estimate of $ITT.^B$

ACA

In following sections, we show that treatment and control groups are balanced on a range of baseline observable characteristics (X_{ij}) of students, parents and schools, lending credibility to the randomization. We will interpret the parameters in the following regression equation (where the *j* subscript for school group is suppressed for now):

$$Y_i = \alpha_0 + \alpha_1 A_i + \alpha_2 B_i + \varepsilon_i \qquad (3)$$

as the *ITT* of being shown the DVD at school, for all students in group *A* (α_1) or of being given the DVD to take home, for students in group *B* (α_2). These unbiased estimates inform us about what to expect if each program was implemented as designed. We test for whether α_1 and α_2 are significantly different from zero, jointly and separately. Y_i outcomes are measured in the follow up survey for student *i* in each school and ε_i is a person-specific error term. Fixed effects for five strata of 2007 school SIMCE score are included in all regressions and results are presented first without and then including controls for baseline covariates, X_{ij} that could influence variation in outcomes. Since assignment to treatment is randomized across schools, standard errors are clustered at the school-level. If α_1 (α_2) is different from zero, showing the DVD to students (providing the DVD to students to take home) affects the outcome Y_i. If $\alpha_1 \neq \alpha_2$, then program *A* and program *B* have different *ITT* effects on the outcome.

Our outcomes of interest fall into four categories, all of which are important for understanding the effects of the treatments. The first set relates to plans about and knowledge of financial assistance for college or vocational school (scholarships, loans, family or no method and answers to questions about financial aid eligibility). These plans require students to have information on forms of financial assistance and eligibility requirements, as well as beliefs about their likelihood of accessing these channels. The second set of outcomes capture education expectations: students are asked whether they will continue to study after high school and for how many years (more details on survey questions are in the Survey Questions Appendix). The third set captures measures of effort in school: we use self-reported and parent-reported absenteeism as well as school attendance data. The fourth set of variables describes educational outcomes that are directly relevant for future educational attainment: SIMCE test results at the end of Grade 8 and school-assigned grades at the end of Grade 8.[21][22]

ii) Estimating the impact of the Student treatment (A) relative to the Family treatment (B): Effects for compliers

Our experimental design resulted in differential compliance rates with watching the DVD across *A* and *B* groups. In the *Student* treatment, all students were exposed to the program in class, so our estimate of α_1 is an unbiased estimate of the average effect of watching the DVD for all students.[23] In contrast, just under 60% of students assigned to the *Family* treatment reported watching the DVD at home. This differential compliance makes it difficult to draw conclusions about the relative effect size of actually watching the DVD (alone or with family) using only the *ITT* estimates.

To see this, rewrite equation (2):

$$\alpha_2 = E\left[Y_i^B \middle| A_i = 0, B_i = 1\right] - E\left[Y_i^0 \middle| A_i = 0, B_i = 1\right] \tag{4}$$

[21] In other contexts, grade repetition may also be an important educational outcome to consider. However, in Chile, the probability of repeating a grade is zero for Grades 1-3, and very low up until Grade 9 or 10. Requirements for passing include 85% attendance at school, a minimum grade of 45 (one course failed) or a minimum grade of 50 (if two courses are failed). In 2008, 3.2% of 8th Grade students failed at the end of the year, 1.6% left school, and the vast majority, 95,2%, passed on to Grade 9 (Roman, 2009; Ministerio de Educacion, 1997; Ministerio de Educacion, 2009).

[22] Where appropriate, we discuss alternative tests of significance (Bonferroni tests) to guard against over-rejection in this multiple-testing environment. We present balancing tests using 27 baseline variables, as well as results on effects of the treatment for 8 to 10 outcome variable, which means there is a reasonable probability that some fraction of null hypotheses will be rejected.

[23] Under the assumption that our schools are representative of all urban schools in the lowest two income classes in Chile, the ATE would tell us what to expect if we were to roll out the program to all such schools. We comment further on the external validity of our results at the end of the paper.

$$= E\left[Y_i^B \middle| A_i = 0, B_i = 1, W_i = 1\right] * \Pr[\mathrm{W_i} = 1|\mathrm{B_i} = 1, \mathrm{A_i} = 0] + E\left[Y_i^B \middle| A_i = 0, B_i = 1, W_i = 0\right] * \Pr[W_i = 0|\mathrm{B_i} = 1, \mathrm{A_i} = 0] - E\left[Y_i^0 \middle| A_i = 0, B_i = 1\right]$$

where we have used the fact that randomization eliminates selection bias into treatment to simplify the expression in the first row. From (4), it is clear that the average value of outcomes for students in group *B* is a weighted average of the effect on students who watch the DVD in group *B* ($\Pr[\mathrm{W_i} = 1|\mathrm{B_i} = 1, \mathrm{A_i} = 0]$) and those who do not watch the DVD ($\Pr[\mathrm{W_i} = 1|\mathrm{B_i} = 1, \mathrm{A_i} = 0]$). α_2 will underestimate the effect of actually watching the DVD at home as long as spillovers from DVD watchers to non-watchers are zero or small (positive or negative) (i.e. $E\left[Y_i^B \middle| A_i = 0, B_i = 1, W_i = 0\right] = 0$) or small).[24] This implies that even if we estimate $\alpha_1 \geq \alpha_2$ from (3), watching the DVD at home could still have a larger impact than watching at school. Non-compliers in (4) obscure this potential difference.[25]

To estimate the impact of watching the DVD on endogenously selected compliers in group *B*, we specify the treatment as "watching the DVD at home" and instrument this with assignment to group *B*. Without additional covariates, this scales up the ITT^B by 1/0.6. If we assume there are homogeneous treatment effects of watching the DVD at home or in school, then the IV estimate identifies the ATE of the *Family* treatment which we could *directly* compare to the ATE of the *Student* treatment, α_1. However, homogenous treatment effects are unlikely in this setting. We show that it is the higher ability group *B* students that are more likely to watch the DVD at home and that the effect of providing information about future education opportunities looks different for students of different ability. Hence, the average effect for students choosing to watch the DVD at home is unlikely to reflect the average for all students if they were made to watch the DVD at home. In the presence of heterogeneous treatment effects, the IV strategy identifies the effect for *compliers* (Angrist and Imbens, 1994). An implication of this is that if the IV estimates of the effect of group *B* are larger than the ATE for group *A*, we cannot say whether this is because the *Family* treatment has a larger impact on outcomes than the *Student* treatment or whether the estimated effect for the compliant students in group *B* is larger than the estimated effect for the average

[24] It is possible, although we think unlikely, that there are negative spillovers from DVD watchers to DVD non-watchers in Group *B*.

[25] One maintained assumption throughout is no discouragement effects of watching the DVD, either at school or at home. Nguyen (2008) finds some evidence of discouragement effects of "role models" in the experiment of providing information about returns to education via statistics and role models in Madagascar. However, this effect was hard to disentangle from the non-standardized messages that the role models were providing. Moreover, in preliminary work, we look at whether any students who said they wanted to study after high school switched their response to no study after high school, in the follow-up, and whether this was larger in the A or B group. There was no evidence of discouragement in either group.

student in group *A* because of heterogeneity in responses.[26]

To address this issue, we combine experimental variation in assignment to groups with non-experimental methods to isolate the impact of watching the DVD at home *on compliers* in group *B* and the impact of watching the DVD at school for group *A* students who are *most like compliers* in group *B*. In order to be as transparent as possible in creating this group non-experimentally, we reweight the *ATE* of *A* using inverse probability weights to reflect the outcomes of students with baseline observable characteristics most like those of compliers in group *B* (Horwitz and Thompson 1952). To create the weights, we estimate a probit model of whether a student in group *B* reported watching the DVD at home, controlling for a set of baseline variables, including the baseline value of each relevant outcome variable. These estimated coefficients from the probit model are applied to students in group *A* to create predicted `synthetic' probabilities of watching the DVD ($\hat{p}(x)$) under an alternative regime where choice is possible.[27] Using these predicted probabilities, we construct inverse probability weights $wt_i = \frac{\hat{p}(x)}{(1-\widehat{p(x)})}$ for students in group *A*, $wt_i=1$ for students in group *C* (since these students have zero probability of watching the DVD by experimental design) and $wt_i=1$ for students in group *B* (the treated students).[28]

Students with a high predicted 'synthetic' probability of selecting into DVD watching, based on observables, are weighted up in the regression.[29] Combining the reweighting of group *A* students with IV for the DVD watchers in group *B* creates comparable estimates of the effects of watching the DVD at school or at home, for compliant-type students. Intuitively, the IV rescale the ITT^B to represent the effect among compliers while the inverse probability weighting (IPW) of group A rescales the ITT^A to represent the effect among compliant-types. Our main regression equation becomes:

$$Y_i = \tilde{\alpha}_0 + \alpha_{1,IPW} A_i + \alpha_{2,IV} DVD\ at\ home_i + \epsilon_i \quad (5)$$

We show four specifications in our results: one that exploits the randomization and produces estimates of the *ITT* effects α_1 and α_2 (equation (3)); a second combining the randomization with IV to estimate the

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[26] We could, alternatively, produce bounds on the estimated *ATE* for watching the DVD at home for all students in group *B*.

[27] The randomization to groups ensures that the distribution of X's has common support across groups (i.e. observables are balanced across groups).

[28] Weights for students in group *B* do not depend on the estimated propensity score. Intuitively, the IV estimator for the effect of watching the DVD at home is already reweighting for the types of students who choose to watch the DVD in group *B* (Angrist and Krueger, 1999).

[29] Inverse Probability Weighting (IPW) is often used in the context of adjusting outcomes for untreated units to create a comparable comparison group for treated units (Horvitz and Thompson 1952; Hirano, Imbens and Ridder 2003; Wooldridge 2002). In this case, we are reweighting outcomes for students who have been *treated under a different regime*, or, not exposed to the *Family* treatment at all. In this way, we think of students in program *A* as one possible control group for students in the *Family* treatment.

the *ATE* for the Student treatment (α_1) and the *TOT* for the *Family* treatment ($\alpha_{2,IV}$) (Imbens and Angrist, 1994)[30]; a third combining the randomization with IPW to isolate effects for compliant-types in group *A* ($\alpha_{1,IPW}$) which we refer to as a synthetic *TOT;* and a fourth combining randomization with the IV and IPW procedures to estimate $\alpha_{1,IPW}$ and $\alpha_{2,IV}$ in equation (5).[31]

With these results in hand, we can make clearer statements about the relative effects of watching the DVD at school or at home. If $\alpha_{1,IPW} < \alpha_{2,IV}$ then watching the DVD at home has a larger average impact on outcomes than watching at school for students who would choose to watch the DVD if given the option to watch. If, on the other hand, $\alpha_{1,IPW} \geq \alpha_{2,IV}$, then watching at home has no larger impact on outcomes than watching at school for this type of student. In the final part of the paper, we investigate why it is that watching the DVD at home may (or may not) be important for explaining differences in educational inputs and outputs across treatment groups.

5. Data description

i. Sample characteristics

Table 1 provides some summary statistics from our data. After stratifying on 2007 Grade 8 SIMCE scores (averaged within schools), 56 schools were randomly assigned to the child-treatment (group A), 56 schools to the family treatment (group B), and the remaining 115 schools to the control group (227 schools in total.[32] 31% of our schools are private voucher schools and the rest are municipal (public) schools. Our baseline survey included responses from 6,233 students. At follow-up, we retain responses from the 5,009 students who gave consent at baseline. This leaves data on 80% of students from the baseline survey who are present when we return for the follow-up. Importantly, as shown in Table 2, the attrition rate for students is the same across treatment and control groups, at about 20%. Since school absenteeism is one of our main outcomes, we further discuss attrition below.

The second panel of Table 1 shows the student-level control variables. Students are about 14 years old (95% of students are aged 13, 14 or 15) and 47% of students are female. Only 52% of mothers have completed high school (using student reports of parental education), although this information is missing

[30] Because neither group A nor group C had the option to watch the DVD at home, the IV estimate of the effect of watching the DVD at home for group B represents an estimate of the treatment on the treated (Bloom, 1984).
[31] Note that we do not want to compare the reweighted estimate of A with the ITT of B. This is because the ITT of B weights all observations (those who watch the DVD and those who did not) equally; while the IV estimates of the effect of B identifies the TOT for students who watched the DVD.
[32] One school in the control group dropped out of our study between baseline and follow-up.

for 15% of students.[33] Although the families that send their children to these schools are from the two lowest income quintiles in Chilean society, they live in a middle-income country, so it is not surprising that 86% of the sample have a working DVD player at home. Most students in group *B* could have watched the DVD at home if they had wanted to.

The average 7th grade score, captured from school records, is a 53 (on a scale from 10 to 70), with a standard deviation of 5.39 points. We have school grade data for 88% of students, and this fraction does not vary with program assignment

Since a body of social psychology research emphasizes the importance of impulse control as a predictor of cognitive control and positive school outcomes later on in life (Eigsti et al 2006, Mischel, Shoda and Peake 1988; Shoda, Mischel and Peake 1990, Mischel et al. 1989), we measured how patient each student is, albeit in a fairly crude way. At the end of the baseline survey, each student was given the choice to accept one sweet immediately or five sweets one week after the survey. 52% of students are impatient (chose immediate reward) according to this measure.[35]

The third panel of Table 1 reports our main student-level outcomes at baseline. A very large fraction of students (76%) report that they want to study beyond high school, for an average of 2.1 years. This is much higher than the 14% of young adults aged 18-25, in the lowest two income quintiles, who are actually enrolled in any post-secondary education in 2006 (CASEN, 2006). Since most students report wanting to study further, we capture information from all respondents on how they plan to pay for post-secondary schooling. Most students, at baseline, think that they will finance their post-secondary studies with scholarships and family finance and a very low fraction (9%) of students report that they plan to use loans to pay for studies. 47% report that they have no idea what finance assistance they will use. Our intervention is designed to address this information gap.

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[33] We impute an average value of education for mothers and create a missing education indicator to capture this missing information. We do not use reports on father education, since this was missing for a much larger fraction of the students (25%).

[35] Although we would have liked to measure time preference more finely, this would have required a longer, more detailed set of questions that would have been difficult to implement in the classroom conditions under which we conducted our surveys. Instead, we chose this question because it gave us a revealed preference measure of which children were more able to delay gratification. This measure has been used extensively in the social psychology literature to capture how well children control their attention when facing temptation. In longitudinal studies, this measure has been shown to positively predict outcomes in adolescence, including cognitive control (Eigsti et al, 2006), concentration and self-control (Mischel, Shoda and Peake, 1988; Shoda, Mischel and Peake, 1990), test scores (Mischel et al., 1989) and educational attainment. One might worry that this measure is also picking up risk tolerance, as these students were not able to know whether they could trust us to return the week after. While this is possible, we believe that we gave the students every indication that we would be returning. First, the consent form we asked them to sign said that we would be back to visit them at the end of the year, to ask them some more questions. Second, we gave each of them a parent questionnaire to take home, and asked them to return this to the school for us to pick up, a week later.

Average absenteeism reported at baseline (by students, parents and schools) underlines the importance of our focus on absenteeism as a measure of effort in school. Over one third of students report being absent from school at least once in the week before our baseline visit; while schools (from which we have data) report that over half of students were absent at least once in the month of May.[36] In contrast, parents report absenteeism for only 27% of students, although this is from the subset of parents who had their surveys returned to school.[37]

ii. Attrition and absenteeism

Because we survey students at school, the main source of attrition between baseline and follow-up is absenteeism on the day of our follow-up visit. Non-random absenteeism at follow-up is tricky to interpret: on the one hand, evidence of non-random absenteeism (attrition) could be taken as evidence that program assignment affected behavior, since school attendance is one of our measures of effort in school. On the other hand, non-random absenteeism at follow-up would cast doubt on the internal validity of our impact estimates for all other outcomes.

In fact, we do not find any evidence of differential absenteeism across school groups. The top panel of Table 1 shows that 80% of students are present at the follow-up. To analyze if the fraction of student present varies across treatment group, we explain the absenteeism at baseline and follow-up with treatment assignment. The results in Table 2 (second column) show that the fraction of students present at follow-up is the same across groups and statistically indistinguishable. Of course, the composition of students who attend school at follow up may have changed differentially across groups, which would also give us cause for concern. Figure 4 shows an obvious relationship between Grade 7 score and the probability of attriting by (being absent at) follow-up (the graph is almost identical if we exclude students with imputed Grade 7 scores). The three lines in the figure represent attrition probabilities for each of groups *A*, *B* and *C*. Students with lower grades are more likely to be absent in all groups. The figure suggests that lower performing students are much more likely to attrit from Group *A* than from other groups, while higher performing students look like they may be more likely to attrit from Group *B*. However, as column (5) of Table 2 indicates, there is no statistical difference in the probability of students of different ability attriting across groups at follow up. Across the board, attrition is non-

[36] Since absenteeism data is captured by each teacher, manually, it was difficult for all schools to provide us with legible records. We copied absenteeism data from almost all school (what fraction) and then coded the absenteeism data for May and October, for those schools and students with legible data. We matched student absenteeism data to our survey data using student ID numbers and names. Overall, we found complete absenteeism data for 64% of the follow up sample. This percent is not significantly different across the treatment and control groups.
[37] The fraction of parental response is not different across treatment and control groups.

negligible, but not differentially related to group assignment or to the distribution of ability within each group.

The lack of differential attrition (overall and by student ability) across treatment and control groups gives us more confidence in the internal validity of our results. In the next sections we will show our treatment has an heterogeneous effect that depends on school performance. Since attrition does not depend differently on school performance in any treatment groups, our estimated treatment effects will not be caused by selection of which students are present at follow up. However, it does raise the question of whether we should expect to see any effects of the intervention on effort in school, as measured by attendance on the day of our visit. We suspect (and are investigating) that school attendance on the day of our visits is not representative of general attendance behavior and that students may have made additional effort to attend school on the day that we were visiting at follow up.[38] Luckily, we are able to use alternative measures of attendance that do not rely on whether students show up to class on the follow up day: student and parent self-reports of absenteeism in the *week* before the survey and detailed school attendance registers in the *months* before and after our survey.

A final point about attrition at baseline is worth noting. Absenteeism of students at baseline (attrition before surveying) may affect the external validity of our results. Although not statistically different across schools or by student ability across schools (see the marginal effects from attrition probits presented in Table 2, columns (1) and (4)), the rate of absenteeism on the day that we visited schools at baseline is high, at about 20% (not shown). Just as in the follow up sample, it is clearly the worst performing students (using class rosters and Grade 7 scores) who are less likely to be present at baseline (column (4), Table 2). This finding implies that all of our results will be weighted by the behavioral responses of higher ability students exposed to different treatments at baseline. Since the intervention provides information on how to finance post-secondary studies, these higher ability students in the poorest schools are likely the relevant group for policy.

iii. Balance of characteristics across schools and students at baseline

Table 3 provides a set of balancing tests for outcome and control variables at baseline. All regressions include controls for school group assignment and standard errors that are robust to arbitrary heteroscedasticity and clustered at the school level. At the bottom of each column, we present two p-values: one for the joint test of the coefficients on the *Student* and *Family* treatments being zero, and the

[38] Anecdotally, our study became known as the "Super 8" study, since we gave students in all groups a choice of sweets at the end of the baseline survey. Also, the promise to be entered into a lottery for a computer, conditional on returning parent questionnaires, seemed to create additional excitement. Since students might have been informed of our second visit ahead of time, they may have made extra effort to be in school on that day.

second for the test of whether the coefficients on the two treatments are significantly different from each other.

The first panel shows the balance of student-level outcomes. All outcomes are balanced at the baseline: all educational expectations (whether they will study beyond high school; the number of years they will study after high school, whether they will study at a college) have the same mean across groups. In addition, all variables related to financing of post-secondary education, educational inputs and effort are balanced.

The lower panel of Table 3 presents balance test results for the main control variables. Although most of the variables are balanced across assignment groups, a small number are not, as one might expect from multiple testing of different outcomes. Mother education information is missing more often in the Student treatment group (0.026 higher fraction of missing education information) and school-reported Grade 7 scores are marginally lower (-0.70 lower average score) in this group.[39] Since we are testing 31 variables for balance at baseline (not all shown), some fraction of these tests is likely to reject the null. Performing the Bonferroni test for joint significance of the coefficients on *Student* and *Family* treatment dummies across all of these balancing regressions, we cannot reject the null that all coefficients are equal to zero. This gives us more confidence that the treatment and control groups do indeed look the same across a range of observable characteristics.[40]

6. <u>Results</u>

i. Immediate impact on information and expectations for Student treatment

Table 4 examines the effect of watching "*Abre la Caja*" in class on student's knowledge of relevant financing options for further study and on their educational expectations, immediately after they have watched the program. If there was any impact of the DVD on information sets and on expectations, we should observe this in the immediate change in responses for these students. The table shows means of each variable reported by students before they were shown the DVD (column 1), and immediately after they were shown the DVD (column 2). Column 3 presents the difference in these means (After-Before) and the standard error of the difference, robust to heteroscedasticity and clustered at the school-level.

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[39] Some tests are not shown. Omitted regression results that produced significant differences across treatment and control groups include: missing school poverty score (only one school has a missing poverty score), and whether the student reports wanting to study at a TP school after high school (higher fraction in the *Family* treatment group).
[40] We performed the same set of balancing tests on the set of students who are present at follow up. The balancing tests produce very similar results.

These differences in means show that watching the DVD had an immediate and substantial effect on student reports of plans to finance post-secondary education. The fraction of students reporting they will use scholarships or fellowships for their ongoing education rises by 5.8 percentage points, which is a 20% increase on the baseline fraction. The largest change appears for students reporting that they will finance their studies using loans; the fraction reporting this option doubles from 9.3% to 17.6% immediately after the DVD. This statistically significant increase may reflect the fact that the government loan programs are much less well-known among students than the merit-based scholarship programs. Concomitant with the increase in fraction of students reporting alternative financing methods for further study, there is also a decline of 7.6% in the fraction of students who say they do not know how they will pay for their studies. It is also comforting to see that the DVD did not manipulate students' reports that their families should pay for their post-high school education as there was no information provided in the DVD related to parental ability to pay for tuition.

The lower part of Table 4 shows that the DVD also had an immediate, large and significant impact on the fraction of students reporting that they will continue studying beyond high school and on the average number of years they expect to study. 3.4% more students claim they want to study after high school (off a high baseline of 75%) and the average increase in number of years of education expected after viewing the DVD is a significant 0.17 of one year. Although reported expectations change in response to watching the DVD, we cannot measure whether behavior (effort in school) actually responds to this information until 3-4 months later.

The results of Table 4 are not entirely obvious. That is, the significant differences in reports observed in column (3) are produced by students absorbing some of information from the program and also deciding that this information is relevant to the set of choices they might make in the future. So, if none of the students had found the information to be relevant, we might not have seen any impact on mean response. Another implication of this point is that not all students may respond equally to the DVD. In the last three columns of Table 4, we explore whether the immediate impact on student responses varies by student ability as measured by 7^{th} Grade scores. We create three indicator variables – one for a score that is below 50 ("low"), one for scores between 50 and 60 ("medium") and a third for scores above 60 ("high") – and examine differences in mean responses for students within each score group.

Looking at the financial aid reports: low grade students report the largest reductions in 'lack of information about financial aid' while high grade students report the largest increases in plans to use loan finance for funding further education. As we might have expected, the high grade students report the smallest change in plans to use scholarship finance after high school – this is probably because they are already aware of this source of finance. In terms of educational expectations: both medium and high grade students are about 4% points more likely to report that they will study after high school while the medium and low grade students reporting wanting to study for between 0.14 and 0.24 more years after high school.

The magnitudes of the immediate responses for students watching the DVD in class point to an interesting and somewhat expected source of heterogeneity. The DVD clearly did not induce all students to change their expectations about how to finance post-secondary studies or whether they want to study, in the same way. High ability students become more aware of loan finance and report higher levels of demand for post-secondary schooling; low ability students report improvements in their information sets across the board and a demand for more years of post-secondary school. Thus, we should expect the longer-run effects of the program to differ by student ability. Further, we will be interested in knowing whether these changes persist in magnitude and significance, 3 to 4 months after the intervention.

ii. Effects of program assignment on expectations and effort at follow-up: ITT estimates

Table 5 shows the effects for the intervention, 3 to 4 months after the initial survey; our interest is in the coefficients on the *Student* and *Family* treatments. We present two specifications for each outcome variable: one that controls for group assignment and SIMCE stratum fixed effects only, and a second that includes controls for all student, parent and school variables that could be important for the outcomes, and the baseline value of the dependent variable. Sample size varies when students have missing information on the baseline value of the dependent variable.

Almost uniformly, the estimated effects of being assigned to the *Family* treatment are larger than, although not always significantly different from, the effects of being assigned to the *Student* treatment. These effects are also significantly different from zero. Also, the estimated coefficient on the *Family* treatment reflects the lower bound on the effect of watching the DVD at home, since not everyone watched the DVD.

In the *Student* treatment group, the fraction of students reporting that they will finance education with a government loan increases by between 3.7 and 4.6 percentage points. This effect is large and significant but less than one third the size of the immediate impact of the DVD on loan finance that was reported in Table 4. Information from the DVD may "wear off" over time, and this information may also concentrate in a subset of students. The impact of being assigned to the *Family* treatment on reported use of loan finance is, remarkably, about the same as being assigned to the *Student* treatment: the fraction who report they want to use loan finance is 3 to 3.1 percentage points higher in the *Family* treatment compared to the control group. These results are robust to the inclusion of controls.

Assignment to *Family* treatment also raises the fraction of students reporting that they will use scholarships for post-secondary school finance: by about 3.4-3.7 percentage points. This point estimate is significantly different from the control group and from being assigned to the *Student* program. It is also only about two-thirds as large as the immediate effect for students in Table 4. *Family* treatment students are also significantly less likely to report that they do not know how they will finance their post-secondary education.

As we might have expected, students with more educated mothers and with higher Grade 7 scores are more likely to report that they will finance post-secondary schooling with scholarship or loans, and less likely to report that they have no idea how to pay for tertiary education. However, the effect size for mother education in particular is small, relative to the impact of the programs themselves.

The last set of results for knowledge of eligibility rules described in the DVD, in columns (9) and (10) of Table 5, suggests that students in the *Family* treatment retained more information from the DVD than those in the *Student* treatment. Almost all of this score difference comes from more accurate responses of *Family* treatment students to a question about the relevant PSU exam score threshold for entry to college and technical college (results not shown): this was one of the explicit pieces of information provided in the DVD.

Exposure to the DVD in class seems to have increased expectations of loan finance for post-high school studies alone, and in a way that is diluted up to 50% over time (0.046/0.093). Exposure to the DVD at home produces large effects (relative to baseline) on information about both types of financial aid and about the details of the eligibility process. Although the coefficients on the *Family* treatment are almost always larger than for the *Student* treatment, these coefficients are not significantly different from each other. The joint test results in Table 5 do reflect that being assigned to either treatment group significantly improved student reports of relevant (columns (1-8)) and general (columns 9 and 10) financial aid information, especially once we control for baseline values of the dependent variable. The fact that the *Family* results are driven by students who choose to watch the DVD at home, possibly with their parents, and the observation that the immediate impact estimates for the *Student* groups were different by ability suggest a heterogeneity in effects for different types of students. We postpone a discussion of the heterogeneity in effects until we have concluded with our analysis of program impacts on education expectations and on effort in school.

Table 6 shows that there are no significant longer-term effects of being assigned to the *Family* treatment on whether students think they will study after high school, or on the number of years they think they will study after high school; however, the fraction of students in group who report that they want to go to college after high school increases by 4.2 percentage points (result not shown). The fraction of students in the *Student* treatment who report that they want to study after high school rises by a significant 3.9% (close to the immediate impact shown in Table 4) and the fraction reporting that they plan to study at a college is higher by 2.6 percentage points, once we control for a set of covariates. Jointly, the treatments seem to have raised the fraction of students reporting any post-high school study and the fraction wanting to go to college; these estimates are not significantly different across *Student* and *Family* treatment groups.

Putting the results of Table 6 together with results in the previous table, we see that being assigned to either treatment group increased the information about financial aid for students in both groups by a large (relative to baseline) and significant amount, and this is accompanied by small, statistically weaker improvements in expectations for post-secondary studies. Given the high fraction of students at baseline who report wanting to continue with further studies (75%), it is not surprising that "*Abre le Caja*" did not move educational expectations by a significant amount, especially 3-4 months after treatment.

Despite these small changes in education expectations (as we are able to measure them), new information provided by the DVD may still affect behaviors. Consider the set of students who report that they will continue with their studies. This fraction was more than five times larger than the actual fraction of students from similar backgrounds who are currently enrolled in post-secondary education. This suggests a slippage between Grade 8 expectations and Grade 12 opportunities. Part of what our DVD emphasized was the importance of current schooling investment decisions -- putting in effort now -- for opening up opportunities four years later. Therefore, it is plausible that we could see an improvement in behaviors among students who already wanted to continue with their studies.

In Table 7 we present some evidence that behaviors do change. In this table, one particular self-reported behavior at school changes and in similar ways across *Student* and *Family* treatments. We have two measures of self-reported behavior that capture effort in school: whether the student was absent from school at all last week and the number of days the student missed school last week (where no absences are coded as zeros). The interpretation of self-reported absenteeism data is obviously problematic and we are wary of affording them too much weight in our conclusions. However, since these questions were asked at baseline and at follow-up, we can use baseline values of this variable to control for unobserved, time-invariant tendencies of specific students to underreport bad behavior. We also present results for actual absenteeism recorded in school attendance registers in May and October (pre and post-intervention). Importantly, comparing behaviors across treatment and control groups helps to control for the fact that students with high initial educational expectations may have improved their effort in school over the school year, even without being exposed to the DVD.

For school absenteeism, the coefficients on both treatment groups are large and negative, suggesting that absenteeism fell by as much as 4.5 percentage points for students in the *Student* treatment and by 2.2 percentage points (not significant) for students in the *Family* treatment. This represents a 6 to 12% reduction in absenteeism prevalence. However, these coefficients are not jointly significantly different from zero. Self-reported total number of days absent falls significantly in both groups, by between 0.13 and 0.17 days. Given a mean of 0.59 days absent, this translates into a 36 to 47% reduction in self-reported absenteeism for students in either of our treatment groups, or 0.1-0.17 of a standard deviation.

Reported absenteeism from school records provides even stronger evidence that being assigned to one of the treatment groups reduced absenteeism prevalence and total days absent. We measure absenteeism in October 2009 in the last four columns of Table 7: a few weeks before we visit the school for the follow-up survey. This reassures us that schools were not manipulating attendance records in anticipation of our visit, as they did not know at the time that we would request records for the entire year. The results show that for students assigned to the *Family* treatment, absenteeism at all falls by 11 percentage points on average (or a 20% reduction, given the baseline prevalence). The coefficient on the *Student* treatment is smaller in this regression, but not statistically different from the *Family* coefficient. Moreover, in both *Family* and *Student* treatments, total absenteeism in October falls by 0.6-0.73 days, a 32 to 40% reduction off the baseline mean; or 0.2-0.3 of a standard deviation reduction.

Jointly, both treatments significantly reduce the total number of days absent among students compared to control students and also significantly reduce the probability of being absent at all, according to school records. The fact that the effects for absenteeism are apparent in school records as well as the self-reported data makes us more confident that we are measuring actual changes in student behavior. Note that once again, we cannot reject the equality of the *ITT* results for *Student* and *Family* treatments.

Overall our estimates of ITT^A and ITT^B suggest that assignment to either of the treatment groups improved student information about financial aid options for post-secondary study in Chile, particularly regarding the use of student loans. Students in the *Family* treatment report more knowledge of financial aid eligibility rules and are also more likely to have a plan for financing college or vocational school with some combination of loans and scholarships. Educational expectations improved to a smaller extent, although they were high to begin with. Finally, both *Student* and *Family* treatment groups adjusted their inputs in school by reducing absenteeism substantially, 3-4 months after baseline. These average effects at follow up mask differential responses for students of different types that were evident in the immediate effects measured in the *Student* treatment group in Table 4. In the next section, we investigate whether this heterogeneity is evident in both groups at follow up and whether there is any differential impact of the *Student* and *Family* treatments after accounting for how this heterogeneity interacts with compliance in the *Family* treatment.

iii. Heterogeneity, selection into DVD watching and difference in Family and Student treatments: IPW and IV estimates

The information provided in "*Abre la Caja*" will be most relevant to students who do not already know about financial aid for post-secondary studies and for those who are most able to meet the entrance requirements for college and technical/professional schools. We already noted that the immediate impact of watching the DVD for individuals in the *Student* treatment group was different by ability (measured by 7th grade scores). For these reasons, it is hard to imagine that any behavioral responses will be the same across types of students, within the *Student* treatment group.

In addition, incomplete compliance with watching the DVD for students in the *Family* treatment is clearly not random. Figure 4 shows how the probability of watching the DVD (estimated in a probit model) increases strongly in Grade 7 average grade. Students scoring at the top of their class have a predicted probability of watching the DVD of close to 80%, while those scoring a 50 (in the lower range of the score distribution) have just over 50% predicted probability of watching.[41] Table 8 presents estimated marginal effects from the probit regression of whether a student reported watching the DVD at follow up, or not. The sample is restricted to students in the *Family* treatment who are present at follow up. Only 60% of students watched the DVD at all, with younger students, students with higher 7th grade scores and students in private voucher schools being significantly more likely to report watching the DVD, as intuition might suggest.[42]

In Table 9, we present some evidence that heterogeneity in response of information sets is evident across the grade distribution, while heterogeneity in behavioral responses is driven by students with medium and high grades. We estimate OLS regressions for a subset of the main outcome variables (results for the remainder are provided in an appendix) where we control for whether the student scored a low, medium

[41] This is over those that were present at the baseline, which are those with the best grades (Table 2), so the selection is deeper.

[42] The mean number of times these children watched the DVD was 1.195 times, the median number of times was once. And, of those 711 students who reported watching the DVD at least once, 68% of them reported watching at least once with their parents.

or high grade at the end of Grade 7 and all of the interactions of these indicators with *Student* and *Family* treatments.

Focusing on the S*tudent* treatment, the results in Table 9 suggest that the DVD improved information about the financial aid process and educational expectations most for the low and medium scoring students, while absenteeism behavior (number of days absent from school) fell the most for medium and high grade students in this group. In contrast, information about financial aid sources improved the most for high grade students in the *Family* treatment while educational expectations increased more for students with medium and high grades, and absenteeism fell most for students in the medium grade range. Since the lowest scoring students in the *Family* treatment group are very unlikely to have watched the DVD, it is not surprising these students do not experience large differences in the majority of outcomes.

With differential impacts of the treatment on students of different ability, it is much harder to draw conclusions about the relative effects of watching the DVD with and without parental involvement, simply by comparing ITT^A and ITT^B. To get closer to comparing the effects of each program for students who are most likely to choose to watch the DVD if presented with a choice, we instrument for watching the DVD in the *Family* treatment group to identify an effect for compliers and apply IPW to observations in the *Student* treatment to identify an effect for complier-like students. Table 10 presents the results of this exercise. For each outcome, there are four sets of results: the first replicates the simple OLS comparison for the subsample of students who have data relevant for the IV and IPW analysis; the second presents results where the *Student* treatment group has been reweighted, the third presents the IV results where we instrument for watching the DVD in the *Family* treatment and the fourth combines the IV and IPW approaches.

Across the board, the estimated coefficients for the *Student* treatment increase with reweighting and the estimated coefficients for the *Family* treatment increase after instrumenting. As a result, the effects of the treatment for compliers are larger for every outcome variable. And, while the joint test of whether $\hat{\alpha}_{IPW}$ and $\hat{\alpha}_{IV}$ are equal to zero is rejected at the 5% level in each case, for no outcome can we reject that $\hat{\alpha}_{IPW}=\hat{\alpha}_{IV}$. Students in both treatment groups are 6 percentage points more likely to report wanting to study at college and 5-6 percentage points more likely to report financing using scholarships and loans. Focusing on the results for the behavioral outcomes: watching the DVD in class or at home significantly reduces the number of days reported absent in the week before the survey by about one quarter; it reduces the number of days recorded absent by the school in the month before the survey by between 1.2 and 1.7 days. This represents a 35% and 75% reduction in number of days absent relative to the baseline mean, or between 0.2-0.6 of a standard deviation reduction in number of days absent. There are corresponding reductions in the fraction of students reporting any absenteeism of between 8 and 44% relative to baseline absenteeism.

Although the set of students who selected into watching the DVD under the *Family* treatment are arguably positively selected so that the effects on outcomes for these students are larger than for the average student in this group, once we adjust for this selection by reweighing individuals in the *Student* treatment, there is again no difference in the effects of watching the DVD in either the classroom or home environment. The intervention clearly had an effect on student reports of financial aid resources, on their knowledge of the financial aid process and a large impact on their effort in school. The results of this exercise suggest that having parents involved in the process does not seem to increase the size of these effects.

iv. Why parents may not matter for absenteeism: monitoring ability

There are three possibilities for why the *Student* and the *Family* treatments had significant effects on outcomes that were of the same magnitude: either parents in the *Student* treatment also obtained the information about the DVD because students talked about it at home, parents in the *Family* treatment did not actually watch the DVD, or even though parents saw the program, they did not learn from the information provided in the DVD or their involvement did not matter for the outcomes we focus on here.

Most (68%) of the students report watching the DVD with parents, so parents would have been exposed to the information contained in the program. In Table 11, we find evidence that tentatively rules out both of the first two reasons, at least for the set of parents who returned their surveys to the school. We present OLS results for a regression of parental responses to the financial aid eligibility questions on program assignment, with and without controls. Note that we do not control for baseline responses to these questions, since parents in the *Family* group may have watched the DVD before returning their surveys to school. The results show that parents in the *Student* group do not score significantly higher on the financial aid eligibility questions relative to control group parents, whereas parents in the *Family* treatment score between 0.17 and 0.3 points higher on these questions (columns 1 and 2).[43]

Although a large fraction of parents do not return their surveys to school at baseline and/or at follow up, we feel confident that sample selection is not driving the results. As shown in Appendix Table 1, parental attrition from the survey (at baseline and follow up) is effectively the same across *Student*, *Family* and control schools. We also explored whether attrition was statistically different within treatment groups by

[43] The DVD was designed to appeal to 14-15 years old. However, results in table 11 show that parents learned from the information provided on it.

7th grade scores: it was not. In fact, all observable characteristics of students and schools (used as controls in our prior results) were balanced across observations in treatment and control groups that have parental survey data at follow up. Under a standard monotonicity assumption on the process governing non-response for parents and given that we are analyzing data from a randomized experiment, comparison of outcomes across treatment and control groups provides a valid estimate of the impact of treatment on outcomes reported by parents (Lee, 2009). However, as a check, we also computed bounds for results on parental absenteeism reports in columns 4 to 6 of Table 11 (following the sharp bounds of Horowitz and Manski 1998). Since the bounds contain zero, we cannot reject that parental reports of financial aid knowledge at follow up are the same across all groups.

Even though parents in the *Family* treatment who returned their surveys have substantially better knowledge of the financial aid process than other parents who returned their surveys, neither parent group reports accurate information about their child's absenteeism behavior. Table 12 presents coefficients from OLS regressions of parent reports of child absence from school in the week before the survey (absent at all, number of days absent), as measured at follow up. We also present results computing the Manski bounds on these effects. Regardless of whether we use the subset of parents who responded or the bounded outcome variables, we find that neither *Student* treatment parents nor *Family* treatment parents report significantly lower absenteeism rates at follow up. The coefficients move around substantially in the bounded results; however, the bounds contain zero, so we cannot reject that parents report no change in absenteeism of their children.

Given the large improvements in absenteeism reported by students and recorded at schools, it is surprising that parents do not know more about their child's behavior. The conclusion we draw from this set of results is that parents may not be monitoring this form of school effort, either because they are not interested and do not try, or because they might find it difficult to monitor. This is one reason why we do not find larger effects for *Family* treatment relative to the *Student* treatment, even after adjusting for the characteristics of compliant types of students. Our finding echoes some of the findings of Burztyn and Coffman (2010), where they show that parents in urban areas of Brazil have a stated preference for cash transfers with conditionality tied to school attendance captured by schools (indirect monitoring), but are willing to relax this conditionality when they are provided with a feasible means of directly monitoring their children (text messages). [44]

7. Discussion

[44] Informal discussions with principals and teachers of schools like the ones in the sample before the intervention are in line with this result. Teachers reported that parents do not care about their children school performance.

Access to post secondary education in Chile is highly correlated with socioeconomic status of families, even though there are many loans and scholarships available for students in need. Eligibility for loans and scholarships depend on a combination of high school grades, scores on a national selection test (PSU) and financial need. However, learning about eligibility requirements is not easy, given the variety of financial aid programs available; and realizing that effort in high school matters for later outcomes may occur too late for some students. Our project experimentally manipulates exposure to information about financial aid opportunities and eligibility rules for post-secondary school education. The information was delivered by a standardized set of role models and implemented in the last year of primary school, the point at which choices about effort, schools and type of study start to define the student's post secondary opportunities.

There are two main findings in this paper. First, we find strong evidence that among Grade 8 students, this new information was retained 3 to 4 months after the intervention, with the largest impact on knowledge of loan finance. Although watching "*Abre la Caja*" had only a small impact on educational expectations (which are unrealistically high given the actual fraction of students that actually have after high school studies), it improved effort in school by reducing days of absenteeism by between 0.2 and 0.3 standard deviations. These large effects are evident in student self-reports and in school records and we find some evidence that students with medium and high scores at school are driving this behavioral response. These results show that information access can change behavior. Our paper adds to the growing body of evidence that a lack of information may generate underinvestment in human capital in poor countries (Jensen, 2010; Nguyen, 2009).

The second important finding is that the effects of being exposed to "*Abre la Caja*" in the classroom or at home are statistically indistinguishable from each other. Comparing the two treatment effects is not straightforward because of selection of students that watched the DVD in the *Family* treatment group and because of heterogeneity in the responses of students in both treatment groups, related to ability. We implement a variety of empirical strategies to make the results comparable, including a reweighting of the *Student* treatment group to make it comparable to the *Family* treatment compliers. The finding that the *Student* and *Family* treatments have the same impact on outcomes is robust to these alternative specifications.

The final part of our paper puts forward one reason for why parental involvement does not increase the size of the *Family* treatment effects: parents may not be able to directly observe the effort that we measure in school absenteeism. We show that parents in both *Student* and *Family* treatment groups do not report any significant changes in absenteeism behavior of their children, despite the strong evidence from kids and from schools that absenteeism did decline in these groups. Although we have a selected sample of parental surveys, bounding the results for missing data still does not allow us to reject zero effect of program assignment on parental absenteeism reports. It will be interesting to see (in future versions of the paper) whether the *Family* treatment does have a larger effect on realized school outcomes as reflected in Grade 8 final grades and in the 2009 Grade 8 SIMCE scores, as monitoring of time spent on study at home may be an easier task than monitoring school attendance.

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Figure 1

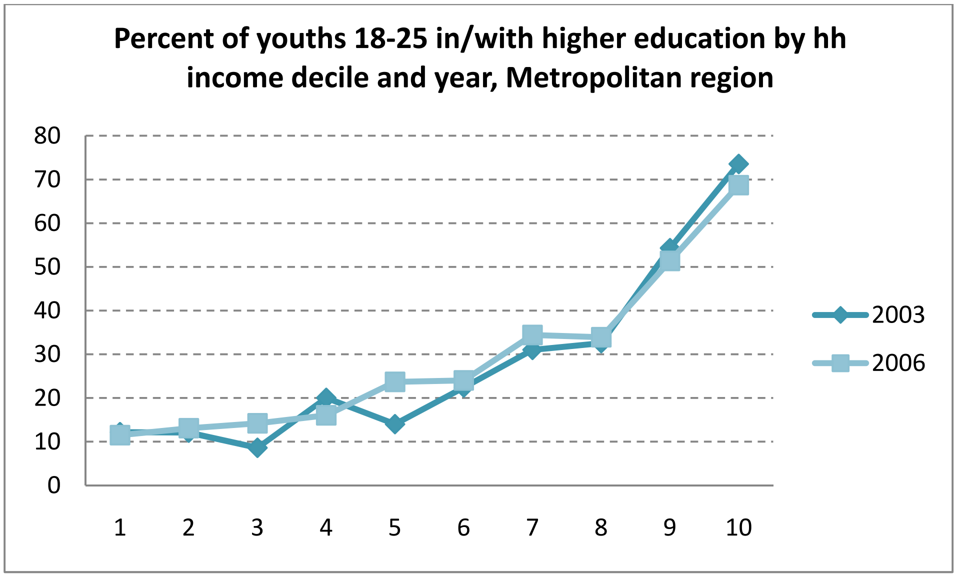


Figure shows the (sample weighted) fraction of young adults age 18-25 in the Metropolitan region of Chile who are enrolled in higher education (college or vocational schools) or who have graduated from some post secondary school, by decile of household per capita income. Data are from 2003 and 2006 CASEN.

Figure 2

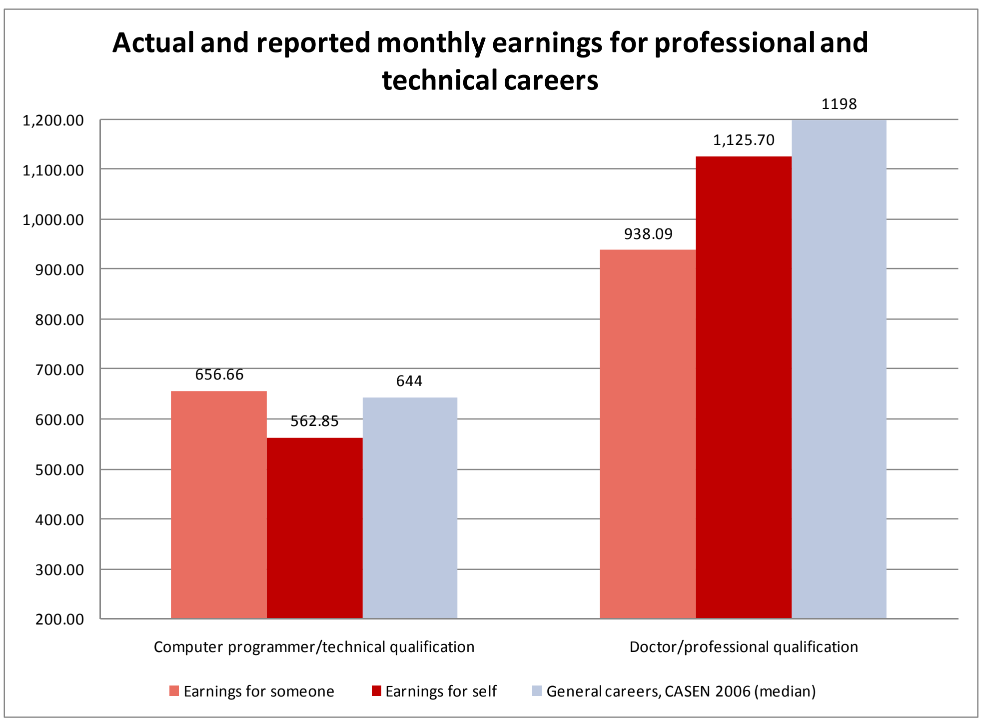
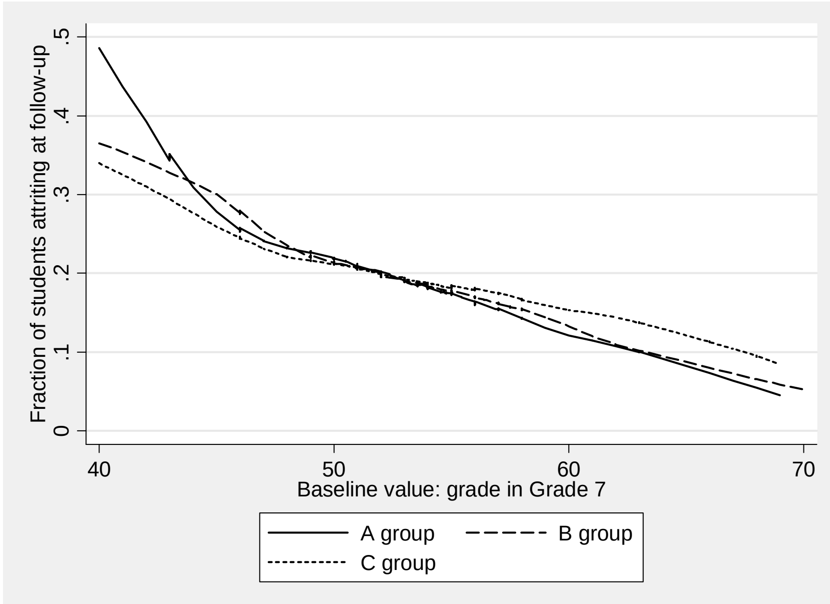


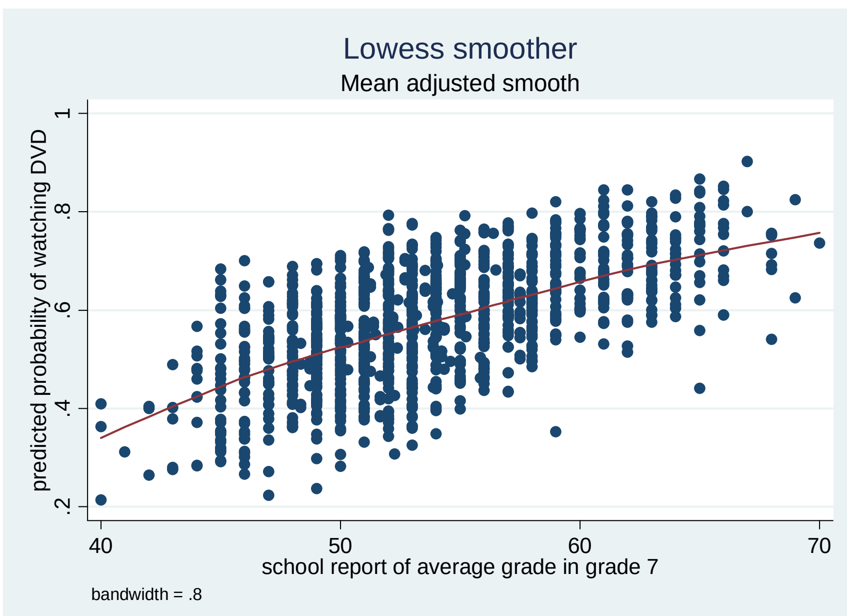
Figure shows median monthly earnings in USD (533 pesos=1 USD in 2009) for different levels of education. Light blue bars represent (weighted) median monthly earnings computed from actual data of adult workers age 30-40 years, who are either technically or professionally trained ("General Careers" from CASEN 2006) and who live in the Metropolitan region of Chile. Red bars are from student reports of what they think they would earn at the age of 30, if they worked as a computer programmer or doctor ("Earnings for self") or what they think someone of age 30 working as a computer programmer (or doctor) could earn ("Earnings for someone").

Figure 3: Grades at baseline and attrition of students at follow-up



Graph shows lowess-smoothed regressions of attrition at follow-up on values of Grade 7 grades that are recorded at baseline, separately for each of groups A, B and C. In a probit regression of attrition status on treatment assignment dummies, Grade 7 scores and interactions of A and B with Grade7 scores, attrition is not significantly different across A and B groups, nor for B group students of differing ability. For A group students of lower ability, the probability of attriting is marginally higher and significant at the 10% level.

Figure 4



Graph of predicted probabilities from probit of whether the student watches the DVD on a set of observable characteristics. The sample is restricted to students in group B only. Grades are measured at the end of grade 7, before our survey.

Table 1: Summary statistics

	Students present at baseline	Students present at follow up	Retention rate		
A: Student treatment	1,536	1,254	0.82		
B: Family treatment	1,518	1,195	0.79		
C: Control group	3,179	2,560	0.81		
Total students	6,233	5,009	0.80		

	N	Mean	s.d.	Min	Max
Student-level variables as baseline					
A: Student treatment	6,233	0.246	0.43	0	1
B: Family treatment	6,233	0.244	0.43	0	1
Age	6,233	13.98	0.85	11	18
Age missing	6,233	0.004	0.06	0	1
Female	6,233	0.47	0.50	0	1
Mother completed high school	6,233	0.52	0.50	0	1
Missing mother education indicator	6,233	0.15	0.35	0	1
School-reported grade 7 score~	6,233	53.54	5.39	40	70
School-reported grade 7 score, missing indicator~	6,233	0.12	0.32	0	1
Impatient (revealed preference)	6,233	0.52	0.50	0	1
Have a DVD player at home	6,166	0.90	0.31	0	1
DVD player is working	6,063	0.86	0.34	0	1
Student-level outcomes at baseline					
Will study beyond high school	5,918	0.76	0.43	0	1
Years will study after high school	5,934	2.13	2.57	0	10
Expected earnings at age 30 (2009 USD)	4,771	1,128.10	798.29	188	2,814
Will study at college	5,936	0.24	0.43	0	1
Will study at a TP school	5,936	0.32	0.47	0	1
Pay for studies w/ scholarships	6,151	0.31	0.46	0	1
Pay for studies w/ loans	6,151	0.09	0.28	0	1
Pay for studies w/ family resources	6,151	0.38	0.48	0	1
No idea how to pay for studies	6,151	0.47	0.50	0	1
Self-reported absenteeism (any, last week)	5,338	0.36	0.48	0	1
Self-reported absenteeism (days last week)	5,338	0.59	1.02	0	5
School-reported absenteeism (any, in May)~	4,066	0.56	0.50	0	1
School-reported absenteeism (days in May)~	4,066	1.87	2.77	0	18
Parent-reported absenteeism (any, last week)	4,393	0.27	0.45	0	1
Parent-reported absenteeism (days last week)	4,393	0.38	0.76	0	5
Has school absenteeism data~	6,233	0.70	0.46	0	1
School-level variables at baseline					
A: Student treatment (56 schools)	226	0.25	0.43	0	1
B: Family treatment (56 schools)	226	0.25	0.43	0	1
Combined treatment	226	0.50	0.50	0	1
Control schools (115 schools)	226	0.50	0.50	0	1
Fraction private voucher schools^	226	0.31	0.46	0	1
School poverty score (higher is poorer)^	226	46.42	9.08	26	80
School continues to grades 9-12	226	0.24	0.43	0	1
Stratum of SIMCE scores in 2007^	226	2.98	1.41	1	5

All student variables are measured at baseline and at follow-up. Means and standard deviations presented in this table are calculated over the baseline values only. ^ variables are measured in another survey, the 2007 SIMCE dataset. The SIMCE score is the combined school-averaged math and language scores on the 2007 Grade 8 SIMCE tests. We stratified the sample into 5 strata of these pre-intervention test scores. ~ variables are collected from school administrative records; two schools were unable to provide us with Grade 7 final scores. For control variables with missing values, we assigned students the mean value of the variable in the data, or a value of 0 for indicator variables, and created a missing indicator variable to flag these observations in all regressions. Missing values for outcome variables are not imputed: Appendix Table 1 contains an analysis of item non-response.

Table 2: Absenteeism at baseline and attrition at follow-up by Grade 7 scores: Probit marginal effects

	Student absent at baseline	Student absent at follow-up	Student absent at baseline or follow-up	Student absent at baseline	Student absent at follow-up	Student absent at baseline or follow-up
	(1)	(2)	(3)	(4)	(5)	(6)
A: Student	0.007	-0.010	-0.003	0.002	-0.073	-0.044
	(0.021)	(0.017)	(0.024)	(0.076)	(0.064)	(0.065)
B: Treatment	0.008	0.020	0.022	0.019	-0.022	0.007
	(0.018)	(0.026)	(0.026)	(0.067)	(0.062)	(0.057)
Grade 7 scores				-0.0316***	-0.0232***	-0.0332***
				(0.005)	(0.005)	(0.004)
A: Student*Grade 7 scores				-0.001	-0.015	-0.010
				(0.009)	(0.009)	(0.007)
B: Family*Grade 7 scores				0.003	-0.009	-0.001
				(0.010)	(0.010)	(0.009)
N	7,666	6,233	7,666	7,588	6,193	7,548

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parentheses, clustered at the school-level. All regressions contain stratum fixed effects that define the quintile of the SIMCE 2007 score distribution into which each school falls. Grade 7 scores are de-meaned, and a constant is included in the probit regression.

Table 3: Balance of variables at baseline - OLS regressions

Panel I: Student-level outcome variables	Reported financial resources for post-secondary education: Loan finance	Scholarship finance	Family finance	DNK how to finance	Education expectations: Think you will study beyond high school	Number years will study after h/s	Expected earnings at 30?	Will study in college?	Educational inputs: Self-reported absenteeism last week: any	Self-reported absenteeism last week: days	School records: absent at all in May	School records: days absent in May
A: Student	0.002	-0.006	0.023	-0.020	-0.015	0.048	33.576	-0.023	0.011	-0.011	-0.079	-0.260
	(0.016)	(0.011)	(0.019)	(0.020)	(0.018)	(0.100)	(33.764)	(0.020)	(0.021)	(0.044)	(0.049)	(0.382)
B: Family	0.019	0.005	0.007	-0.014	0.001	0.039	-31.893	-0.001	0.015	0.035	-0.064	-0.092
	(0.019)	(0.010)	(0.018)	(0.018)	(0.017)	(0.092)	(33.935)	(0.017)	(0.022)	(0.045)	(0.051)	(0.383)
Constant	0.310***	0.089***	0.368***	0.482***	0.765***	2.106***	1,127.638***	0.251***	0.354***	0.585***	0.601***	1.964***
	(0.010)	(0.006)	(0.011)	(0.011)	(0.009)	(0.054)	(19.471)	(0.011)	(0.012)	(0.024)	(0.028)	(0.233)
N	6,151	6,151	6,151	6,151	5,918	5,934	4,771	5,936	5,338	5,338	4,066	4,066
R2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00
Pval for joint test of A, B	0.57	0.67	0.48	0.53	0.68	0.85	0.25	0.28	0.74	0.66	0.20	0.79
Pval for test of A=B	0.71	0.82	0.46	0.79	0.44	0.94	0.10	0.47	0.86	0.74	0.79	0.15

Panel II: Student and school-level control variables	Demographics: Age	Female	Mother finished high school	Mother education missing	School reported grade 7 score	School reported grade 7 score missing	Ability: SIMCE score in grade 4	SIMCE score in grade 4, missing	Patient: revealed preference question	School variables: School baseline SIMCE scores, 2007	School is private voucher school	School poverty rank
A: Student	0.040	-0.034	-0.035	0.026*	-0.709**	0.028	-1.626	0.007	-0.047	0.835	0.023	0.994
	(0.036)	(0.025)	(0.022)	(0.014)	(0.296)	(0.032)	(2.067)	(0.020)	(0.036)	(2.359)	(0.075)	(1.369)
B: Family	0.022	-0.024	-0.008	0.001	-0.350	-0.001	-2.634	-0.008	0.034	-0.748	0.094	1.109
	(0.043)	(0.020)	(0.021)	(0.013)	(0.376)	(0.023)	(2.044)	(0.022)	(0.036)	(2.278)	(0.078)	(1.611)
Constant	13.969***	0.488***	0.535***	0.139***	53.802***	0.112***	235.750***	0.265***	0.521***	229.916***	0.281***	45.904***
	(0.022)	(0.013)	(0.012)	(0.008)	(0.190)	(0.010)	(1.216)	(0.012)	(0.018)	(1.341)	(0.042)	(0.840)
N	6,233	6,233	6,233	6,233	6,233	6,233	6,231	6,233	6,233	226	226	226
R2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.003
Joint test A, B: p-value	0.53	0.29	0.14	0.14	0.82	0.62	0.41	0.83	0.18	0.84	0.48	0.685
Test A=B: p-value	0.20	0.14	0.49	0.08	0.35	0.14	0.06	0.67	0.06	0.12	0.43	0.948

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. Sample size varies because of non-response by students; see Appendix Table 1 for analysis of item non-response.

Table 4: Immediate effects of viewing "*Abre la Caja* " on financial aid information and education expectations in group A

	Mean response at baseline	Mean response after DVD	Difference	Difference for low grade students	Difference for medium grade students	Difference for high grade students
	(1)	(2)	(3)	(4)	(5)	(6)
Will pay post-secondary school with scholarship/fellowship	0.312	0.370	0.058***	0.053***	0.066***	0.039*
N (s.e.)	3,031	3,031	(0.011)	(0.019)	(0.014)	(0.023)
Will pay post-secondary school with loans	0.093	0.176	0.093***	0.068***	0.096***	0.158***
N (s.e.)	3,031	3,031	(0.011)	(0.013)	(0.016)	(0.028)
Will pay post-secondary school using family resources	0.390	0.385	-0.005	0.001	-0.015	0.019
N (s.e.)	3,031	3,031	(0.009)	(0.016)	(0.012)	(0.019)
Do not know how I will pay for tertiary studies	0.431	0.355	-0.076***	-0.082***	-0.078***	-0.045*
N (s.e.)	3,031	3,031	(0.012)	(0.018)	(0.018)	(0.026)
Do you think you will continue with studies after high school?	0.751	0.785	0.034***	0.022	0.041***	0.039*
N (s.e.)	2,973	2,973	(0.010)	(0.019)	(0.011)	(0.021)
How many years do you think you will study after high school?	2.154	2.328	0.174***	0.242***	0.147**	0.084
N (s.e.)	2,947	2,947	(0.048)	(0.068)	(0.061)	(0.086)
Do you think you will study at college?	0.23	0.24	0.016*	0.02	0.014	0.018
N (s.e.)	2,356	2,356	(0.01)	(0.015)	(0.010)	(0.021)

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. Controls include age, age missing, female, mother completed high school, missing mother education, school-reported grade 7 score, school-reported grade missing, impatient and impatient missing indicator, school poverty rank and school poverty rank missing indicator, private-voucher school indicator and stratum fixed effects that define the quintile of the SIMCE 2007 score distribution that each school falls into. Item non-responses for outcome variables are not imputed. See Appendix Table 1 for analysis of item non-response by treatment assignment.

Differences by Grade 7 grades are estimated from a regression of the outcome variable on an "After" dummy, dummy variables for medium and high grade, and all interactions of these with "After", as well as all other controls.

Table 5: Effects of program assignment on reported financing methods and eligibility knowledge at follow-up

	Scholarship finance		Loan finance		Family finance		Don't know how to finance		Score on eligibility rules test (= 0 to 5)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Student	-0.009	0.004	0.037***	0.046***	0.000	-0.009	-0.006	-0.013	0.035	0.037
	(0.017)	(0.014)	(0.011)	(0.011)	(0.018)	(0.017)	(0.020)	(0.019)	(0.041)	(0.041)
B: Family	0.037*	0.034**	0.030***	0.031***	0.021	0.017	-0.051**	-0.049***	0.104**	0.100**
	(0.022)	(0.016)	(0.011)	(0.011)	(0.018)	(0.017)	(0.020)	(0.019)	(0.043)	(0.041)
Age		-0.005		0.002		-0.026***		0.017*		0.023
		(0.007)		(0.005)		(0.008)		(0.008)		(0.017)
Female		0.011		-0.003		-0.023*		0.026*		-0.022
		(0.012)		(0.008)		(0.013)		(0.015)		(0.029)
Mother complete HS		0.028**		0.014*		-0.005		-0.014		-0.045
		(0.012)		(0.008)		(0.014)		(0.016)		(0.032)
School grade 7 score		0.015***		0.005***		-0.002		-0.010***		0.005*
		(0.001)		(0.001)		(0.001)		(0.001)		(0.003)
Impatient		-0.008		-0.008		-0.001		0.003		-0.001
		(0.011)		(0.008)		(0.013)		(0.014)		(0.028)
School poverty rank		0.001		-0.001**		0.000		-0.001		-0.002
		(0.001)		0.000		(0.001)		(0.001)		(0.002)
Private voucher school		-0.008		0.015		0.005		-0.014		0.049
		(0.013)		(0.010)		(0.015)		(0.018)		(0.037)
Baseline value of dependent variable		0.379***		0.276***		0.307***		0.321***		
		(0.014)		(0.025)		(0.013)		(0.014)		
N	4,967	4,908	4,967	4,908	4,967	4,908	4,967	4,908	5,009	5,009
R2	0.003	0.245	0.004	0.089	0.003	0.119	0.003	0.136	0.002	0.011
Pval for joint test of A, B	0.149	0.094	0.001	0.000	0.473	0.433	0.040	0.032	0.051	0.051
Pval for test of A=B	0.058	0.091	0.608	0.236	0.316	0.204	0.061	0.102	0.163	0.196

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. All regressions contain stratum fixed effects that define the quintile of the SIMCE 2007 score distribution into which each school falls. Additional controls (coefficients not shown): missing indicators for age, mother education, school poverty rank, impatience, school-reported grade 7 score and a constant. Outcomes in column 1-8 are binary; eligibility rules score outcome is a score on a 0-5 scale. See Appendix Table 1 for analysis of item non-response for outcome variables by treatment group assignment.

Table 6: Effects of program assignment on educational expectations of students at follow-up

	Do you think you will study after high school?		Number of years you think you will study after high school?		Do you think you will study at a college?	
	(1)	(2)	(4)	(5)	(7)	(8)
A: Student	0.016	0.039**	-0.041	-0.023	0.005	0.026*
	(0.020)	(0.017)	(0.114)	(0.095)	(0.020)	(0.014)
B: Family	0.007	0.011	0.123	0.076	0.037*	0.042***
	(0.019)	(0.017)	(0.103)	(0.091)	(0.020)	(0.016)
Age		-0.022***		-0.027		-0.012**
		(0.008)		(0.045)		(0.006)
Female		0.084***		0.137*		0.043***
		(0.014)		(0.072)		(0.012)
Mom completed HS		0.045***		0.256***		0.037***
		(0.013)		(0.077)		(0.012)
School grade 7 score		0.008***		0.027***		0.010***
		(0.001)		(0.007)		(0.001)
Impatient		-0.005		-0.002		0.000
		(0.013)		(0.076)		(0.011)
School poverty rank		-0.001		-0.008*		-0.002**
		(0.001)		(0.004)		(0.001)
Private voucher school		0.018		0.174**		0.051***
		(0.016)		(0.087)		(0.014)
Baseline value of dependent variable		0.399***		0.443***		0.438***
		(0.017)		(0.016)		(0.016)
N	4,918	4,678	4,794	4,605	4,849	4,639
R2	0.004	0.194	0.002	0.215	0.012	0.241
Pval for joint test of A, B	0.725	0.070	0.366	0.612	0.190	0.018
Pval for test of A=B	0.712	0.121	0.200	0.363	0.195	0.351

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. All regressions contain stratum fixed effects that define the quintile of the SIMCE 2007 score distribution into which each school falls. Additional controls (coefficients not shown): missing indicators for age, mother education, school poverty rank, impatience, school-reported grade 7 score and a constant. Outcomes are binary. See Appendix Table 1 for analysis of item non-response in outcome variables.

Table 7: Effects of program assignment on effort at follow-up

	Self-reports				School reports			
	Absent last week at all		Days absent last week		Absent in October at all		Days absent in October	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Student	-0.029	-0.045*	-0.150*	-0.172**	-0.064	-0.050	-0.656	-0.619*
	(0.026)	(0.025)	(0.078)	(0.070)	(0.050)	(0.041)	(0.416)	(0.342)
B: Family	-0.023	-0.022	-0.144*	-0.132*	-0.128**	-0.113**	-0.719	-0.736**
	(0.028)	(0.028)	(0.077)	(0.068)	(0.051)	(0.044)	(0.440)	(0.348)
Age		0.030***		0.085***		0.020**		0.262***
		(0.010)		(0.025)		(0.008)		(0.064)
Female		0.030**		0.015		0.010		-0.094
		(0.015)		(0.036)		(0.015)		(0.108)
Mother completed HS		0.004		-0.052		0.021		-0.059
		(0.016)		(0.038)		(0.015)		(0.108)
School grade 7 score		-0.007***		-0.015***		-0.006***		-0.041***
		(0.001)		(0.004)		(0.002)		(0.012)
Impatient		0.009		0.036		0.000		-0.110
		(0.016)		(0.037)		(0.019)		(0.166)
School poverty rank		0.000		0.000		-0.002		0.009
		(0.001)		(0.003)		(0.002)		(0.011)
Private voucher school		-0.115***		-0.418***		-0.187***		-1.373***
		(0.023)		(0.074)		(0.038)		(0.278)
Baseline value of dep. Var.		0.182***		0.169***		0.272***		0.443***
		(0.016)		(0.025)		(0.024)		(0.047)
N	4,577	4,028	4,577	4,028	3,970	3,970	3,970	3,970
R2	0.004	0.063	0.007	0.065	0.018	0.147	0.013	0.211
Pval for joint test of A, B	0.486	0.196	0.110	0.039	0.041	0.035	0.180	0.074
Pval for test of A=B	0.840	0.462	0.937	0.344	0.279	0.188	0.886	0.738

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. All regressions contain stratum fixed effects that define the quintile of the SIMCE 2007 score distribution into which each school falls. Additional controls (coefficients not shown): missing indicators for age, mother education, school poverty rank, impatience, school-reported grade 7 score and a constant. Analysis of item non-response in Appendix Table 1. School-reported absenteeism data available for 70% of students.

Table 8: Predictors of DVD-watchers in Family treatment: Probit marginal effects

	Student watched the DVD?
Age	-0.039*
	(0.022)
Female	0.051*
	(0.031)
Mom completed HS	-0.016
	(0.035)
School grade 7 score	0.013***
	(0.003)
Impatient	-0.021
	(0.036)
School poverty rank	-0.004**
	(0.002)
Private voucher school	0.109**
	(0.044)
N	1,194

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. All regressions contain stratum fixed effects that define the quintile of the SIMCE 2007 score distribution into which each school falls. Additional controls (coefficients not shown): missing indicators for age, mother education, school poverty rank, impatience, school-reported grade 7 score and a constant. Coefficients are estimated marginal effects from the probit.

Table 9: Effects of program assignment by baseline student ability

	Financial aid information					Education expectations		Education inputs	
	Scholarships	Loans	Family	No idea	Eligibility rules score	Will study after hs?	Will study at a college?	Absent in October (school report)	Days absent in October (school report)
	(1)	(2)	(3)	(4)	(3)	(4)	(5)	(6)	(7)
Medium grade	0.056***	0.012	0.003	-0.050*	0.064	0.056**	0.027	0.024	0.081
	(0.018)	(0.011)	(0.019)	(0.025)	(0.044)	(0.024)	(0.019)	(0.023)	(0.242)
High grade	0.207***	0.057***	0.014	-0.159***	0.105	0.148***	0.121***	-0.112**	-0.893**
	(0.028)	(0.019)	(0.026)	(0.029)	(0.065)	(0.030)	(0.028)	(0.045)	(0.389)
Student*low grade	-0.005	0.031*	0.038	-0.04	0.106*	0.052*	-0.006	-0.011	-0.117
	(0.021)	(0.018)	(0.023)	(0.029)	(0.057)	(0.029)	(0.024)	(0.054)	(0.515)
Family*low grade	0.011	0.022	0.048*	-0.049	0.023	0.005	0.002	-0.09	-0.45
	(0.023)	(0.016)	(0.027)	(0.030)	(0.067)	(0.031)	(0.024)	(0.059)	(0.558)
Student*medium grade	0.006	0.050***	-0.026	-0.005	-0.003	0.037*	0.034*	-0.093	-0.946**
	(0.019)	(0.016)	(0.022)	(0.026)	(0.053)	(0.022)	(0.019)	(0.057)	(0.418)
Family*medium grade	0.018	0.019	0.015	-0.032	0.133**	0.016	0.047**	-0.147***	-0.908**
	(0.023)	(0.013)	(0.023)	(0.025)	(0.053)	(0.022)	(0.022)	(0.055)	(0.445)
Student*high grade	0.004	0.054	-0.046	0.036	0.042	0.019	0.062	-0.10	-0.953**
	(0.037)	(0.034)	(0.035)	(0.038)	(0.105)	(0.033)	(0.039)	(0.075)	(0.478)
Family*high grade	0.149***	0.097***	-0.041	-0.121***	0.123	0.001	0.104**	-0.098	-0.577
	(0.037)	(0.034)	(0.039)	(0.036)	(0.104)	(0.034)	(0.041)	(0.066)	(0.497)
N	4,908	4,908	4,908	4,908	5,009	4,678	4,639	4,086	4,086
R2	0.24	0.09	0.12	0.14	0.01	0.19	0.24	0.03	0.03

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. All regressions contain stratum fixed effects that define the quintile of the SIMCE 2007 score distribution into which each school falls. High grade = 1 if Grade 7 score above 60; medium grade = 1 if Grade 7 score between 50 and 60; low grade = 1 if Grade 7 score below 50. All other controls controls included.

Table 10: Effects of program assignment on outcomes, IV and reweighting

Panel I	Financing methods: Scholarships				Loans				Knowledge of eligibility rules from DVD (Max score =5)			
	OLS	Reweight A	IV B	Reweight A and IV B	OLS	Reweight A	IV B	Reweight A and IV B	OLS	Reweight A	IV B	Reweight A and IV B
A: Student	-0.010	0.053***	-0.010	0.053***	0.037***	0.060***	0.037***	0.060***	0.034	0.062	0.034	0.063
	(0.017)	(0.019)	(0.017)	(0.019)	(0.011)	(0.013)	(0.011)	(0.013)	(0.041)	(0.045)	(0.041)	(0.045)
B: Family	0.037*	0.036*	0.061*	0.061*	0.030***	0.030***	0.050***	0.050***	0.104**	0.105**	0.176**	0.177**
	(0.022)	(0.022)	(0.036)	(0.036)	(0.011)	(0.011)	(0.018)	(0.018)	(0.043)	(0.043)	(0.069)	(0.070)
N	4,966	4,966	4,966	4,966	4,966	4,966	4,966	4,966	5,008	5,008	5,008	5,008
R2	0.003	0.005	0.006	0.007	0.004	0.009	0.005	0.009	0.002	0.002	0.006	0.006
Pval for joint test of A, B	0.144	0.010	0.138	0.010	0.001	0.000	0.001	0.000	0.051	0.038	0.043	0.033
Pval for test of A=B	0.054	0.505	0.047	0.841	0.604	0.046	0.483	0.623	0.160	0.414	0.042	0.108

Panel II	Future education expectations: Study after high school?				Num. of years of study				Will study in college			
	OLS	Reweight A	IV B	Reweight A and IV B	OLS	Reweight A	IV B	Reweight A and IV B	OLS	Reweight A	IV B	Reweight A and IV B
A: Student	0.015	0.065***	0.015	0.065***	-0.039	0.216*	-0.039	0.216*	0.005	0.067***	0.005	0.067***
	(0.020)	(0.021)	(0.020)	(0.021)	(0.114)	(0.130)	(0.114)	(0.131)	(0.020)	(0.024)	(0.020)	(0.024)
B: Family	0.007	0.007	0.012	0.012	0.123	0.125	0.206	0.210	0.037*	0.037*	0.061*	0.062*
	(0.019)	(0.019)	(0.031)	(0.031)	(0.103)	(0.104)	(0.170)	(0.172)	(0.020)	(0.020)	(0.033)	(0.034)
N	4,917	4,917	4,917	4,917	4,793	4,793	4,793	4,793	4,848	4,848	4,848	4,848
R2	0.004	0.009	0.005	0.010	0.002	0.005	0.005	0.007	0.012	0.018	0.016	0.021
Pval for joint test of A, B	0.732	0.007	0.732	0.007	0.371	0.190	0.361	0.188	0.191	0.012	0.182	0.012
Pval for test of A=B	0.721	0.014	0.922	0.097	0.206	0.528	0.157	0.975	0.198	0.277	0.099	0.878

Panel III	Self-reported absenteeism: Absent last week?				Num. days absent last week?			
	OLS	Reweight A	IV B	Reweight A and IV B	OLS	Reweight A	IV B	Reweight A and IV B
A: Student	-0.030	-0.071**	-0.030	-0.071**	-0.151*	-0.256***	-0.151*	-0.256***
	(0.026)	(0.028)	(0.026)	(0.028)	(0.078)	(0.079)	(0.078)	(0.079)
B: Family	-0.023	-0.023	-0.037	-0.037	-0.144*	-0.145*	-0.235*	-0.236*
	(0.028)	(0.028)	(0.046)	(0.046)	(0.077)	(0.078)	(0.125)	(0.125)
N	4,576	4,576	4,576	4,576	4,576	4,576	4,576	4,576
R2	0.00	0.01	0.00	0.01	0.01	0.01	0.01	0.01
Pval for joint test of A, B	0.48	0.04	0.48	0.04	0.11	0.01	0.11	0.01
Pval for test of A=B	0.83	0.14	0.87	0.46	0.92	0.11	0.40	0.84

Panel IV	School reports of absenteeism: Absent in October?				Num. days absent in October?			
	OLS	Reweight A	IV B	Reweight A and IV B	OLS	Reweight A	IV B	Reweight A and IV B
A: Student	-0.083	-0.123**	-0.063	-0.104**	-0.987**	-1.251***	-0.703*	-0.970**
	(0.050)	(0.051)	(0.051)	(0.052)	(0.412)	(0.402)	(0.411)	(0.401)
B: Family	-0.121**	-0.121**	-0.230**	-0.230**	-0.711	-0.709	-1.251*	-1.247*
	(0.051)	(0.051)	(0.089)	(0.089)	(0.433)	(0.433)	(0.724)	(0.724)
N	3,897	3,897	3,317	3,317	3,897	3,897	3,317	3,317
R2	0.02	0.02	0.01	0.02	0.02	0.03	0.02	0.03
Pval for joint test of A, B	0.04	0.01	0.04	0.02	0.05	0.01	0.14	0.05
Pval for test of A=B	0.52	0.98	0.06	0.15	0.52	0.18	0.39	0.65

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level.
There are no additional controls in these regressions. Reweighted regressions include inverse probability weights as described in the text.
Instrumented regressions instrument for whether students reported watching the DVD at home, using assignment to group B as the instrument.
School reported absenteeism data are for 70% of students. See Appendix Table 1 for analysis of item non-response.

Table 11: How much do parents remember from the DVD?

	Score on eligibility rules test (= 0 to 5)		Upper bound score		Lower bo
	(1)	(2)	(3)	(4)	(5)
A: Student	0.023	-0.016	1.113***	2.159***	-1.124***
	(0.037)	(0.043)	(0.085)	(0.093)	(0.080)
B: Family	0.332***	0.179***	1.443***	2.176***	-0.911***
	(0.038)	(0.056)	(0.092)	(0.110)	(0.086)
All controls included?	N	Y	N	Y	N
N	3,860	2,884	5,009	5,009	5,009
R2	0.02	0.01	0.18	0.34	0.11
Pval for joint test of A, B	0.00	0.00	0.01	0.90	0.00
Pval for test of A=B	0.00	0.00	0.00	0.00	0.00

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. All regressions stratum fixed effects that define the quintile of the SIMCE 2007 score distribution into which each school falls.See A Table 1 for analysis of item non-response for outcome variables by treatment group assignment.

Table 12: Effect of program assignment on parental reports of absenteeism

	Chile was absent at all last week?		Number of days child was absent for last week?					
	Parent report		Parent report		Upper bound		Lower bound	
A: Student	-0.021	-0.024	-0.045	-0.048	-2.262***	-2.278***	2.328***	2.309***
	(0.022)	(0.023)	(0.063)	(0.062)	(0.092)	(0.089)	(0.113)	(0.112)
B: Parent	-0.001	0.007	-0.06	-0.034	-2.259***	-2.257***	2.147***	2.158***
	(0.024)	(0.024)	(0.057)	(0.054)	(0.092)	(0.092)	(0.133)	(0.131)
Other controls?	N	Y	N	Y	N	Y	N	Y
N	2,697	2,697	2,697	2,697	4,577	4,577	4,577	4,577
R2	0.00	0.02	0.00	0.02	0.29	0.30	0.29	0.30
Pval for joint test of A, B	0.62	0.46	0.57	0.71	0.00	0.00	0.00	0.00
Pval for test of A=B	0.46	0.25	0.80	0.82	0.95	0.65	0.29	0.37

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level.

Bounds are computed following Horowitz and Manski (1998, 2000a).

Appendix Table 1: Item non-response at follow-up

ITEM NON-RESPONSE: indicator for missing values in outcome variables

	Want to study after high school		Number of years want to study after high school		Scholarship finance		Loan finance		Family finance		No idea how to finance		Absent at all last week (self report)		Number of days absent last week (self report)	
	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up
A: Student	-0.023***	-0.005	-0.005	-0.002	0.001	-0.001	0.001	-0.001	0.001	-0.001	0.001	-0.001	0.016	0.006	0.016	0.006
	(0.006)	(0.004)	(0.009)	(0.007)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.014)	(0.015)	(0.014)	(0.015)
B: Treatment	-0.002	0.000	-0.009	0.009	-0.002	0.001	-0.002	0.001	-0.002	0.001	-0.002	0.001	0.005	-0.015	0.005	-0.015
	(0.007)	(0.006)	(0.006)	(0.008)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.013)	(0.016)	(0.013)	(0.016)
Constant	0.057***	0.020***	0.052***	0.041***	0.014***	0.009***	0.014***	0.009***	0.014***	0.009***	0.014***	0.009***	0.138***	0.088***	0.138***	0.088***
	(0.005)	(0.003)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.008)	(0.009)	(0.008)	(0.009)
N	6,233	5,009	6,233	5,009	6,233	5,009	6,233	5,009	6,233	5,009	6,233	5,009	6,233	5,009	6,233	5,009
R2	0.002	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.000	0.001
F test A,B = 0	8.061	0.701	0.21	1.568	0.495	0.248	0.495	0.248	0.495	0.248	0.495	0.248	0.494	1.381	0.494	1.381
Pval joint test, A, B=0	0.005	0.403	0.648	0.212	0.482	0.619	0.482	0.619	0.482	0.619	0.482	0.619	0.483	0.241	0.483	0.241

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. Outcome is 1 if variable value is missing, =0 if not.

Appendix Table 2: Comparison of schools in experimental sample with other non-experimental schools

	Mean (sd) in experimental schools	Mean (sd) in non-experimental schools (same SES, Metro region)	Mean (sd) in non-experimental schools (same SES, all Chile)	(1) - (2)	(1) - (3)
	(1)	(2)	(3)		
SIMCE Grade 8 score in 2007 (math + language)	230.02	230.95	233.91	-0.94	-3.893***
	(14.20)	(17.52)	(16.12)	(1.46)	(1.13)
	227	264	1,422		
Lowest SES	0.07	0.05	0.17	0.02	-0.0994***
	(0.26)	(0.22)	(0.38)	(0.02)	(0.03)
	227	264	1,423		
Second lowest SES group	0.93	0.95	0.83	-0.02	0.0994***
	(0.26)	(0.22)	(0.38)	(0.02)	(0.03)
	227	264	1,423		
School poverty score (higher is poorer)	46.42	46.03	48.09	0.39	-1.670**
	(9.23)	(9.17)	(10.69)	(0.87)	(0.76)
	219	234	1,361		
Number grade 8 classes	1.77	1.75	1.69	0.02	0.08
	(0.85)	(0.86)	(0.80)	(0.08)	(0.06)
	227	264	1,423		
Number grade 8 kids	57.63	56.38	49.31	1.26	8.320***
	(33.63)	(33.82)	(29.43)	(3.05)	(2.15)
	227	264	1,423		
Grade 8 class size	31.92	31.23	28.28	0.69	3.640***
	(6.00)	(8.27)	(8.32)	(0.66)	(0.58)
	227	264	1,423		
Mother education	8.63	9.14	8.77	-0.51	0.13
	(3.46)	(3.30)	(3.46)	(0.33)	(0.26)
	199	225	1,268		
Father education	9.19	9.21	8.80	-0.02	0.06
	(3.38)	(3.50)	(3.64)	(0.34)	(0.28)
	191	220	1,212		
Households with DVD	0.77	0.76	0.69	0.01	0.0810***
	(0.09)	(0.10)	(0.13)	(0.01)	(0.01)
N	227	264	2,979		

Column (1) includes only experimental schools

Column (2) includes all non-experimental schools in the lowest 2 income groups in urban areas of the Metropolitan region in Chile.

Column (3) includes all non-experimental schools in the lowest 2 income groups in urban areas across the entire country (Metropolitan and other regions)

In the final two columns, significant differences represented by *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix: Survey questions

Main questions	Baseline	Follow up
How do you plan to pay for your studies after high school? (Mark all alternatives that are relevant)	Scholarship/fellowships Loans Parents/other family No idea how I will pay	Scholarship/fellowships Loans Parents/other family No idea how I will pay
As it stands now, do you plan to continue studying after high school?	Yes No, why not? Do not know	Yes, what will you study? No, why not? Do not know
How many years do you think you will study after high school?	Number of years Do not know	Number of years Do not know
How much time do you spend doing homework on a typical school day?	I don't study Less than 1 hour 1 hour 2 hours 3 hours 4 hours More than 4 hours	I don't study Less than 1 hour 1 hour 2 hours 3 hours 4 hours More than 4 hours
How many days did you miss class last week? (Monday to Friday)	Number of days Do not know	None 1 day 2 days 3 days 5. 4 days 5 days I don't know