Investing in schooling in Chile: The role of information about financial aid for higher education

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This paper investigates whether informing 8th graders about financial aid for higher education improves school outcomes, and whether informing parents magnifies these impacts. We evaluate a field experiment that exposed Chilean adolescents and a subset of parents to such information. In both treatment groups, financial aid knowledge improves, enrollment in college-preparatory high schools rises among students attending terminal primary schools, and absenteeism falls. Parental exposure to information does not magnify any behavioral effects. These results demonstrate that new information about how to pursue higher education goals can affect important school choices and raise student effort, regardless of parental involvement.

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Financial aid programs for higher education are prevalent in rich and poor countries, and despite academic and policy debate about the effects of this aid, these programs are often touted to broaden access to higher education and to promote socioeconomic mobility.[2] However, students who benefit from such financial aid programs are likely to be the ones who are better prepared for it: they know this aid exists, they can figure out how to apply for it and they can meet the eligibility requirements. Since academic preparation for aid eligibility is often cumulative, lack of awareness of different channels of financial support could undermine human capital investment decisions as a child progresses through school.[3] This lack of awareness could be one important barrier to education in developing countries undergoing education transitions, where parents have attained far less education than their children. In such a setting, parents may have more difficulty in helping adolescents make informed choices about schooling and assisting them in taking advantage of available resources for further education.

In this paper, we ask whether child effort in Grade 8 and high school enrollment choices respond to new information about financial aid for post-secondary education in Chile. Chile has achieved massive increases in secondary schooling in the past two decades but still struggles

[2] Angrist 1993, Dynarski 2002 and 2003, Bound and Turner 2002 and Kaufmann 2008 provide evidence for credit constraints. Carneiro and Heckman 2002 argue that early-life disadvantages cumulate for students from low socioeconomic backgrounds, and that the resulting lack of preparedness for college entry is a more binding constraint. Meneses and Blanco (2010) and Solis (2010) use IV and RD research designs to show that the probability of enrolling in Chilean colleges increases by up to 30% after the award of financial aid. These results indicate the presence of substantial credit constraints for poor families in Chile.

[3] Concerns about low awareness of public financial aid and the complex applications procedures in the USA have motivated recent work exploring whether direct assistance with applying for financial aid just prior to college enrollment can improve uptake of this financial aid among disadvantaged households in the US (Bettinger, Long, Oreopolous and Sanbonmatsu 2009).

with extreme inequality in access to higher education. In 2009, only 16% of 18-24 year olds from the poorest households were enrolled in tertiary education, compared with 61% of households in the top income decile (CASEN, 2009). Recent expansions in public financial aid for post-secondary schooling combined with limited experience of higher education among low income parents makes Chile a good context in which to ask: does a particular source of imperfect information about the net returns to higher education –how to go about financing tuition – lead students from poor backgrounds to choose specific school paths and perhaps under-invest in schooling four years *before* they apply for higher education?

To address this question, we randomly assigned over 6,000 8th graders in 226 poor urban schools, and some of their parents, to receive (or not receive) a short DVD program providing information about financial aid opportunities. In this program, young adults from similarly poor backgrounds discuss their trajectories towards college or vocational school and provide specific details on how high school performance affects eligibility for the new sources of financial aid. We interpret exposure to the information DVD as providing students with a better understanding of the higher education production function. We collect survey data from students at baseline and follow-up survey data, and match this to school and education ministry administrative data on outcomes to examine the short-run response of information sets, expectations and education behaviors: high school choice, school grades at the end of Grade 8 and absenteeism. To learn more about the characteristics of marginal students, we explore how exposure to this new information interacts with baseline school grades, one proxy for student ability.

A novel aspect of our work which is relevant for education research more generally is that we test whether providing information to children and parents together is substantially more effective at changing behavior than providing information to children alone. We provided the

DVD in two ways: in the *Student* treatment group, all students watched the DVD at school while in the *Family* treatment group, instead of watching at school, each student was given a copy of the DVD to watch at home with parents. We test for differences in effect sizes between these two different ways of delivering the information. To our knowledge, this is the first study to test whether parent learning about the education production function interacts with adolescent learning about this process to affect treatment response in a randomized experiment setting.[4]

Our *Intent to Treat* comparisons reveal that information presented in the DVD "sticks" (information treatment students score 5.5 percent higher on a test of financial aid information) and that exposure shifts reports of how students plan to pay for higher education, e.g. increasing the fraction of students planning to use loan finance, the newest form of financial aid, by almost 50%. Consistent with a model in which students of higher ability are more likely to be marginal when information about expected net returns to higher education change, we show that these information effects are larger for students with medium and high baseline grades, and that students with higher baseline grades are somewhat more likely to expect to attend college (rather than vocational training) after high school.

[4] Few papers step away from a unitary model of decision-making about educational investments to test whether parents or students are the more important agents in determining outcomes. Berry (2009) is an exception; he uses a field experiment to test for whether the identity (parent or child) of the recipient of a cash incentive matters for the effects of a learning incentive scheme in India. Using non-experimental methods, Attanasio and Kaufmann (2009) show that mothers' and adolescents' expectations are predictive of the high school attendance decision while for college attendance choices, only adolescent expectations matter. Giustinelli (2010) uses a structural model and survey data from Italian students to test whether child or parent beliefs and preferences over future outcomes are more important in determining school curriculum choice.

Treatment exposure also has important impacts on education behaviors. For the set of students required to choose a new school in Grade 9 (because their primary school terminates in Grade 8), we find that exposure to treatment raises the fraction of students attending a college-preparatory high school by 11-18%. This is a notable effect, since alternative forms of information provision to parents linked more directly to school quality have so far not had great success in promoting school choice, especially for lower SES families in Chile (Mizala and Urquiola 2007).[5] The enrollment choice results suggest that students responded to the financial aid information by choosing a school trajectory more likely to lead to college, as vocational high schools typically do not prepare students for any post-secondary education.[6] It also shows that when there is no "default" option for high school continuation, and families must actively choose where to enroll, providing early information about the importance of high school performance could be critical in changing education and career trajectories.

Exposure to the information DVD also significantly lowers monthly absenteeism among treated students: absenteeism declines by 14% (0.08 standard deviations) three months after baseline. Although we do not have power to detect small heterogeneous effects by test scores, the pattern of coefficients is suggestive: students with medium and high grades at baseline appear

[5] Mizala and Urquiola (2007) find that school choice does not appear to respond to new information about school quality in Chile (measured as whether a school wins a prestigious teaching quality award). In a different setting, providing parents with school and child "report cards" in rural Pakistan did lead to large changes in the education market and pressure on schools to adjust quality or school tuition, but little movement across schools in equilibrium (Andrabi, Das and Khwaja 2010). In the US, school report cards did impact school choice among low income parents (Hastings, Van Weelden and Weinsten 2007).

[6] 45% of students enrolled in college-preparatory high schools enrolled in some form of higher education in 2006; the comparable number for students enrolled in vocational high schools was 14% (MINEDUC 2010).

to drive this absenteeism effect. The size of the absenteeism responses is within the range of effects from other studies measuring the impact of information or of health interventions on attendance.[7] And, like several of these studies, we find that reduced absenteeism does not translate into improved test scores at the end of Grade 8. Since test scores are likely difficult to change in the short run and since the absenteeism effect is equivalent to an additional 2.5 days of school attendance over the entire school year, this lack of impact on test scores is not surprising. However, if this increased attendance is sustained, there may be long-term improvements in school (or other) outcomes for these students that will not be captured here.[8]

Somewhat surprisingly, our *Intent to Treat* comparison of the *Student* and *Family* treatments shows that parental exposure does not substantially magnify any of the behavioral impacts for these students. We can reject differences in effect sizes of 0.11 to 0.13 standard deviations, depending on the outcome. We do this comparison in several ways: estimating *Intent to Treat* effects, instrumenting for watching the DVD at home to identify the local average treatment effect for DVD watchers, and reweighting the sample of students in the *Student* treatment to make them comparable to DVD watchers in the *Family* treatment. Regardless of the comparison method used, we cannot reject the null that watching the DVD in school (without parents) and watching the DVD at home (with parents) have equivalent impacts. This is in spite

[7] Nguyen (2008) finds attendance gains from providing students in rural Madagascar with returns to education information, Bobonis et al (2006) study the effects of deworming and providing iron supplementation on attendance and other outcomes among Indian students, Kremer and Miguel (2004) see attendance rise after providing deworming medication to Kenyan students.

[8] Berthelon and Kruger (2011) show lower teen pregnancy resulting from a longer school day in Chile, Baird et al (2011) find short run attendance gains, no change in test scores, and long run wage gains for workers exposed to childhood deworming treatments in Kenya.

of the fact that, as we show, parents in the exposed *Family* treatment report knowing significantly more about financial aid after watching the DVD.

We draw two implications of our results for the broad literature on how different kinds of information (or lack of information) affect human capital investment decisions. Recent studies in poorer, rural settings have shown that providing a small amount of information about Mincer returns to education has large impacts on school attendance, test scores, school continuation and school completion.[9] In contrast, field experiments in richer countries have shown that information about financial aid for higher education is insufficient for raising enrollment in higher education.[10] Other researchers have shown mixed results on the impact of providing parents with information about relative school performance (Mizala and Urquiola 2007, Hastings and Weinstein 2008, Andrabi et al 2010). The results of our experiment fall between these developing and developed country results, with information about financial aid for higher education affecting school enrollment choices in Grade 9 and attendance in Grade 8, but having

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[9] Jensen (2010) tests the impact of providing information on the Mincer returns to different levels of education on educational attainment to 8th Graders in rural Dominican Republic (DR); Nguyen (2008) performs a similar test providing information on wages associated with different levels of education to parents of 3rd Graders in rural Madagascar, with or without role models. Both randomized experiments find large positive effects of providing this information on school investments as measured by school attendance (3.5% reduction in absenteeism in Madagascar), performance on tests (0.2 standard deviations in Madagascar), future school enrollment (7% higher in the DR the year after) and total educational attainment (0.2 years more schooling in the DR).

[10] Field experiments in the US (Bettinger, Long, Oreopoulos and Sanbonmatsu 2009) provide high school graduates and/or parents with information about and assistance with college financial aid applications close to the time of high school graduation. Bettinger et al (2009) find large increases in rates of college application and enrollment among families who were assisted with completing complex financial aid forms but no impacts of an information only treatment.

no impact on test scores in the short run. In the context of this broader literature, our study implies that different kinds of information will matter for different sorts of education outcomes, and the impact of providing more complete information – whether this is about wage returns or financial aid or school quality – is context-dependent. In the case of urban Chile, students were aware of the wage returns to higher education at baseline but were much less clear about how they were going to achieve their higher education goals. Our intervention targeted this particular information gap.

A second implication is that policy-makers and academics may want to consider who the relevant agent is for decision-making about human capital investments. Much academic work starts from the assumption that parents are most important for education choices, and many public resources are invested in providing parents with information to enable better decision-making (e.g. student report cards, information about school rankings). Yet, our results indicate that for some types of school effort choices, adolescents may be able to act as their own agents.[11] It is beyond the scope of this paper to investigate why providing parents with information did not have a larger impact on outcomes but future research into these reasons could inform our standard theories of how human capital investment decisions are made within the household.[12]

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[11] Several strands of research in economics take as a starting point that adolescents may not be sufficiently forward-looking to incur the immediate effort costs associated with certain investments (Gruber 2001, Dobbie and Fryer 2010); one implication of this is that child and parent should be treated as separate agents with conflicting preferences in analyzing the school investment decision (for examples, see Kalenkoski 2007, Berry 2009, Lundberg et al 2009).

[12] Avvisati et al (2010) present evidence from a randomized experiment in poor French schools suggesting that programs which actively train parents in how to become involved in their child's schooling can have large positive

The paper begins with an overview of the Chilean education system and our intervention: sample selection and randomization, the characteristics of the information treatment and expected effects. We then outline the empirical strategy and discuss different ways of comparing the *Student* and *Family* treatments where only a subset of students select in to watching the DVD in the *Family* treatment group. Then, we present results for student information sets and education expectations to indicate that students do actually learn from the DVD. We go on to present main results for education behaviors for all students and for students with different baseline grades. Finally, we present results comparing the *Student* and *Family* effects and conclude with a discussion.

1. Background to education in Chile

Chilean children start school at age six and must complete twelve years of education, usually eight years of primary school and four years of high school. During their school careers, students and their parents face several important choices about educational investments: they must choose how hard to work in school, how often to attend, and in which schools to enroll. A well-known school voucher program allows students to attend either free municipal schools or private voucher-subsidized schools that charge tuition.[13] The choice about which high school to attend is

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impacts on behavior in school and on literacy outcomes. These training programs go far beyond providing parents with information relevant to school quality and school choice.

[13] School choice and school outcomes in Chile have been the focus of several important studies because of the national school voucher system introduced in 1981 (Hsieh and Urquiola 2006, Mizala and Urquiola 2007, Urquiola and Verhoogen 2009, Bravo, Mukhopadhyay and Todd 2010). These vouchers established a national education market by allowing families to send children to any public school, thereby introducing competition among schools that was intended to incentivize school quality improvements. If public schools are selective in admissions, as some

made at the end of Grade 8, at which time students may choose to enroll in a traditional (humanistic-scientific) high school offering preparation for post-secondary studies or a vocational high school that generally terminates in a 12th Grade level of education. In our sample, 75% of students are in primary schools that terminate in Grade 8, and so they are forced to choose a different high school for Grade 9; the remaining 25% have the option to continue with Grade 9 in the same school, or switch to a different high school

At the post-secondary level, Technical Training Centres (*Centros de Formación Técnica* or CFTs, two years of study) and Professional Institutes (*Institutos Profesionales* or IPs, four years of study) offer vocational certifications and standard college degrees are offered by traditional Universities (five years of study).[14] Tuition costs are high at all of these institutions: in 2005, tuition was 47.3%, 41.7%, 19.8% and 24.8% of per capita income at private university, public university, CFT and IP respectively (OECD, 2009). This is far higher than the comparable figures for higher education tuition in the US (12%), Japan (12%) and Korea (16%). Approximately 14% of all Chilean students enrolled in post-secondary education have some type of scholarship and 26% pay tuition using loans (OECD 2009).

Financial aid programs for post-secondary education have recently expanded in Chile. Publicly-provided scholarships increased from USD 40 million in 2000 to USD 173 million in 2007 (OECD, 2009) and in 2006, the government loan program was expanded beyond traditional colleges to cover post-secondary technical studies. These new programs are in addition to existing privately funded scholarships. Each government-sponsored scholarship and loan

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suggest, opportunities for students to move across schools may be more limited (Gallego and Hernando 2009). Students may also opt out of the publicly-funded system to attend private unsubsidized secondary schools that typically charge high fees.

[14] In 2007, 660,000 young adults were enrolled in tertiary education: 68% in college, 20% in IP and 12% in CFT.

program has a different set of requirements -- all require good high school grades (in Grades 9-12) and many emphasize good performance on the PSU exam (*Prueba de Selección Universitaria*, similar to the SATs), but with different grade and PSU cut-offs.[15]

Unfortunately, students from poor backgrounds and particularly those attending free municipal schools are consistently less likely to pass the PSU or earn the types of high school grades required to qualify for this financial aid (OECD 2009). Moreover, it is difficult for these students to find the relevant information required to help them prepare for higher education. Over half of the mothers of students in our sample have not completed high school and school guidance counselors interviewed before our survey seemed not to know about many of these financial aid opportunities.[16] This potential information gap about financial aid for post-secondary schooling is something that our intervention seeks to address.

2. The intervention, experimental design and data

i) The intervention: "Abre la Caja"

The intervention provides students with information about how effort and good grades in school open up opportunities later on for further study by increasing the likelihood of being eligible for scholarships and government loans.[17] Since high school performance is critical for financial aid

[15] We estimate the effect of providing students with information about new sources of financial aid and the associated eligibility rules. If this information is likely to eventually filter out to all students then our intervention achieves a speeding-up of this dissemination process.

[16] In contrast, our student sample appeared to be well-informed about wage returns to different levels of education. See Appendix A for more details from our baseline survey.

[17] Unfortunately, we have not been able to obtain data on the fraction of students from poor schools who apply for financial aid for higher education.

eligibility and college admission in Chile, and because over 60% of Chilean students must choose a high school and a specific type of study at the end of 8^{th} Grade, we designed our intervention to target children in grade 8, four years before the relevant time for college and technical school applications.

We developed and produced a 15 minute DVD entitled *"Open the 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) who grew up in poor families in urban Chile.[18] In the program, each person talks about how, by working hard at school and becoming eligible for financial aid, they were able to finance post-secondary education at traditional colleges or at vocational schools. Their studies enabled them to become (among other things) civil engineers, graphic designers, chefs, social workers, lawyers and TV commentators. These life stories informed students about existing academic scholarships and loans for further study and provided specific details on relevant grade cutoffs and PSU cutoffs for scholarship and loan eligibility. *"Abre la Caja"* standardized these messages, thereby improving the fidelity of the treatment implementation. Another nice feature of providing the information in the form of a DVD is that it is easy and cheap to scale up for application in more schools, in contrast to having role model-type speakers give students this information.

ii) Experimental design

The study takes place in 226 schools in the lowest two income quintiles (defined by government administrative records) in Metropolitan Chile. After recruitment into the study (described in detail in Appendix A) we collected class registers and baseline Grade 7 scores from each school. We stratified the sample on school-averaged Grade 8 SIMCE scores from 2007. SIMCE scores

[18] A copy of the program is available at http://works.bepress.com/claudia_martinez_a

come from a national annual exam used to construct indicators of school quality. Within each SIMCE strata, we randomly assigned schools to groups, treatment was randomized at the school level to avoid information spillovers at the grade-level and only one class per school was randomly selected to participate in the study. 56 schools were randomly assigned to the *Student* information treatment (group A), 56 schools to the *Family* information treatment (group B), and the remaining 114 schools to the control group (group C).

An important part of what we wanted to test in this project was whether providing financial aid information to parents and children (*Family* treatment) magnified the impacts relative to providing information to children alone (*Student* treatment). For this reason, we designed exposure to the DVD to occur in two different ways. In the first way, we showed the DVD to all students in the *Student* treatment at school. The second way was constrained by budget and logistical concerns. We were unable to treat parents separately from children in the *Family* treatment, or to visit each household to ensure that all parents watched the DVD.[19] Rather than show the DVD in school to students in the *Family* treatment, we provide them with their own copy of the DVD to watch at home with their parents.[20] We used the survey instrument to test parents on whether any DVD information is retained. Much school information is distributed to parents in this same way (e.g. report cards are sent home with students). However, this design does introduce differential compliance with treatment across the two groups, complicating the interpretation of the effect of actually watching the DVD. We discuss this further in section 4.

iii) Data

[19] Only a small fraction of parents attend parents' meetings.

[20] We chose not to show the DVD in class to students in the *Family* treatment group, as this would make it impossible to separately identify the impact of family being exposed to the DVD and the impact of the student watching the DVD more than once (an intensity effect).

We conducted our baseline survey and intervention in late July and early August 2009. The baseline student questionnaire was administered in class (self-responded) and each child in every group took home a parent questionnaire and was asked to return it to school a week later, at which time our enumerators collected these surveys. Since we were concerned with potential selection in which parents chose to return the questionnaire, we randomly assigned the number of time the enumerators contacted and visited each school to pick up parent questionnaires (one, two or three times).[21]

After answering the baseline questionnaire, students in the treatment groups received the information provided in the DVD: The *Student* group was shown *"Abre la Caja"* in class and the *Family* group was given their own copy of the DVD to take home and watch with their parents. No teachers were present in the classroom during our visit.

At follow-up in November and December 2009, we revisited schools and administered a self-responded student questionnaire with many of the same questions as in the baseline survey. We asked students to take home another parent questionnaire and return it the following week. At this time, we also collected school absenteeism records for Grade 8 students for the whole year, up to the date of the follow-up visit. Schools were not expecting us to collect this information and so it is unlikely that they could have retrospectively tampered with these administrative records. Only one school refused to participate in the follow-up, leaving us with follow-up data for 225 schools.

After our follow-up, the Ministry of Education used national identification numbers to match our survey data with administrative data on student outcomes at the end of Grade 8 (Grade 8 scores and scores on the SIMCE national exam), the outcome of Grade 8 (pass/fail/withdraw)

[21] This follows a solution to selected survey non-response suggested in Dinardo, McCrary and Sanbonmatsu (2006).

and the type of school in which the student enrolled in 2009.[22] These administrative data allow us to follow key outcomes for our sample after the end of our survey. Having this unique and rich survey dataset along with the administrative data allows us to construct a comprehensive picture of how information sets, expectations, school behaviors and outcomes were affected by exposure to *"Abre la Caja"*.

3. Key outcomes and expected effects

In the standard human capital framework (Becker 1967, Card 1999), students choose an optimal level of schooling by balancing expected returns in the labor market with the costs of obtaining more education. Standard explanations for why students do not advance to higher levels of schooling are related to credit constraints, low actual or perceived Mincer returns to more education (Jensen 2010, Nguyen 2008) or, in recent work, to uncertainty about these Mincer returns. We are interested in another possible barrier to higher education: incomplete information about how to finance higher education.

We interpret the information presented in *"Abre la Caja"* as sharpening what students know about the production function of higher education. Students who watch the DVD receive a new, standardized signal about how effort in earlier years translates into more post-secondary education: for example, they learn that scoring an average grade of 5.5 in high school is one component of being eligible for college scholarship eligibility, as is writing the PSU. If they pay attention to the DVD and retain relevant information, students should be more informed about financial aid requirements and details of the loan and scholarship programs. They should also become aware of the fact that different choices about schooling in the present could have specific

[22] We thank the Ministry of Education for the access to this data.

pay-offs in future. We test whether students retain any information using five questions about DVD content in our follow-up survey. To the extent that students believe the information is relevant for them, we should also see differences in how they plan to finance their further education, with larger impacts for students more likely to be eligible for scholarships or loans based on Grade 7 grades.

Learning about the link between effort in school now and eligibility for financial aid later should also impact how much effort students provide in school, although in ambiguous ways. If students are initially over-optimistic about their chances of being able to get a higher degree, then providing them with information on how to cover tuition may discourage them from working in school. However, for students that were initially unaware of the existence of financial aid, watching the DVD should increase the marginal product of effort in school. We should see school inputs increase and, potentially, school outcomes like test scores improve and school attendance.[23] If the information increases their perceived likelihood of continuing with post-secondary education at all, they may also choose to attend high schools more consistent with this option (i.e. college-preparatory high schools) after watching the DVD.

Finally, if students are sufficiently myopic about future returns, parent learning about financial aid for higher education could have a larger impact on behavior, as long as this

[23] Our survey collected two main measures of effort in school: school absenteeism (reported in school administrative records) and student-reported amount of time spent doing homework. We also have two objective measures of school performance: Grade 8 scores reported by schools to the Ministry of Education, and test scores from the national SIMCE exam. We do not present results for the potentially subjective response of time spent on homework (there were no measured impacts on this outcome) nor for the SIMCE exam results (as this outcome, similar to our Grade 8 score measure, does not reflect any impact of assignment to treatment).

behavior can be appropriately incentivized. This larger impact of parental learning is something we investigate by comparing outcomes across *Student* and *Family* group.

4. <u>Empirical framework</u>

i) Intent to Treat (ITT) effects of exposure to "Abre la Caja" and heterogeneity with respect to baseline test scores

We first examine whether providing information affected information sets, expectations and behavior and whether these effects vary for students of different baseline test scores. We use the 7th Grade test score as one proxy for observed ability. The average effect of being exposed to *"Abre la Caja"* on each outcome Y_{ij} for individual i in school j is given by β in:

(1) $$Y_{ij} = \alpha + \beta * T_{ij} + \varepsilon_{ij}$$

where T_{ij} is a binary indicator of *Any Exposure* to the DVD and ε_{ij} is an idiosyncratic error term. Fixed effects for five strata of 2007 school SIMCE score are included in each regression specification and standard errors are clustered at the school-level.

To explore heterogeneous treatment effects by baseline test scores, students are classified as "high ability" if their 7th Grade score is between 60 and 70, "medium ability" if the score is between 50 and 60, and "low ability" if their 7th Grade score is less than 50. These cutoffs are the norm in Chile, and represent very good, good and sufficient performance. We estimate the following equation:

(2)

$$Y_{ij} = \alpha + \beta_1 * high_{ij} * T_{ij} + \beta_2 * medium_{ij} * T_{ij} + \beta_3 * low_{ij} * T_{ij} + \theta_1 * high_{ij} + \theta_2 * medium_{ij} + \mu_{ij}$$

where β_1, β_2 and β_3 represent the average treatment effects of being exposed to the DVD for each of the high, medium and low observed ability students. High scoring students are in the range where acceptance to college and eligibility for financial aid is feasible; medium scoring students are those where acceptance to college and financial aid eligibility is less certain, but where entry to vocational training should be feasible. Students with scores in the lowest bracket would not qualify for financial aid, and they would not qualify for most post-secondary institutions based on current performance. Examining the pattern of the interaction coefficients estimated in (2) will indicate which students are marginal for this information intervention; it also allows us to see whether there are discouragement effects of the DVD for lower scoring students.

ii) Comparing Student and Family Treatments

The second part of our analysis compares the effects of the *Student* and *Family* treatments. For each outcome Y_{ij} measured at follow-up, we estimate the *ITT* of each treatment separately in (3):

(3) $$Y_{ij} = \delta + \lambda_A * A_{ij} + \lambda_B * B_{ij} + \nu_{ij}$$

where A_{ij} is a binary indicator for whether student i in school j receives the *Student* treatment. Similarly, B_{ij} indicates the *Family* treatment. ν_{ij} is a person-specific error term and the regression includes stratum fixed effects and standard errors are clustered at the school level. λ_A and λ_B capture the *ITT* of being shown the DVD at school (λ_A) or of being given the DVD to take home (λ_B). We test for whether λ_A and λ_B are significantly different from zero, jointly and separately and whether they are different from each other.

Comparing the *ITT* impacts answers the question "What would happen to outcomes if we implemented this information intervention in this way, in other schools?" We would also like to unpack whether actually watching the DVD (rather than just being assigned to a treatment group) affects outcomes. Differential compliance across *Student* and *Family* treatments makes it challenging to draw conclusions about the relative effect size of watching the DVD at home or at school and so we perform two different regression-based adjustments as alternative ways of making this comparison.[24]

To describe the nature of differential compliance in more detail: in the *Student* treatment, all students were exposed to the program in class, so our estimate of λ_A is an unbiased estimate of the average effect of watching the DVD for all students as well as the treatment effect on the treated (*TOT*). In contrast, 60% of *Family* treatment students reported watching the DVD at home, implying that λ_B is not an estimate of the *TOT* but a weighted average of the effect on students who watch the DVD in this group and those who do not watch the DVD. Moreover, students who choose to watch the DVD at home are different on observables from those who choose not to watch the program. The "compliers" have higher baseline test scores (among other differences), and as we will show in the analysis of (2), students with higher baseline test scores exhibit differential treatment effects.

To estimate the impact of actually watching the DVD on compliers in the *Family* group, we specify the treatment as "watching the DVD at home" and instrument this with assignment to *Family* treatment. Without additional covariates, this scales up the ITT^B by 1/0.6. However, because of likely heterogeneous treatment effects (especially with respect to baseline test scores), this estimate of the local average treatment effect of watching at home is not directly comparable to our estimate of λ_A. Intuitively, the IV reweights λ_B to represent the effect among compliers in

[24] We will show it is plausible to assume that parents in the *Student* treatment were not exposed to the DVD, and therefore the relevant heterogeneity to consider is between students in the *Student* and *Family* treatment. Furthermore, most students that watched the DVD watched it with their parents, most parents that watched it reported watching with the family, and these reports coincide in most cases.

the *Family* treatment (Angrist and Krueger 1999). We would like some way of similarly reweighting the treatment effects for students in the *Student* treatment.

To isolate students in the *Student* treatment who are most like compliers in the *Family* treatment we use non-experimental methods and reweight λ_A using inverse probability weights. The weights are constructed to weight up the complier-like observations in the *Student* group, those students with baseline observable characteristics most like those of the compliers in the *Family* group (Horvitz and Thompson 1952). Under the – admittedly strong – assumption that treatment effect heterogeneity is only a function of observable characteristics, we can compare this inverse probability weighted estimate of the effect of watching the DVD at school (a "synthetic-*TOT*") to the IV estimate of watching the DVD at home in the following regression:

(4) $$Y_{ij} = \tilde{\alpha}_0 + \alpha_{IPW} A_{ij} + \alpha_{IV} DVD\ at\ Home_{ij} + \epsilon_{ij}$$

Our method for constructing the weights is simple and transparent: we estimate a probit model of whether a student in the *Family* treatment reported watching the DVD at home, controlling for a rich set of baseline variables that affect who watches the DVD and that likely affect outcomes (e.g. demographics, school grades, type of school attended). We must assume that the probit model is correctly specified and that we have all the observables that are important for treatment effect heterogeneity. These estimated coefficients are then applied to students in the *Student* treatment group to create predicted 'synthetic' probabilities of watching the DVD ($\hat{p}(x_i)$) under an alternative regime where choice is possible. Randomization to groups ensures that the distribution of covariates has common support across groups. Using these predicted probabilities, we construct inverse probability weights $wt_i = \frac{\hat{p}(x_i)}{(1-\widehat{p(x_i)})}$ for students in the *Student* group, and assign wt_i=*1* for control group students (since these students have zero probability of watching the DVD by experimental design) and wt_i=*1* for students in the *Family* group. Students with a high predicted 'synthetic' probability of selecting into DVD watching, based on observables, are weighted up in this regression.[25] This strategy isolates the treatment effect of watching the DVD in the *Student* treatment group for the set of students who are observably likely to choose to watch the DVD.

With the results of equation (4) in hand, we can make clearer statements about the relative effects of watching the DVD at school or at home. If $\alpha_{IPW} < \alpha_{IV}$ 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. If, on the other hand, $\alpha_{IPW} \geq \alpha_{IV}$, then watching at home has no larger impact on outcomes than watching at school for this type of student.

5. Data description

i. Sample characteristics and baseline balance

Tables 1 and 2 provide summary statistics for our data, separately for students assigned to the control group, *Any Exposure,* the *Student* and *Family* treatments and the full sample. Our baseline survey captures responses from 6,233 students. At follow-up, we have survey responses from 5,009 students (80% of baseline) who were present and gave consent at the baseline visit. Importantly, attrition from the baseline sample is 20% and balanced across each treatment and control group. Parent response rates are higher at baseline (75%) than at follow-up (58%), but

[25] 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 2001). 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. We can therefore think of students in the *Student* treatment as one possible control group for students in the *Family* treatment.

are also balanced across treatment and control schools. The lack of differential attrition across treatment and control groups gives us more confidence in the internal validity of our results.[26]

The second panel of Table 1 shows the fraction of our baseline student sample with matched administrative data: almost all students present at baseline have 8th Grade scores and 9th Grade enrollment data.[27] These administrative data from the Ministry of Education are useful in that they allow us to follow all matched students who were part of the survey at baseline; even those who were not present on the day of our follow-up visit to the school. Match rates with 7th Grade scores and absenteeism data (both collected from schools) is lower at 88% and 58% respectively. These lower match rates are explained by some schools not having records to share with us (two schools did not have Grade 7 data at all), some schools not having daily absenteeism records, some schools not having legible records, as well as inaccuracies in recording of identification numbers. The important fact to note is that match rates are not significantly different across any treatment and control schools.

Table 2 shows means and standard deviations of student-level and school-level variables measured at baseline for each of the treatment and control groups. We provide separate statistics for a combined treatment (*Any Exposure*), for each treatment group separately (*Student* or *Family* treatment), for the control group and for the full sample. For most variables, means are computed over the sample of students present at baseline. For education expectations and financial aid

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[26] Since school absenteeism is one outcome of interest and absenteeism is high at baseline and follow-up, we discuss attrition in detail in Appendix C, showing that equal attrition across groups on the follow-up visit day does not conflict with differential absenteeism across groups in the month before the survey.

[27] The less-than-perfect match rate is explained by errors in school reports that are corrected but not yet incorporated by the Ministry of Education in official statistics.

information questions, we further restrict the sample to students present at follow-up (i.e. the analysis sample for these outcomes).

Looking at control group means, students are 14 years old and only 54% of their mothers have completed high school. This latter figure highlights the potential for information asymmetries to affect educational investment choices, since many students do not have parents with experience in graduating high school, let alone continuing to post-secondary education. 29% of the control group are in the low grade group (below 50), 58% are in the medium grade group (between 50 and 60) and 13% earn high grades (60 to 70) at baseline. We use these grades as a baseline measure of student ability, as they provide at least a within-school ranking of students.

Children in our sample come from families in the two lowest income quintiles in Chilean society. However, these households are urban areas in a middle-income country, so it is not surprising that 89% of the sample has a working DVD player at home. Most students in the *Family* treatment could have watched the DVD at home if they had wanted to.

The second panel of Table 2 reports the main student-level outcomes at baseline and we again focus on discussing control group means.[28] A large fraction of students (77%) report wanting to study beyond high school. This is much higher than the 16% of young adults aged 18-24, in the lowest two income quintiles, who are actually enrolled in any post-secondary education in 2009 (CASEN, 2009). The students who report that they want to continue with post-

[28] One point to note about the survey data outcomes is that there are sometimes fewer student responses than students appearing at follow-up (N=5,009). With the self-reported design of the survey instrument, students sometimes left items blank. We check for whether item non-response is balanced across groups at baseline and follow-up, in Appendix B Table 1 and find that it is for almost all variables. We cannot reject a joint test of the null that all differences in item non-response are zero.

secondary studies are split evenly between wanting to continue studies at college (32%) and at vocational/technical schools (32%). At baseline, most students plan to finance their post-secondary studies with scholarships and family finance and a very low fraction (10%) of students report that they will use loans (multiple mentions were possible for this question). 41% report they have no idea how to finance post-secondary education. Our intervention is designed to address this information gap.

Average absenteeism reported at baseline (in June) underscores the importance of these outcomes as a measure of effort in school. Over one third of students report being absent from school at least once in the month before our baseline visit and the average number of days absent for students with absenteeism data (including zero absences) was 2.47 days in June. [29]

Table 2 also shows the results of baseline balancing tests for outcome and control variables at baseline by treatment group assignment. We perform a number of different balancing tests: first comparing differences in variables across the combined treatment (*Any Exposure*) to

[29] We were initially concerned with the quality of school administrative records on attendance, especially since minimum school attendance rates are a requirement for schools to receive the public voucher subsidy. We check the quality of the administrative data by comparing it to our own records of whether or not a student was present on the day of our baseline and follow-up visits. That is, for the sample of students for whom we have matched attendance data, we observe whether they are present or absent in class at the time of our baseline visit and we also observe, from school records, whether these students are marked present or absent by their teachers. The average fraction of misreporting by schools is 7%, meaning that 7% of students reported present in school registers are not actually in class when we visit the school. This could be because students arrive at school after the survey implementation occurs, or could reflect intentional misreporting by schools. Although we cannot distinguish between these two possibilities, it is most relevant for our purposes that attendance misreporting in school registers is balanced across groups (Appendix B Table 2).

the control group, where significant differences are indicated by stars and then comparing each individual treatment to the control group (^ denotes differences between *Student* treatment and control, + denotes differences between *Family* treatment and control), and finally comparing differences in variables across each treatment group. All variables are balanced across the *Student* and *Family* treatment groups. As one might expect from multiple testing of different outcomes in the same sample, it is possible to reject that the difference in means at baseline is zero in some cases (DVD ownership, low Grade 7 score and whether the student wants to study in a vocational school). As is becoming standard practice in recent randomization studies, we compute the Bonferroni test for joint significance across each of these balancing regressions, and cannot reject the null that all coefficients are equal to zero, or that the differences between *Student* and *Family* group means are jointly equal to zero. This gives us more confidence that treatment and control groups are the same across a range of observable characteristics at baseline.

6. Results

i. Effects of exposure to "Abre la Caja" on information, expectations and school outcomes

In order for the DVD to affect behavior through an information channel, students first need to have learned and retained relevant information from *"Abre la Caja"*. We show this in Table 3A. The table shows estimates of equations (1) and (2) for survey questions about the content of the DVD asked three months after the baseline survey. In columns (1) and (2), the dependent variable is the number of correct responses to five financial aid questions asked in the follow-up survey.[30] In remaining columns, we show the OLS estimates for a correct response to each specific question. Treated students score 0.068 points more on the test of information provided in the DVD, which is a 5.5% improvement relative to the control group mean. The significant gains on this little test largely come from two questions about the number of students who currently receive financial aid from the government and the minimum grade required to qualify for vocational training loans.

Although we do not have power to detect small differences in effects by student ability, the pattern of coefficients on the interaction terms in Table 3A provide suggestive evidence of heterogeneous. In three of the five questions, a larger fraction of medium and high scoring students get the answers right.

Table 3B goes on to show that exposure to the DVD changes student plans for financing higher education, with some of the same indications of heterogeneity by baseline test scores. Exposure to *"Abre la Caja"* increases the fraction of students reporting that they will finance education with a government loan by 4.6 percentage points (column (3)), and decreases the fraction that do not know how to finance education by 4.2 percentage points (column (7)). This is a large effect: knowledge of loan opportunities increases by almost 50%, and "ignorance" falls by 10%. Interestingly, there is no effect of exposure on reports of using family finance, which was not something touched on in the DVD.

Students with higher baseline test scores seem to drive these average effects: more of the high scoring children report wanting to use scholarships (9.4 percentage points) and loans for

[30] The questions are: 1) How many students do you think receive state grants or loans to continue studying? 2) what is the minimum PSU score you need for college scholarships? 3) what is the minimum grade you need to apply to a vocational training loan? 4) is the PSU free for municipal or publicly subsidized school students? 5) how do government scholarships work? All questions had multiple choice answers.

finance (7.7 percentage points), and fewer of them report not knowing how to finance post-secondary school (-7.6 percentage points). These patterns suggest that the marginal students for this information intervention were students with higher initial baseline test scores.

Although there are large impacts on information sets and financial aid plans in Table 3A and 3B, we do not find large average effects of being assigned to an information treatment on whether students plan to study after school at all, nor on the type of study (Table 4, odd-numbered columns). Given that three quarters of students report wanting to continue with further studies at baseline, it is not surprising that "*Abre le Caja*" did not raise (or lower) overall educational expectations of any post-secondary education by a significant amount. The results do, however, suggest that within ability group, the new information may have shifted the types of schooling desired. Columns (4) and (6) show that students with higher grades are 7 percentage points more likely to report they will study at college compared to students with low grades, and students with low grades are 8 percentage points more likely to report they will study at a vocational training school. These are sensible responses, given that grade and PSU cut-offs for financial aid eligibility are lower for attending a vocational school than for attending college.

Even though overall educational expectations do not increase, new information about financial aid opportunities could still affect student behavior. Part of what the DVD emphasized was the importance of current schooling investment decisions -- putting in effort now -- for opening up opportunities four years later. In Table 5 we present the first results that some behaviors do change.

In the first four columns, we show whether the treatment affects whether a student enrolls in a humanistic-scientific high school i.e. a high school that prepares students for continuing education. The 9^{th} Grade enrollment outcomes are measured first in the sample enrolled in primary schools that have continuing secondary school grades (columns (1) and (2)) and then for the sample of students enrolled in primary schools that terminate in Grade 8 (columns (3) and (4)). This second group is about 75% of our student sample.

For students who are not required to make a decision about enrolling in a new school for Grade 9, there is no apparent difference in the fraction enrolled in a college-oriented high school. However, for the set of students constrained to choose a different school for Grade 9 (columns 3 and 4), about 11% more students enroll in college-preparatory high school (6.3 percentage points more enrollment, p-value of 0.058), relative to the control group. This is a 0.13 s.d. increase in enrollment in college-preparatory high schools, relative to the control group. The results indicate that the enrolment response is largely driven by students with medium grades. For these students, the effect is about an 18% increase in enrollment in a college oriented high school, relative to the control group. These are the students we might consider most marginal for the purposes of financial aid, i.e. those students for whom financial aid eligibility is uncertain and who might benefit the most from attending a better school.[31]

Effects for short run absenteeism outcomes are presented in the next four columns of Table 5. Columns (5) and (6) contain results for whether the student was absent from school in September at all while columns (7) and (8) contain estimates for the number of days the student missed school in September, where no absences are coded as zeros. Measuring absenteeism in September 2009, a few weeks before we visit the school for the follow-up survey, reassures us

[31] For benchmarking, Hastings and Weinstein (2008) find that providing information about school test scores to low income parents in the US increases choice of an alternative school by 5 to 7 percentage points.

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.[32]

For students assigned to *Any Exposure*, absenteeism prevalence falls by a significant 8.8 percentage points on average (p-value 0.016). This is a 14% reduction, relative to the control group mean at follow-up. Looking at the pattern of coefficients on the interactions with baseline grades in column (2), this reduction in prevalence is driven by students with medium and high scores. And, although not quite significant at conventional levels, total absenteeism in September appears to fall on average by 0.25 days or 0.8 of a standard deviation (or 10% of June absenteeism).[33] If this effect was sustained over the course of a 10 month school year, it would translate into an increase in attendance of 2.5 days. This reduction on the intensive margin also appears to be driven by students with medium and high baseline grades although again, we do not have power to precisely estimate these heterogeneous effects.

While the absenteeism effects appear large, they compare well with other estimates in the development literature that look at the impact of health interventions or of information interventions on absenteeism among school children. For example, in a randomized controlled trial of providing primary school students with deworming medication, Miguel and Kremer (2004) show that school attendance increases by about 30 percent. Bobonis, Miguel and Puri-

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[32] As described in the experimental design section, the intervention was conducted in July-August, and the follow-up survey was in October-November. We collected attendance data at the follow-up, and therefore only have attendance data until mid-November at most, depending on the school visit day. Given a teacher strike in October and November, the only post-intervention month with complete attendance data is September 2009.

[33] Results from a poisson regression for the total number of days absent in September variable are qualitatively the same; estimated coefficients on the treatment exposure variables are somewhat smaller, but statistically significant in the same way as in the OlS results.

Sharma (2006) find that a combination of deworming medication and iron supplements reduced absenteeism by about one-fifth (increasing attendance by 5.8 percentage points). More closely related to our work, Nguyen (2008) finds that providing statistical information on returns to education to parents of Grade 3 students in rural Madagascar reduces absenteeism by a much larger magnitude: between one-fifth and one-quarter, or 3.5 percentage points on an overall 15% absenteeism rate.

The last two columns (9) and (10) of Table 5 show us that this increased attendance does not translate into higher scores at the end of Grade 8. There is no effect of treatment assignment on Grade 8 scores on average, or for students with different baseline grades. The point estimates themselves are small, relative to the control group mean grade of 53. Since the intervention increased attendance by two and a half days at most, and since Grade 8 scores are measured only 5-6 months after the intervention, it is not surprising that there is no large effect on learning measured as the year's average school grade.[34]

ii. Differences between watching "Abre le Caja" at school vs home: Accounting for heterogeneity

Our results so far have shown that assignment to any information group raises awareness of financial aid support, may shift expectations about where future study will occur, raises 9th Grade enrollment in college preparatory high schools among constrained students and reduces absenteeism, particularly for students with medium and higher grades at baseline. In this section, we explore whether these *ITT* estimates for *Any Exposure* are the same whether we consider the

[34] We also found no large or significant differences in the end of Grade 8 SIMCE scores for students in treatment relative to control groups, nor for enrollment in any high school. Results available on request.

Student or *Family* treatment groups, or whether parental exposure magnifies the impact of students watching the DVD.

We begin by showing that being assigned to the *Family* treatment does impact parent information sets. In Table 6 column (1), we show OLS estimates for parent DVD knowledge scores on the short test of information about financial aid presented in *"Abre la Caja"*. These test scores are measured at baseline for parents; however, because we sent the DVD home with children at baseline, this first survey for parents captures post-treatment outcomes.

In Table 1, we saw that 75% of parents responded to the baseline survey and this response rate was not different across treatment and control groups. 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 compute Heckman selection-corrected regression results for the same parent test score outcomes at baseline in column (2) of Table 6. The exclusion restrictions here are two indicators for whether the school was visited twice or three times for survey retrieval; the number of visits was randomly allocated to schools. Results from the first stage selection equation are presented in column (3) where we see the number of visits significantly predicts higher response rates.

Regardless of whether or not we adjust estimates for parental non-response, it is clear that parents in the *Family* treatment group score significantly higher on tests of the DVD knowledge – about 30% higher than parents in the control group and the *Student* treatment. Interestingly, parents score lower on these five questions than students do: the mean student score within the control group is 1.23 (Table 3) while for parents, this mean score is only 0.98 (Table 6).

Nonetheless, the evidence in Table 6 indicates that parents in the *Family* treatment did watch the DVD while parents in the *Student* treatment were not exposed to this new information. Both of these points are necessary if we are to infer that equal treatment effects imply a lack of parental effectiveness, rather than a lack of parental exposure (e.g. if *Family* treatment parents did not watch the DVD) or uniform exposure of parents in both treatment groups (e.g. if *Student* treatment parents somehow received the information from their children even without receiving the DVD).

Next, we compare effects on the main behavioral outcomes across the *Student*, *Family* and Control groups in Table7. In each regression, no other controls are included. Results for information and educational expectation outcomes are in Appendix B Table 4.

As described in Section 4, we do this comparison in several ways. For each behavioral outcome there are four sets of regression coefficients. The first are the OLS results from regressions of each outcome on a dummy variable for each treatment group, as in equation (3). The second set are from the regression where we estimate the *ITT* for the *Student* treatment and the local average treatment effect for watching the DVD at home, instrumenting with assignment to the *Family* treatment. To isolate a comparable set of complier-type students in the *Student* treatment group, we implement the reweighted estimator and inflate the *Student* treatment effects to identify what we call the synthetic-*TOT* (third set of regression results).[35] Finally, we combine

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[35] Weights are created using the probit regression coefficients given in Appendix B Table 3. 60% of students and 53% of parents report watching the DVD at all. The mean (median) number of times these children watched the DVD was 1.195 (1) times. Of those 711 students who reported watching the DVD at least once, 65% of them reported watching at least once with their parents. Younger students, students with higher 7^{th} Grade scores and students in private voucher schools are significantly more likely to report watching the DVD, as intuition might suggest. It is striking that low grade students are 42.1 percentage points less likely to watch the DVD. Estimated

the IV and reweighting (RW) approach as in equation (4) to compare the effect sizes for compliers (identified by instrumenting within the *Family* group) and the complier-types (identified by reweighting the *Student* group observations). It is worth noting that we are finally comparing individuals who watched the DVD in the *Family* treatment group with similar individuals in the *Student* treatment group. These students are more likely to have medium or higher grades at baseline, be in private voucher primary schools and be slightly older than the average student (see Appendix B Table 3). Since DVD watchers are generally not the students with lowest grades, we are unable to say what this *Student-Family* comparison would look like for lower grade students.

Given the design of the experiment and taking into account the baseline distribution of Y_{ij}, we have sufficient power to detect whether assignment to *Family* treatment has a larger than 0.15 standard deviation impact on absenteeism outcomes, relative to *Student* treatment effects (i.e. large differences in treatment effects are discernable, smaller differences are not).[36] For other outcomes, we have power to detect whether the difference between λ_A and λ_B is larger than 0.11-0.15 standard deviations. We present each of these minimum detectable differences in terms of standard deviations of the outcome variable measured at baseline (or at follow-up in the administrative data) in the control group, in the last row of each panel in the table.

Comparing OLS coefficients in columns (1) and (5) with the reweighted coefficients in columns (2) and (6), it is clear that reweighting increases the estimated coefficient on the *Student*

values of the predicted probability lie between 0.2 and 0.8, implying that we do not run into the problem of constructing weights with predicted probabilities close to 0 or 1 (Dinardo, 2002).

[36] For our initial power calculations used in the research design, we were only able to use data on the distribution of SIMCE test scores to define appropriate sample sizes for each treatment group. We use baseline survey data and administrative data to define the *ex poste* minimum detectable effect sizes in Table 7.

treatment in all cases. Likewise, the estimated coefficients for the *Family* treatment increase after instrumenting for watching the DVD at home (columns (3) and (7)). The effects of the treatment for compliers are larger for every outcome variable. For school absenteeism on the extensive and intensive margins: assignment to *Student* treatment decreases the probability of absenteeism by between 6.6 and 8.8 percentage points (not significant), whereas the *Family* treatment decreases it by 9.4-17.1 percentage points depending on the specification. When the dependent variable is the number of days absent, the *Student* treatment is only significant when students similar to the DVD watchers in the *Family* group are weighted up with the inverse probability weight (Panel A: column (6)). Students in the *Family* treatment decreased the number of days absent between 0.37 and 0.67 days per month. The joint test of significance for *Student* and *Family* treatments can reject zero for both absenteeism outcomes (Panel A), but not for Grade 8 grades; it is marginally significant with a p-value of 0.1 for 9th Grade enrollment outcomes. This reflects what we found in Table 5 using the combined treatment indicator.[37]

Importantly we cannot reject the null that the two ways of presenting *"Abre la Caja"* had equivalent effects on behavioral outcomes, regardless of how we compare the treatments. We interpret these results, in combination with the results in Table 6, in the following way: even though *Family* treatment parents do learn more about financial aid opportunities and rules than *Student* treatment parents, the impact of watching the DVD on behavior is not significantly

[37] Appendix B Table 4 shows the impact of the *Family* and *Student* treatments on the financial aid knowledge and expectations. There is no differential impact between these two groups on the fraction of students reporting loan finance or the fraction wanting to study in college or in vocational school. We do find that, after combining the IV and RW procedure, the *Family* treatment has a larger impact on the fraction of students wanting to use scholarship finance and wanting to study after high school at all, and a larger impact on test scores related to DVD knowledge (significant at the 10% level).

different across these two treatment groups for the set of students who are likely to watch the DVD on their own.

7. Discussion and conclusions

In many middle- and upper-income countries, access to post-secondary education is highly correlated with socioeconomic status of families. This is certainly the case in Chile, despite massive improvements in general education and the recent expansion of loan and scholarship programs for higher education. Eligibility for loans and scholarships depends on a combination of high school grades, scores on a national selection test (PSU) and financial need. However, learning about these eligibility requirements is not easy, given the variety of financial aid programs available and the limited experience many parents have with the higher education system. For some students, finding out that high school grades are important for the process of accessing higher education may occur too late in the school career.

Our project experimentally manipulated exposure to standardized information about financial aid opportunities and eligibility rules for post-secondary school education during the last year of primary school for over 6,000 students in 226 vulnerable schools in Metropolitan Chile. We additionally manipulated whether students, or students along with their parents, learn about this new information.

Using our survey data matched with rich administrative data that allowed us to follow our students after the follow-up survey, we find evidence that exposure to *"Abre la Caja"* increases knowledge of financial aid sources and eligibility rules (especially regarding student loans), shifts education expectations, increases enrollment in college-preparatory high schools by about 11% for students who are terminal primary schools and reduces absenteeism prevalence by 14%

(0.08 s.d.) on average. There were no effects on test scores at the end of Grade 8. It may have been too soon after the intervention to expect results for test scores, or test score gains may be dependent on complementary inputs like textbooks and motivated teachers (as in De Fraja, Oliviera and Zanchi 2010).

We also find suggestive evidence of heterogeneous treatment effects for students with different baseline test scores, one measure of student ability: those with medium and higher grades were more likely to expect to use scholarships and loans after the intervention and report more knowledge of financial aid eligibility rules. Medium grade students were also more likely to report enrollment in college-preparatory high schools and exhibited larger reductions in absenteeism. The students with medium and higher grades were clearly the marginal students for this particular information intervention.

The second set of findings shows that even though parents in the *Family* group retained more information about financial aid eligibility rules than other parents, the effects of exposure to "*Abre la Caja*" in the classroom or at home were statistically indistinguishable from each other regardless of how this comparison is made. In urban Chile, adolescents appeared capable of making improved human capital investment decisions in the wake of new information about how to get in to post-secondary schooling.

Overall our results on the effects of imperfect information about net returns to education fall somewhere between the results for developing and developed countries. They add to a body of evidence suggesting that different types of imperfect information can lead to underinvestment in human capital (Nguyen, 2008, Jensen 2010) in developing countries and highlight the importance of considering how students of different abilities respond to the new information. We learn that improving information, even among parents, is insufficient for changing some types of school performance (e.g. test scores). This result is more in line with the developed country literature (e.g. Bettinger, Long, Oreopolous and Sanbonmatsu 2009) and suggests that policy-makers in Chile might need to consider earlier or more intense information interventions if the target outcome is improved test scores. Finally, the Grade 9 enrollment results suggest there could be other ways of promoting school choice outside of providing parents with information about school quality, as other studies have done. Overall, our results suggest caution in extrapolating from policy experiments conducted across different country settings for the purposes of cost effectiveness comparisons. Different constraints are likely to be binding in different contexts for different types of educational outcomes.

Given initially high aspirations among our sample students, we do not find it surprising that some students alter behavior in response to information about how to achieve higher education, nor that the medium and higher grade students are the ones most marginal in this intervention. What is more surprising is that in urban Chile, parents did not significantly magnify the impact of our information intervention on adolescents. Adolescents appeared to understand how current school behavior affects eligibility for funding sources at the end of high school, and make some changes to their own education behaviors and choices. It is beyond the scope of this paper to investigate why parents did not have a larger impact on outcomes. However, since so much academic work starts from the assumption that parents are the most important actors in human capital investment choices for children, and so many policies directly target parents in attempts to alter school outcomes, our results suggest that it may be important to consider the question of when adolescents become their own agents in decisions about schooling.

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Table 1: Distribution of students across experimental groups and match rates with administrative data

	Control group (C)	Any Treatment (T=Student/ Family)	Student Treatment (A)	Family Treatment (B)	Full sample
Distribution of sample					
N schools at baseline	114	112	56	56	226
N schools at follow-up	114	111	56	55	225
Retention rate of schools (1-attrition)	1.00	0.99	1.00	0.98	1.00
N students on class roster	3,902	3,794	1,908	1,886	7,696
N students present at baseline	3,179	3,054	1,536	1,518	6,233
Attendance rate at baseline	0.81	0.80	0.81	0.80	0.81
N students present at follow-up (Analysis sample)	2,560	2,449	1,254	1,195	5,009
Retention rate of baseline student sample (1-attrition)	0.81	0.80	0.82	0.79	0.80
Response rate of parents at baseline	0.75	0.75	0.76	0.74	0.75
Response rate of parents at follow-up	0.58	0.57	0.56	0.57	0.58
Match rates with administrative data					
N students with matched June absenteeism data	1,992	1,608	884	724	3,600
Frac. of baseline students with matched absenteeism data, June	0.63	0.53	0.58	0.48	0.58
N students with matched September absenteeism data	1,998	1,617	887	730	3,615
Frac. of baseline students with matched absenteeism data, Sept	0.63	0.53	0.58	0.48	0.58
N students with matched Grade 7 scores data	2,822	2,670	1321	1,349	5,492
Frac. of baseline students with matched grade 7 scores	0.89	0.87	0.86	0.89	0.88
N students with matched Grade 8 scores	3,145	3,036	1,529	1,507	6,181
Frac. of baseline students with matched grade 8 scores	0.99	0.99	1.00	0.99	0.99
N students with matched Grade 9 enrollment data	2,982	2,878	1,437	1,441	5,860
Frac. of baseline students with matched Grade 9 enrollment data	0.94	0.94	0.94	0.95	0.94

Table provides summary statistics for schools, students and parents participating in the project. Students present at baseline are those who show-up at school on the day of our visit and responded to the survey. Match rate with administrative data is the fraction of students present at our survey at baseline who we can match with administrative data based on national identification number. Differences in match rates not statistically significant.

Table 2: Summary statistics and baseline balance tests

	Full Sample		N	Any Treatment (Student/ Family) (T)	Student Treatment (A)	Family Treatment (B)	Control (C)
	Mean	s.d.		Means			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Baseline student variables							
Age	13.98	0.85	6,233	14.00	14.01	13.99	13.97
Female	0.47	0.50	6,233	0.46	0.45	0.46	0.49
Mother completed high school	0.52	0.50	6,233	0.51	0.50	0.53	0.54
Missing mother education indicator	0.15	0.35	6,233	0.15	0.17^	0.14	0.14
Grade 7 score is Low	0.31	0.46	6,233	0.33*	0.34^^	0.31	0.29
Grade 7 score is Medium	0.57	0.50	6,233	0.56	0.55	0.57	0.58
Grade 7 score is High	0.12	0.33	6,233	0.11	0.104^^	0.12	0.13
Grade 7 score missing~	0.12	0.32	6,233	0.13	0.14	0.11	0.11
Has a DVD player at home	0.90	0.31	6,166	0.91**	0.90	0.91++	0.89
DVD player is working	0.86	0.34	6,063	0.88**	0.88	0.88++	0.85
Education expectations							
Will study beyond high school	0.76	0.43	5,918	0.76	0.75	0.77	0.77
At college	0.31	0.46	4,346	0.30	0.29	0.31	0.32
At a vocational school	0.34	0.47	4,346	0.35*	0.35	0.36+	0.32
Financial aid expectations							
Pay for studies w/ scholarships	0.36	0.48	4,466	0.37	0.36	0.37	0.35
Pay for studies w/ loans	0.11	0.31	4,466	0.11	0.10	0.11	0.10
Pay for studies w/ family resources	0.39	0.49	4,466	0.40	0.42^^	0.38	0.37
No idea how to pay for studies	0.40	0.49	4,466	0.39	0.38	0.40	0.41
Absenteeism							
Has school June absenteeism data~	0.58	0.49	6,233	0.53	0.58	0.48+	0.63
Absent at all in June (school)~	0.67	0.47	3,600	0.65	0.68	0.62	0.68
Days absent in June (school)~	2.47	2.99	3,600	2.48	2.72	2.19	2.46
School-level variables							
Fraction private voucher schools$	0.31	0.46	226	0.34	0.30	0.38	0.28
School poverty score (Poorest=80)$	46.42	9.08	226	46.95	46.90	47.01	45.90
School continues to grades 9-12	0.24	0.43	226	0.24	0.30	0.18	0.25
Stratum of SIMCE scores in 2007$	2.98	1.41	226	3.01	3.04	2.98	2.96
Fraction providing attendance data	0.79	0.41	226	0.98	1.00	1.00	1.00

Table shows summary statistics for variables collected in our survey or from administrative data (~ from schools, $ from 2007 SIMCE data). Sample for student-level variables and absenteeism variables includes all students present at baseline; sample for student expectations questions is further restricted to analysis sample present at follow-up. SIMCE score is the combined school-averaged math and language scores on the 2007 Grade 8 SIMCE tests. We impute values for control variables missing values using the mean value of the variable over non-missing observations, or a value of 0 for indicator variables. We create a missing indicator variable to flag these imputed observations in regressions. Missing values for outcome variables are not imputed. We implement balancing tests by regressing the variable of interest on a constant and an indicator for assignment to a particular treatment (T, or A and B separately). Robust standard errors are clustered at the school level. In comparing T and C, *** denotes difference significant at 1% level, ** and 5% level and * at 10% level. Similar notations are used to indicate statistically significant differences between A and C (^^^, ^^ and ^) and B and C (+++, ++ and +). There are no statistically significant differences in any means in the group A-group B comparison. See Appendix Tables for analysis of balance in item non-response.

Table 3A: Effects of exposure to "*Abre la Caja*" on financial aid knowledge and expectations at follow-up: OLS

	A: Score on test of financial aid knowledge learned from DVD											
	Total (scale of 0 to 5)		1. How many students now receive state financial aid?		2. Min. PSU score needed to attend college?		3. Min. grade to qualify for a vocational training loan?		4. Is PSU free for students in municipal schools?		5. Do you know how financial aid loans work?	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Any exposure to "*Abre la Caja*"	0.068**		0.016*		0.002		0.022*		0.014		0.015	
	(0.034)		(0.009)		(0.015)		(0.013)		(0.016)		(0.016)	
Any exposure*Low grade		0.071		0.013		-0.027		0.019		0.0456*		0.021
		(0.051)		(0.013)		(0.021)		(0.025)		(0.025)		(0.025)
Any exposure*Medium grade		0.066		0.014		0.015		0.014		0.007		0.016
		(0.045)		(0.012)		(0.020)		(0.016)		(0.019)		(0.021)
Any exposure*High grade		0.092		0.035		0.024		0.048		-0.016		0.001
		(0.086)		(0.025)		(0.040)		(0.033)		(0.035)		(0.040)
Medium grade		0.065		0.0226*		0.015		-0.0387*		0.0520**		0.014
		(0.045)		(0.012)		(0.019)		(0.020)		(0.021)		(0.021)
High grade		0.101		0.025		0.029		-0.0769***		0.0738**		0.051
		(0.066)		(0.017)		(0.027)		(0.028)		(0.030)		(0.031)
N	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009
Control group mean	1.23	1.23	0.09	0.09	0.30	0.30	0.23	0.23	0.21	0.21	0.42	0.42
Pval: Any*Low=Any*Med		0.94		0.94		0.14		0.88		0.16		0.88
Pval: Any*Med=Any*High		0.78		0.44		0.84		0.35		0.54		0.73
Pval: Any*High=Any*Low		0.83		0.42		0.26		0.48		0.15		0.66

Table presents OLS coefficients on an indicator for Abre la Caja exposure, and (in even-numbered columns) indicators for whether baseline grades were medium or high and low, medium and high grade interactions with treatment assignment.*** 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 and a missing Grade 7 score indicator. All outcomes except for total score are binary; total score ranges from 0 to 5.

Table 3B: Effects of exposure to "*Abre la Caja*" on financial aid knowledge and expectations at follow-up: OLS

	B: Expected source of finance for post-secondary education?							
	Scholarship finance		Loan finance		Family finance		DNK how to finance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any exposure to "*Abre la Caja*"	0.026		0.046***		0.009		-0.042**	
	(0.020)		(0.012)		(0.016)		(0.019)	
Any exposure*Low grade		0.032		0.0418**		0.040		-0.050
		(0.028)		(0.020)		(0.030)		(0.035)
Any exposure*Medium grade		0.020		0.0419***		0.001		-0.038
		(0.024)		(0.016)		(0.021)		(0.025)
Any exposure*High grade		0.0937**		0.0773**		-0.015		-0.0761*
		(0.044)		(0.034)		(0.037)		(0.040)
Medium grade		0.1189***		0.021		-0.002		-0.0597*
		(0.024)		(0.015)		(0.026)		(0.032)
High grade		0.3908***		0.0808***		-0.010		-0.2106***
		(0.038)		(0.025)		(0.035)		(0.038)
N	3,372	3,372	3,372	3,372	3,372	3,372	3,372	3,372
Control group mean	0.32	0.32	0.10	0.10	0.29	0.29	0.46	0.46
Pval: Any*Low=Any*Med		0.73		1.00		0.29		0.78
Pval: Any*Med=Any*High		0.12		0.35		0.67		0.39
Pval: Any*High=Any*Low		0.24		0.37		0.26		0.61

Table presents OLS coefficients on an indicator for Abre la Caja exposure, and (in even-numbered columns) indicators for whether baseline grades were medium or high and low, medium and high grade interactions with treatment assignment.*** 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 and a missing Grade 7 score indicator. All outcomes are binary and sources of financial support outcomes are not mutually exclusive categories.

Table 4: Effects of exposure to *"Abre la Caja"* on student education expectations at follow-up: OLS

	Do you think you will study:					
	After high school?		At college?		At a vocational school?	
	(1)	(2)	(3)	(4)	(5)	(6)
Any exposure to *"Abre la Caja"*	0.011		0.022		0.003	
	(0.016)		(0.020)		(0.020)	
Any exposure*Low grade		0.013		-0.026		0.0865**
		(0.028)		(0.032)		(0.035)
Any exposure*Medium grade		0.022		0.038		-0.026
		(0.020)		(0.026)		(0.026)
Any exposure*High grade		0.002		0.0768*		-0.049
		(0.030)		(0.045)		(0.045)
Medium grade		0.0986***		0.0576**		-0.006
		(0.024)		(0.025)		(0.029)
High grade		0.2400***		0.2057***		-0.0836**
		(0.033)		(0.042)		(0.038)
N	4,918	4,918	3,301	3,301	3,301	3,301
Mean outcome for control group	0.68	0.68	0.36	0.36	0.42	0.42
Pval: Any*Low=Any*Med		0.76		0.08		0.01
Pval: Any*Med=Any*High		0.59		0.44		0.66
Pval: Any*High=Any*Low		0.80		0.06		0.02

Table presents OLS coefficients on an indicator for Abre la Caja exposure, and (in even-numbered columns) indicators for whether baseline grades were medium or high and low, medium and high grade interactions with treatment assignment.*** 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 and a missing Grade 7 score indicator. All outcomes are binary. Sample in columns (3)-(6) is restricted to individuals who responded yes to the question: Do you think you will continue studying after high school?

Table 5: Effects of exposure to *"Abre la Caja"* on student effort and education outcomes at follow-up: OLS

	Enrollment in college-oriented high school (MINEDUC data)				Absenteeism (School administrative records)				Test scores (MINEDUC data)	
	Sample in primary schools with continuing Grades 9-12		Sample in primary schools without continuing Grades 9-12		Absent in September?		N days absent in September		Grades at the end of Grade 8	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any exposure to *"Abre la Caja"*	-0.082		0.063*		-0.088**		-0.253		-0.235	
	(0.092)		(0.033)		(0.036)		(0.162)		(0.320)	
Any exposure*Low grade		-0.081		0.0367		-0.067		-0.143		0.169
		(0.114)		(0.041)		(0.044)		(0.277)		(0.503)
Any exposure*Medium grade		-0.072		0.0816**		-0.1130***		-0.3715**		0.102
		(0.088)		(0.037)		(0.040)		(0.181)		(0.290)
Any exposure*High grade		-0.110		0.0404		-0.082		-0.4238**		0.115
		(0.121)		(0.052)		(0.056)		(0.177)		(0.406)
Medium Grade		0.040		-0.0299		-0.029		-0.5121***		5.4105***
		(0.044)		(0.027)		(0.030)		(0.190)		(0.317)
High Grade		0.061		0.0306		-0.1830***		-1.1785***		12.9882***
		(0.066)		(0.040)		(0.044)		(0.216)		(0.356)
N	1,462	1,462	4,191	4,191	3,615	3,615	3,615	3,615	6,181	6,181
Mean outcome for control group	0.75	0.75	0.60	0.60	0.64	0.64	2.10	2.10	53.69	53.69
Pval: Any*Low=Any*Med		0.91		0.43		0.26		0.41		0.90
Pval: Any*Med=Any*High		0.63		0.23		0.53		0.80		0.98
Pval: Any*High=Any*Low		0.77		0.95		0.81		0.33		0.93

Table presents OLS coefficients on an indicator for Abre la Caja exposure, and (in even-numbered columns) indicators for whether baseline grades were medium or high and low, medium and high grade interactions with treatment assignment. All regressions contain stratum fixed effects that define the quintile of the SIMCE 2007 score distribution into which each school falls and a missing Grade 7 score indicator. In the first two columns, the sample is restricted to students in primary schools that continue with Grades 9 to 12, the third and fourth columns are restricted to the sample of students enrolled in primary schools that terminate in Grade 8. Absent in September and Enrollment variables are binary; Days absent in September range from 0 to 22; Grade 8 scores range from 0 to 70. Sample size varies because of differential match rates between each administrative data outcome and our baseline student sample.

Table 6: Effects of treatment assignment on parental scores of financial aid eligibility rules test (Range = 0 to 5)

	OLS: Uncorrected for parent non-response	OLS: Heckman selection-corrected for parent non-response	First stage marginal effects for the Heckman selection
	(1)	(2)	(3)
A: *Student* treatment	0.025	0.028	0.033
	(0.034)	(0.025)	(0.039)
B: *Family* treatment	0.293***	0.290***	-0.032
	(0.034)	(0.033)	(0.037)
Two repeat visits			0.283***
			(0.039)
Three repeat visits			0.371***
			(0.041)
N	4,664	6,233	6,233
Control group mean	0.98	0.98	0.98
Pval for joint test of A, B	0.00	0.00	0.00
Pval for test of A=B	0.00	0.00	0.00
Mills ratio			0.24
S.e. Mills ratio			(0.18)

Table shows coefficients from regressions of parent scores on DVD knowledge questions (scale of 0 to 5) on assignment indicators. Sample in column (1) includes only parents who who returned surveys to schools at baseline; sample in column (2) is entire baseline sample. Column (3) presents the marginal effects from the first stage of the Heckman selection correction that we implement to deal with parental non-response. Indicators for the number of repeat visits to each school to collect parent surveys are included in the participation equation and excluded from the main equation.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. 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 and a missing Grade 7 score indicator.

Table 7: Effects of treatment assignment on effort in school and educational outcomes: OLS, IV and reweighting

Panel A	School reports of absenteeism							
	Absent in September				Num. days absent in September			
	OLS	Reweight A	IV B	RW & IV	OLS	Reweight A	IV B	RW & IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: *Student* treatment	-0.066	-0.0885**	-0.066	-0.0885**	-0.220	-0.3819**	-0.220	-0.3819**
	(0.047)	(0.043)	(0.047)	(0.043)	(0.180)	(0.159)	(0.180)	(0.158)
B: *Family* treatment	-0.0940*	-0.0940*	-0.1712*	-0.1712*	-0.3663*	-0.3663*	-0.6673*	-0.6673*
	(0.051)	(0.051)	(0.095)	(0.095)	(0.196)	(0.196)	(0.362)	(0.361)
N	2,912	2,912	2,912	2,912	2,912	2,912	2,912	2,912
Control group mean	0.64	0.64	0.64	0.64	2.10	2.10	2.10	2.10
Pval for joint test of A, B	0.13	0.06	0.14	0.06	0.15	0.04	0.15	0.03
Pval for test of A<B	0.31	0.46	0.13	0.18	0.25	0.47	0.10	0.20
Minimum detectable diff: (A-B)	0.15 s.d.	0.15 s.d.	0.15 s.d.	0.15 s.d.	0.13 s.d.	0.13 s.d.	0.13 s.d.	0.13 s.d.
Panel B	School Grades (MINEDUC data)				Enrollment (MINEDUC data)			
	Grades at the end of Grade 8				In a college-preparation high school (constrained sample)			
	OLS	Reweight A	IV B	RW & IV	OLS	Reweight A	IV B	RW & IV
A: *Student* treatment	-0.541	0.605	-0.541	0.605	0.063	0.0757*	0.063	0.0757*
	(0.357)	(0.448)	(0.357)	(0.447)	(0.044)	(0.042)	(0.044)	(0.042)
B: *Family* treatment	0.110	0.110	0.183	0.183	0.0674*	0.0674*	0.1107*	0.1107*
	(0.377)	(0.377)	(0.630)	(0.629)	(0.038)	(0.038)	(0.062)	(0.062)
N	4,969	4,969	4,969	4,969	3,440	3,440	3,440	3,440
Control group mean	53.69	53.69	53.69	53.69	0.60	0.60	0.60	0.60
Pval for joint test of A, B	0.24	0.40	0.24	0.40	0.15	0.11	0.15	0.10
Pval for test of A<B	0.07	0.17	0.12	0.27	0.46	0.43	0.22	0.28
Minimum detectable diff: (A-B)	0.11 s.d.	0.11 s.d.	0.11 s.d.	0.11 s.d.	0.15 s.d.	0.15 s.d.	0.15 s.d.	0.15 s.d.

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. No additional controls in these regressions. Reweighted regressions are inverse probability weighted, as described in the text. IV regressions instrument for student watching the DVD at home, using assignment to *Family* treatment group as the instrument. Minimum detectable effect size is minimum difference in *Student-Family* treatment effects that we can detect given our sample size, number of clusters, a power of 0.8 and the intra-cluster correlation in the specific outcome variable at baseline. Sample size varies based on whether we are able to match our survey data to administrative data.

Appendix A: Experimental Design

Sample selection and randomization

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.[1] 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. 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.[2] These criteria left us with over 400 schools, all of which were approached to be part of our experiment.[3]

We hired a call center to contact the schools in our sampling frame and ask them to join our study. A total of 226 schools were reached and agreed to participate. Some fraction of schools declined to participate while others were unable to be reached by phone. We discuss implications of this sample selection on the external validity of our results in Appendix D-External Validity.

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 an R^2 of 0.26-0.29. The intra-cluster correlation, cluster size and R^2 were computed with previous SIMCE data.

Survey data and treatment application

Before the baseline visit and randomization, we visited each school to obtain signed 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 scores for these students.[4] After obtaining these lists from each

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[1] 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.

[2] 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)

[3] In sample selection, we also approached three schools in the next highest income class, in order to make up our sample size.

[4] 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

school, we grouped schools into five strata by SIMCE 2007 score and randomly assigned schools to one of three groups (group A-Student, B-Family or C-Control) within these strata. Randomization at the school-level avoids the possibility of information spillovers within schools. One Grade 8 class was randomly selected from each school to participate in the study. [5]

The baseline survey and the intervention were conducted in July and August 2009; the follow-up survey in late November and early December 2009. All students present at baseline and follow-up filled in a self-reported survey and were asked to take home parent surveys that were to be returned to school the following week. To incentivize return of the parent survey, we ran a lottery in which students who returned their parent surveys had the chance to win a computer. 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 questionnaires (one, two or three times). This follows one solution to selected survey non-response suggested in Dinardo, McCrary and Sanbonmatsu (2006).

At the follow-up time, we also collected school records on absenteeism for Grade 8 students for the whole year, up to the date of the follow-up visit. Schools were not expecting us to collect this information, and so it is unlikely that they could have retrospectively tampered with these administrative records.

A nation-wide teacher strike occurred 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 225 schools.

To complement our survey data, we obtained administrative data from the Ministry of Education on student outcomes at the end of Grade 8 (reported at the school-level), student scores on the 2008 SIMCE, the outcome of Grade 8 (pass/fail/withdraw) and the school in which the student is enrolled in 2009. We match all of our survey data with the administrative data using the national identification number.

Nature of the treatment and randomizing financial aid information

Students were shown the DVD in class in group A (*Student* treatment) schools, and students in group B

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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.

[5] Questionnaires were printed with the students name and national ID, and the enumerator individually gave them to each students. We therefore expect none, or minimal, manipulation of the students that received the intervention within the school.

(*Family* treatment) schools were given a copy of the DVD to take home and watch with their parents. At the end of watching the program at school, a short follow-up questionnaire was also completed by students in group A schools.

Treating the parents separately from children was deemed infeasible for budget reasons. We were not able to visit each household separately. We also chose not to provide students in the *Student* treatment with their own DVD because it would then be impossible to separately identify the impact of family being exposed to the DVD and the impact of the student watching the DVD more than once (an intensity effect).

Since our sample is based in urban Chile, we expected most students to know that earnings increase with education level and to have a good idea of what this slope is like, both in the aggregate and idiosyncratically. Several small focus groups before baseline confirmed this idea. However, to provide even more evidence on this, we asked variants of two simple questions of two halves of the control group at the end of the baseline survey: [6]

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

In Appendix A Figure 1, 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 (MIDEPLAN, 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.

Students living in urban Chile are very different from students in rural parts of the Dominican Republic.

[6] Researchers have tried to elicit returns to education information from students for some time (see Dominitz and Manski, 1997; Avery and Kane (2004), Rouse (2004), Jacob and Wilder (2010)). 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.

As Jensen (2010) explains, students in rural areas of that country are unlikely to be exposed to successful individuals from whom they can infer returns to education. The setting in urban Chile is also different from the situation facing Grade 3 students in rural Madagascar, where Nguyen (2008) shows that less than half of parents have completed primary school. In both of these studies, information about Mincer returns to education did have sizeable effects on effort in school. The information in Appendix A Figure 1 suggests, in contrast, that manipulating information about wages for different levels of education might not have been useful among Chilean students and that any effects that we see from our treatment are unlikely to operate through changes in the expected returns to education.

We defined the intervention around two critical elements that may affect early educational choices of children from poor backgrounds in addition to returns to education information: knowledge about loans and scholarships opportunities and role models. While the program was primarily designed to provide financial aid information, there is an aspect of motivation and inspiration inherent in the messages provided by the “role models” in the DVD. 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 similar backgrounds managed to pursue higher education (50% of students in the sample report knowing someone in their family that has studied after high school). Our program, providing a standardized message to students, emphasizes the importance of good high school performanceappn, produced by effort at school, for eligibility and access to financial aid for future studies.

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 “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.[7] Using test score data on what information was retained from the DVD, we show that both students and parents learned new information. This suggests that any motivation effect cannot account for all of the impact on outcomes.

[7] Dynarski and Scott-Clayton (2006) discuss different “default options” that teenagers from poor and rich socioeconomic backgrounds may face.

Appendix A Figure 1

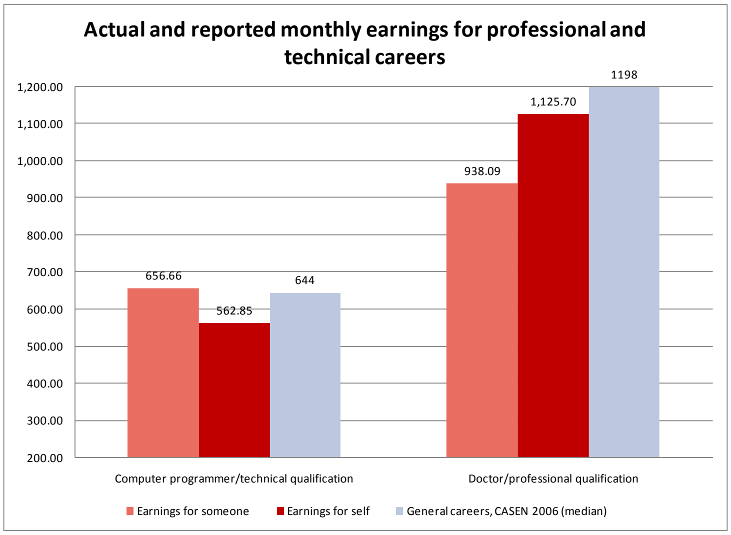

Figure shows median monthly earnings in USD (533 pesos=1 USD in 2009) for different levels of education. Light blue bars represent (weighted) median monthly earnings computed from actual data of adult workers age 30-40 years, who are either technically or professionally trained ("General Careers" from 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").

Appendix B Table 1: Item non-response for outcome variables from survey questions: Balance across groups at baseline and follow-up, OLS

	Want to study after high school		Want to study at college		Want to study at vocational school		Scholarship finance		Loan finance		Family finance		No idea how to finance	
	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up	Baseline	Follow up
Combined Treatment variable														
Any exposure to *"Abre le Caja"*	-0.012*	-0.003	-0.002	-0.016	-0.002	-0.016	0.00	-0.011	0.00	-0.011	0.00	-0.011	0.00	-0.011
	(0.007)	(0.004)	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)	(0.016)	(0.015)	(0.016)	(0.015)	(0.016)	(0.015)	(0.016)
Constant	0.055***	0.020***	0.297***	0.349***	0.297***	0.349***	0.276***	0.332***	0.276***	0.332***	0.276***	0.332***	0.276***	0.332***
	(0.005)	(0.003)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
N	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Separate Treatment variables														
A: *Student* treatment	-0.022***	-0.005	-0.002	-0.023	-0.002	-0.023	0.003	-0.017	0.003	-0.017	0.003	-0.017	0.003	-0.017
	(0.007)	(0.004)	(0.020)	(0.020)	(0.020)	(0.020)	(0.019)	(0.020)	(0.019)	(0.020)	(0.019)	(0.020)	(0.019)	(0.020)
B: *Family* treatment	-0.002	0.000	-0.001	-0.009	-0.001	-0.009	-0.003	-0.005	-0.003	-0.005	-0.003	-0.005	-0.003	-0.005
	(0.008)	(0.006)	(0.019)	(0.019)	(0.019)	(0.019)	(0.018)	(0.019)	(0.018)	(0.019)	(0.018)	(0.019)	(0.018)	(0.019)
Constant	0.055***	0.020***	0.297***	0.349***	0.297***	0.349***	0.276***	0.332***	0.276***	0.332***	0.276***	0.332***	0.276***	0.332***
	(0.005)	(0.003)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
N	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009	5,009
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pval joint test, A, B=0	0.00	0.46	0.99	0.53	0.99	0.53	0.96	0.69	0.96	0.69	0.96	0.69	0.96	0.69

Each column shows coefficients from a regression of a missing value indicator for that variable on treatment assignment variables and a constant. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. In each regression, sample is restricted to students present at baseline and at follow-up.

Appendix B Table 2: School misreports of student attendance by group assignment: OLS

	Outcome is school misreporting rate: fraction of students with conflicting attendance data from school records and our survey records	
	Baseline	Follow up
Combined Treatment variable	0.008	0.002
Any exposure to *"Abre le Caja"*	(0.007)	(0.018)
N	137	111
R^2	0.03	0.05
Separate Treatment variables		
A: *Student* treatment	0.017	0.008
	(0.011)	(0.022)
B: *Family* treatment	-0.002	-0.008
	(0.006)	(0.022)
N	137	111
R^2	0.06	0.05

Each column shows coefficients from a regression of the school misreporting rate on treatment group assignment, a constant and SIMCE stratum fixed effects. Column (1) shows results for regressions at baseline, and column (2) for regressions at follow-up.*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. The sample includes all students on the school registers we collected before baseline who also have matched school attendance data. The school misreporting rate is the fraction of students in the sample whose attendance information on the day of our survey visits conflicts with the attendance information from their class registers on the same day.

Appendix B Table 3: Predicting which students watch the DVD in Family treatment group: Probit marginal effects

	Student watched the DVD
Age	-0.110**
	(0.05)
Female	0.100
	(0.079)
Mom completed HS	0.003
	(0.089)
Mother education missing	-0.135
	(0.122)
Medium grade	0.285***
	(0.074)
High grade	0.421***
	(0.147)
School-reported grade 7 score missing	-0.008
	(0.136)
Impatient	-0.123
	(0.105)
Impatient missing	-0.049
	(0.249)
School poverty rank	-0.009*
	(0.005)
School poverty rank missing	-0.050
	(0.128)
Private voucher school	0.351**
	(0.142)
N	1,234

Coefficients are estimated marginal effects from a probit regression of whether the student reports watching the DVD or not. *** 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 and a missing Grade 7 score indicator. Sample is restricted to students in Family treatment group present at baseline and follow-up.

Appendix B Table 4: Effects of program assignment on expectations and knowledge: IV and reweighting

Panel A	Financing methods: Scholarships				Financing methods: Loans				Score on eligibility rules test (max =5)			
	OLS	Reweight A	IV B	RW & IV	OLS	Reweight A	IV B	RW & IV	OLS	Reweight A	IV B	RW & IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A: *Student* treatment	-0.013	0.035	-0.013	0.035	0.0480***	0.0631***	0.0480***	0.0631***	0.036	0.061	0.036	0.061
	(0.021)	(0.022)	(0.021)	(0.022)	(0.014)	(0.015)	(0.014)	(0.015)	(0.041)	(0.045)	(0.041)	(0.045)
B: *Family* treatment	0.0633**	0.0633**	0.0968**	0.0968**	0.0430***	0.0430***	0.0657***	0.0657***	0.1049**	0.1049**	0.1764**	0.1764**
	(0.027)	(0.027)	(0.041)	(0.041)	(0.015)	(0.015)	(0.023)	(0.023)	(0.043)	(0.043)	(0.070)	(0.070)
N	3,371	3,371	3,371	3,371	3,371	3,371	3,371	3,371	5,009	5,009	5,009	5,009
Control group mean	0.32	0.32	0.32	0.32	0.10	0.10	0.10	0.10	0.00	0.00	1.23	1.23
Pval for joint test of A, B	0.03	0.04	0.02	0.04	0.00	0.00	0.00	0.00	0.05	0.04	0.04	0.03
Pval for test of A<B	0.00	0.17	0.00	0.06	0.39	0.14	0.22	0.46	0.08	0.20	0.02	0.05
Minimum detectable diff: (A-B)	0.11 s.d.	0.11 s.d.	0.11 s.d.	0.11 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.

Panel B	Study after high school?				Future education expectations: Will study technical school				Will study in college			
	OLS	Reweight A	IV B	RW & IV	OLS	Reweight A	IV B	RW & IV	OLS	Reweight A	IV B	RW & IV
A: *Student* treatment	0.016	0.0610***	0.016	0.0610***	0.005	-0.029	0.005	-0.029	0.005	0.049	0.005	0.0485*
	(0.020)	(0.022)	(0.020)	(0.022)	(0.026)	(0.028)	(0.026)	(0.028)	(0.027)	(0.029)	(0.027)	(0.029)
B: *Family* treatment	0.007	0.007	0.011	0.011	0.003	0.003	0.004	0.004	0.037	0.037	0.056	0.056
	(0.019)	(0.019)	(0.032)	(0.032)	(0.025)	(0.025)	(0.038)	(0.038)	(0.027)	(0.027)	(0.041)	(0.041)
N	4,917	4,917	4,917	4,917	3,300	3,300	3,300	3,300	3,300	3,300	3,300	3,300
Control group mean	0.68	0.68	0.68	0.68	0.42	0.42	0.42	0.42	0.36	0.36	0.36	0.36
Pval for joint test of A, B	0.74	0.02	0.74	0.02	0.98	0.52	0.98	0.52	0.39	0.17	0.38	0.16
Pval for test of A<B	0.34	0.01	0.44	0.06	0.47	0.15	0.50	0.20	0.16	0.36	0.11	0.43
Minimum detectable diff: (A-B)	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.09 s.d.	0.11 s.d.	0.11 s.d.	0.11 s.d.	0.11 s.d.

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the school-level. No additional controls in these regressions. Reweighted regressions are inverse probability weighted, as described in the text. IV regressions instrument for student watching the DVD at home, using assignment to *Family* treatment group as the instrument. Minimum detectable effect size is minimum difference in *Student-Family* treatment effects that we can detect given our sample size, number of clusters, a power of 0.8 and the intra-cluster correlation in the specific outcome variable at baseline. Sample size varies because of item non-response and skip pattern of questions (e.g. only kids answering that they want to study after high school were asked where they would like to study).

Appendix C: Attrition and absenteeism

As noted in Table 1, we find no evidence of differential attrition across school groups. This lack of differential attrition across treatment and control groups gives us more confidence in the internal validity of our results. However, 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. Given that school-attendance is one of our measures of child effort, 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, differential attrition across groups at follow-up could cast doubt on the internal validity of our impact estimates for all other outcomes, as it would in any experimental setting.

Attendance measured on the day of our follow-up survey is unlikely to be a good measure of effort in school, since our visits were pre-arranged with school principals and students might have behaved differently on these days. 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. To investigate whether attendance is different on our survey date, we use daily attendance data for the control group only and estimate a set of OLS regressions for whether a student is absent from school on a given school day or not, for all school days after the baseline and up to the end of our sample period. The idea here is that if our follow-up survey day is "different" to other school days, then we should find absenteeism is significantly lower on our follow-up day, even among control group students who did not receive any information about financial aid.

Appendix C Table 1 presents the output from these regressions and shows the coefficient on an indicator for whether the school day in question is the follow-up visit day or not. Column (1) is from the regression of absenteeism on follow-up visit day indicator and stratum fixed effects; column (2) additionally controls for student fixed effects, column (3) includes student fixed effects and column (4) combines student and day fixed effects. Columns (5)-(8) repeat the same regressions but also include a control for the number of sweets that were given out in the baseline at each school relative to the number of students in the class, and its interaction with follow-up day. This is to test the "Super-8" hypothesis. Standard errors are robust and clustered at the student level, since the observation is a student-school day.

The results clearly show that within the control group, students are 3 percentage points less likely to be absent on a follow-up day, relative to other school days; this rises to 8 percentage points when we include day fixed effects and remains about 7.5 percentage points when both sets of fixed effects are included in column (4). What we learn from this table is that, among control group students, attendance is higher on the follow-up visit day than at other times during the school year.appnx

Furthermore, columns (5)-(8) provide suggestive evidence that this may have been related to the promise of more sweets on our follow-up visit: the interaction of “Sweets” and “Follow-up Indicator” is negative in all cases, although not always statistically significant.

We conclude that attendance on the day of our follow-up visit is not a good measure of the behavioral response to the intervention. Rather, since we are interested in the sustained behavioral effects of our intervention, we use school-reported absenteeism in the month before the survey as our main outcome variables.

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 the rate of absenteeism on the day that we visited schools at baseline is high, at 20%. The students who are unlikely to be at school on the baseline day are, not surprisingly, the students with the lowest Grade 7 scores (results not shown). This means that our results reflect the behavioral responses of higher ability students exposed to different treatments at baseline. However, 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.

Appendix C Table 1: Absenteeism among Control group students on follow-up visit day relative to other school days: OLS

	Outcome is =1 if Student is absent from school on a given day							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Follow-up visit indicator	-0.0359***	-0.0833***	-0.0337***	-0.0756***	-0.0313**	-0.0660***	-0.0353***	-0.0634***
	(0.007)	(0.009)	(0.007)	(0.008)	(0.013)	(0.014)	(0.013)	(0.013)
Sweets					-0.0644**	-0.0845***		
					(0.030)	(0.030)		
Sweets*Follow-up visit indicator					-0.0301	-0.113*	0.0101	-0.0769
					(0.064)	(0.064)	(0.063)	(0.063)
Day FE?	N	Y	N	Y	N	Y	N	Y
Student FE?	N	N	Y	Y	N	N	Y	Y
N	119,765	119,765	119,765	119,765	119,765	119,765	119,765	119,765
R^2	0.00	0.04	0.18	0.21	0.00	0.04	0.18	0.21

Table shows OLS coefficients from the regression of whether a student was absent from school on a given day, on a constant and an indicator for whether the day observed was our follow-up visit day. Subsequent regressions add in day fixed effects, then student fixed effects, then both sets of fixed effects. Observations are student-day observations for school days occuring after the baseline and before the end of our sample data; sample is restricted to students in the control group who were present at baseline. Sweets is a school-level variable for the number of sweets per child that were given out in the baseline, as a results of the Super-8 question.***p<0.01, **p<0.05, *p<0.1 Robust standard errors clustered at the student-level in columns 1, 2, 5 and 6; at the school-level in column 3, 4, 7 and 8). All regressions contain stratum fixed effects that define the quintile of the SIMCE 2007 score distribution into which each school falls and a missing Grade 7 score indicator.