INVESTING IN SCHOOLING IN CHILE: THE ROLE OF INFORMATION ABOUT FINANCIAL AID FOR HIGHER EDUCATION

Taryn Dinkelman and Claudia Martínez A.*

Abstract—We investigate the impacts of providing low-income Chilean adolescents with information about how to finance higher education and ask whether providing parents with the same information magnifies the effects on schooling outcomes. We randomly assigned eighth graders and some parents to receive information about aid for higher education. Exposure to information raised college preparatory high school enrollment, primary school attendance, and financial aid knowledge, with gains concentrated among medium- and high-grade students. Parental exposure to information did not significantly magnify these effects. Our results demonstrate that access to relevant information about financial aid affects important schooling choices long before tertiary education begins.

I. Introduction

FINANCIAL aid programs for higher education are remarkably common throughout the world. Almost all OECD countries, half of all African countries, one in three Latin American countries, and about one in six countries in Asia, including China, have means-tested loan or scholarship programs for students pursuing higher education (Marcucci & Johnstone 2010). These programs are often touted to broaden access to higher education by relieving credit constraints for the poorest families.[1] However, academic preparation for financial aid eligibility is typically cumulative, starting long before the end of high school. Lacking information about how to finance higher education, families who might benefit most from this financial aid may end up being the least able to take advantage of it. These information constraints are likely to be especially binding in developing countries, where rapid education transitions mean that parents have little direct experience with systems of higher education.

In this paper, we investigate whether relaxing this information constraint before high school even starts can increase investments in schooling at this early stage. To do this, we randomly assigned over 6,000 eighth graders in 226 poor urban Chilean schools and some of their parents to receive standardized information (in the form of a short DVD program) about financial aid opportunities for higher education. We measure the impact of this exposure on schooling investment choices (school attendance, school grades at the end of the year, and high school choice) three to seven months later using survey and administrative data.

A novel aspect of our work is that we investigate whether delivering information to children together with their parents is substantially more effective at improving school outcomes than providing the same information to children alone. Previous studies that analyze the impact of providing different kinds of information on schooling outcomes have targeted information to either children or parents. We analyze how much new financial aid information matters for outcomes and whether parental involvement magnifies these impacts by randomly allocating schools to a control group (no information DVD), a *Student* treatment group (in which only children watch the DVD at school), or a *Family* treatment group (in which we send a copy of the DVD program home with all children and encourage them to share it with their parents). To our knowledge, this is the first study to investigate whether parental learning about the higher education production function interacts with adolescent learning to affect treatment response in a randomized experiment setting.[2]

Our research has the potential to shed light on how early information about future financial aid is important for investments in schooling. Specifically, the results of our study can be used to answer three questions: (a) Does exposure to new information about financial aid for higher education increase effort in school four years before the end of high school? (b) Does increasing parent knowledge magnify any impacts? (c) Do either of these ways of delivering the new information increase the likelihood of accessing financial aid and continuing to higher education?

Received for publication July 15, 2011. Revision accepted for publication December 19, 2012.

* Dinkelman: Dartmouth College, BREAD, and CEPR; Martínez A.: Department of Economics, Faculty of Economics and Business, University of Chile.

Funding for this project was provided by the Inter-American Development Bank, the Center for Economic Policy Studies at Princeton University, and PEP-NET (Poverty and Economic Policy Research Network). We thank Claudia Peirano and her team, especially Valentina Rivera, Patricia Reyes, and Ilana Nussbaum, at the Centro de Microdatos at the Universidad de Chile for their tireless work in project implementation; Ana María Baeza and Paola Leiva at the Ministry of Education, Chile, for assistance with administrative data; and Alberto Chong (IDB) for his continuous support of the study. We thank Patty Anderson, Erzo Luttmer, Nancy Qian, Tomas Rau, Sam Schulhofer-Wohl, Dean Yang, and seminar participants at Bocconi University, Eidaudi Institute for Economics and Finance, the Harris School of Public Policy, MIT, Princeton, Yale, the University of Cape Town, Universidad de Chile, Universidad Católica de Chile, Universidad Adolfo Ibañez, Impact Evaluation Network-LACEA, Universidad Diego Portales and the Rimini Conference in Economics and Finance (2010) for useful comments.

A supplemental appendix is available online at http://www.mitpressjournals.org/doi/suppl/10.1162/REST_a_00384.

[1] There is evidence that credit constraints reduce access to higher education for some families in the United States (Angrist, 1993; Dynarski, 2002, 2003; Bound & Turner, 2002), in Mexico (Kaufmann, 2008) and in Chile (Meneses et al., 2010; Solis, 2010). Concerns about low awareness of public financial aid and the complexity of applications in the United States have motivated recent work exploring whether direct assistance with applying for financial aid just prior to college enrollment can improve take-up among disadvantaged families (Bettinger et al., 2012). In contrast, Carneiro and Heckman (2002) argue that credit constraints are far less important than the cumulative effect of early-life disadvantages on college preparedness of students from low socioeconomic backgrounds in the United States.

[2] The standard human capital investment model tends to consider a unitary decision maker, but recent empirical evidence suggests it may be important to consider whether parents or children are the relevant agents in education decisions, since they may face different incentives (Berry, 2009; Burztyn & Coffman, 2012), hold different expectations about desired level of education and costs of investing (Attanasio & Kaufmann, 2009; Giustinelli, 2010), or be differentially forward looking (as suggested in Gruber, 2001; Dobbie & Fryer, 2011).

We concentrate on addressing the first two questions, since our respondents have not yet completed high school. Using the experimental variation in school assignment to groups, we show that exposure to new information about financial aid availability and eligibility has important impacts on schooling behaviors in the short run. Exposure to treatment raises the probability of enrolling in a college preparatory high school by about 6 percentage points (10%) among those students required to choose a new school in grade 9. Exposure to the information DVD also significantly lowers the probability of being absent from school by between 8 and 10 percentage points (12% to 15%) three months after baseline. This reduced absenteeism does not translate into improved test scores at the end of grade 8, five months after the intervention.

Our point estimates from the *Intent to Treat* comparisons suggest slightly smaller effects of students being exposed to the information DVD at school (without parents) than of exposure at home (with parents). Given the design of our experiment, we are able to reject that the *Family* treatment has a significantly larger impact on behavior than the *Student* treatment, but we cannot rule out small differences in effect sizes.[3] This is despite the fact that we show that parents in the *Family* group learned and retained much more about the details of the financial aid programs relative to all other parents. Our interpretation of these findings is that at least some students in both groups responded to the new information regardless of what their parents knew.

From a policy perspective, as well as to shed more light on behavioral responses, it is important to consider which students were marginal for this intervention. We do this by pooling the two treatments to maximize power and estimating different responses for students who have high, medium, and low school grades at baseline. While school grades are not a perfect measure of ability, they provide some signal about an individual's suitability for higher education.

Through this analysis, we show that relevant information from the DVD was retained by the relevant students. That is, while all students exposed to the DVD retained some knowledge about financial aid programs, those with medium and high grades at baseline score the highest on tests of financial aid knowledge. High-grade students are more likely to report wanting to use scholarship and loan finance for further education and report a change in preference away from vocational postsecondary education toward a college education. In contrast, low-grade students remember some information from the DVD, particularly about loan assistance, and appear to switch their choice of postsecondary education from college to vocational schooling. The students with high and medium grades at baseline, and particularly those with medium grades, contribute the most to the attendance and high school choice results. These heterogeneous effects suggest that the students with better chances of qualifying for financial aid later (medium- and high-grade students) were the ones most marginal for the improved education outcomes.

The behavioral responses to this information intervention are important for several reasons. First, since there is mixed evidence on whether providing direct information about school quality promotes more school choice, it is notable that students exposed to the financial aid information in this DVD are more likely to enroll in college preparatory high schools.[4] The fact that a relatively small amount of information (provided at a cost of between $11 and $13 per student; all dollar amounts are in U.S dollars) can have meaningful impacts on enrollment is encouraging and suggests that the right information provided at the right time might do a lot to promote school choice and, with it, improvements in educational outcomes in the longer run (Pop-Eleches & Urquiola, 2011). Second, since enrollment responds only among the set of students constrained to choose a new school at the end of grade 8 (the majority of the sample), it would seem that providing early information about financial aid could be effective whenever there is no default option for continuing education. Third, despite the fact that the information intervention did not affect test scores in the short run, improved attendance suggests that students do invest more in schooling. While it remains to be seen whether these investments ultimately affect the uptake of financial aid and access to postsecondary schooling, improved attendance may have other positive long-term payoffs (for example, higher wages, reduced teenage pregnancy), as some studies have shown.[5]

The results are also important from a public policy perspective in Chile. Chile has achieved massive increases in secondary schooling in the past two decades and has been a leader in creating a school market aimed at encouraging school choice, yet the country still struggles with extreme inequality in access to higher education.[6] In 2009, only

[3] This finding of statistically indistinguishable differences between the impacts of *Student* and *Family* treatments prevails when we use treatment assignment to instrument for whether the student reported watching the DVD at home.

[4] Mizala and Urquiola (2007) find that school choice does not respond to new information about school quality in Chile. In rural Pakistan, Andrabi, Das, and Kwhaja (2010) show that providing parents with school and child "report cards" put pressure on schools to adjust quality or school tuition but had little impact on actual movement across schools in equilibrium. In the United States, school report cards and school quality rankings have been found to affect school choice among low-income parents (Hastings, Van Weelden, & Weinstein, 2007; Hastings & Weinstein, 2008).

[5] Our absenteeism results are within the range of effects from several other studies measuring the impact of information and health interventions on school attendance. Nguyen (2008) finds large attendance effects of an information intervention in Madagascar; Bobonis, Miguel, and Puri-Sharma (2006) show attendance gains in response to deworming and iron supplementation in India; Kremer and Miguel (2004) show attendance gains in Kenya (without concomitant test score improvements) in response to deworming; Baird et al. (2011) show long-term wage gains in response to childhood deworming, despite the absence of test score gains in childhood; Berthelon and Krueger (2011) find that a longer school day translates into fewer teen pregnancies in Chile.

[6] There is also an ongoing debate about the quality of higher education in Chile; this is not something we address in this paper.

16% of 18- to 24-year-olds from the poorest households were enrolled in tertiary education, compared with 61% of young adults in households in the top income decile (MIDEPLAN, 2009). Our study provides evidence that students from these lowest quintiles, particularly those with grades suited for further study, can be motivated to increase their investments in schooling in response to new and cheap-to-provide information about financial aid opportunities, regardless how this information is delivered (at school or at home) and even when it is provided early on.

Our work is related to two strands of literature in economics. First, we contribute to the growing literature suggesting that information constraints matter for optimal human capital investment decisions. Recent studies by Jensen (2010) for the Dominican Republic and Nguyen (2008) for Madagascar have shown that providing a small amount of information about Mincer returns to education has large impacts on school attendance, test scores, primary and high school continuation, and high school completion in poor, rural settings.[7] In contrast, field experiments in richer countries (Bettinger et al., 2012) have shown that information about financial aid for higher education is insufficient for raising enrollment in higher education.[8] The results of our study fall between these developing and developed country results, with information about financial aid for higher education affecting enrollment and attendance but having no impact on test scores in the short run. In the case of urban Chile, students were reasonably aware of the wage returns to higher education at baseline (see appendix A in the online supplement) and had high aspirations for further education as we show, but they were less clear about how to finance their higher education goals at baseline. Our intervention targeted this particular information gap. An emerging implication from all of these studies is that different kinds of information will matter for different sorts of education outcomes, and the impact of providing more complete information, whether regarding wage returns or financial aid or school quality, is context dependent.

Second, our results speak to the larger literature on optimal human capital investment. In this literature, parents are often treated as the key actors for education investment choices, and many programs are geared toward providing parents with information and resources to enable better decision making about schooling—for example, student report cards, information about school rankings, and direct parent training (Avvisati et al., 2010). These programs may be particularly relevant if students are sufficiently myopic about future returns. Yet our results indicate that for some types of school effort choices, at least some adolescents may be able to act on the new information they receive regardless of whether their parents have the same information. Future research into when an adolescent grows into the role of decision-making agent appears warranted and is likely to inform our understanding of how parents and children make decisions about investing in schooling.

II. Background: Education in Chile

Chilean children must complete twelve years of education: eight years of primary school and four years of high school. A well-known national school voucher program allows students to attend free municipal schools or private voucher-subsidized schools that charge tuition.[9] The choice about which high school to attend is made at the end of grade 8, when students enroll in a traditional high school offering preparation for postsecondary studies or a vocational high school that provides limited opportunities for further study. Data from the Chilean Ministry of Education (MINEDUC) highlight the importance of this choice for higher education: in 2006, 45% of students enrolled in college preparatory high schools went on to enroll in some form of higher education; the comparable rate among vocational high school students was just 14%. In our sample, 75% of students attend primary schools that terminate in grade 8, and these children must 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. We consider how grade 9 enrollment choices respond to exposure to financial aid information for each of these two groups.

After completing high school, students can pursue vocational certifications (for two to three years) or college degrees (for five years). The most recent statistics, from 2007, show that 68% of postsecondary students were enrolled in colleges and the rest in vocational training schools. Tuition costs are high for both options: in 2005, average annual tuition for technical studies was 20% to 25% of annual per capita income and average college tuition was even higher, at 42% to 47% of per capita income (OECD, 2009).

Financial aid programs for postsecondary education have recently expanded in Chile. Publicly provided scholarships increased from $40 million in 2000 to $173 million in 2007 (OECD, 2009). In 2006, the government loan program was expanded beyond traditional colleges to cover postsecondary technical studies, complementing existing privately funded scholarships. As a result, almost half of students enrolled in higher education receive some type of financial aid, with the majority of this aid being in the form of loans (Comisión de Financiamiento Estudiantil, 2012).[10]

[7] 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).

[8] Bettinger et al. (2012) 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.

[9] School choice and school outcomes in relation to the voucher system in Chile have been the focus of several important studies (Hsieh & Urquiola, 2006; Mizala & Urquiola, 2007; Urquiola & Verhoogen, 2009; Bravo, Mukhopadhyay, and Todd, 2010).

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

Students from poor backgrounds and particularly those attending free municipal schools face considerable disadvantages in accessing this financial aid. Almost all scholarship and loan programs require good grades in grades 9 to 12 and good performance on a standardized exam (the Prueba de Selección Universitaria, or PSU, which is similar to the SAT). Each program has distinct eligibility cutoffs for school grades and PSU scores, with scholarships and college aid programs requiring higher scores than loans and vocational training aid programs. Students attending free municipal schools have significantly lower pass rates on the PSU and are less likely to earn the types of high school grades required to qualify for any form of financial aid (OECD, 2009) or be enrolled in college preparatory high schools. These students are also likely to have parents who have not completed high school, putting them at an even greater disadvantage in terms of getting advice on how to attain higher education goals. It is not surprising that although enrollment in higher education in Chile more than tripled between 2000 and 2011, only one-quarter of all enrolled students in 2009 were from the lowest two income quintiles (MINEDUC, 2012). These are the types of students we target in our study.

III. The Intervention, Experimental Design, and Data

A. *The Intervention*: Abre la Caja

The intervention provides students with information about how effort and good grades in school open up opportunities for further study by increasing the likelihood of being eligible for government scholarships and loans. Since high school performance is critical for financial aid eligibility and admission to postsecondary schooling in Chile and because over 60% of Chilean students must choose a high school and a specific type of study at the end of eighth grade, we designed our intervention to target children in grade 8, four years before the relevant time for college and vocational training applications. This timing differs from recent experiments that target information interventions either much closer to the end of high school (Bettinger et al., 2012) or at the start of primary school (Nguyen, 2008).

We developed and produced a fifteen-minute DVD entitled *Open the Box* (*Abre la Caja*) showcasing the higher education experiences of thirteen adults (five women and eight men; nine professional and four technical careers) who grew up in poor families in urban Chile.[11] In the program, each person talks about how, by working hard at school and becoming eligible for financial aid, he or she was able to finance the postsecondary education at traditional colleges or at vocational schools. These life stories provided specific details about relevant grade cutoff scores and PSU cutoff scores for scholarship and loan eligibility.

Respondents who watch the DVD receive a new, standardized signal about how effort in earlier years translates into postsecondary 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; another is taking the PSU. In essence, they learn about some of the key inputs into the production function for higher education. If they pay attention to the DVD, they should be more informed about financial aid requirements, and we measure how much direct information viewers retain from the DVD at follow-up. To the extent this information is relevant to a particular student, we should see differences in plans for financing further education and in schooling investment behaviors.

B. *Experimental Design*

Our study takes place in metropolitan Santiago in a sample of 226 schools in the lowest two income quintiles as defined by government administrative records (see appendix A for details of our sampling frame and school recruitment). We stratified the sample on school-averaged grade 8 SIMCE scores from 2007, and treatment assignment was randomized within strata and at the school level to avoid information spillovers at the grade level.[12] Fifty-six 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 delivering financial aid information to children together with their parents magnified the impacts relative to providing information to children alone. For this reason, we designed exposure to the DVD to occur in two ways. First, we showed the DVD to all students in the *Student* treatment at school after our baseline survey. All children present in class the day of our visit watched *Abre la Caja*. The second delivery method was constrained by budget and logistical concerns and motivated by how the Ministry of Education typically collects information from the parents: by sending the SIMCE questionnaires home with children. Since we could not treat parents separately from children or visit each household to ensure that all parents watched the DVD, we provided students in the *Family* treatment with their own copy of the DVD and encouraged them to watch at home with their parents (also, after the baseline). While this design means that not all of the students in the *Family* treatment actually watched the DVD, it is still valid to compare the impacts of these two practically

[11] Although we obtained a letter of support for our research from the Ministry of Education and used this in an introduction to the principals, there was no mention of the Ministry of Education in the DVD or in any of the baseline or follow-up surveys. A link to a copy of the program is available at http://works.bepress.com/claudia_martinez_a/7/.

[12] SIMCE (2008) stands for System of Measurement of the Quality of Education (Sistema de Medición de Calidad de la Educación). The system is a standardized test that assesses student competencies in fourth, eigth, and tenth grades.

TABLE 1.—COMPOSITION OF SAMPLE, RESPONSE RATES, ATTRITION, AND MATCH RATES WITH ADMINISTRATIVE DATA

	Full Sample	*Control* group (C)	*Any Treatment* (T)	*Student* Treatment (A)	*Family* Treatment (B)
Distribution of sample					
Number of schools at baseline	226	114	112	56	56
Number of schools at follow-up	225	114	111	56	55
Retention rate of schools (1-attrition)	1.00	1.00	0.99	1.00	0.98
Number of students on class roster	7,696	3,902	3,794	1,908	1,886
Number of students present at baseline	6,233	3,179	3,054	1,536	1,518
Attendance rate at baseline	0.81	0.81	0.80	0.81	0.80
Number of students present at follow-up (analysis sample)	5,009	2,560	2,449	1,254	1,195
Retention rate of baseline student sample (1-attrition)	0.80	0.81	0.80	0.82	0.79
Response rate on parent survey at baseline	0.75	0.75	0.75	0.76	0.74
Response rate on parent survey at follow-up	0.58	0.58	0.57	0.56	0.57
Match rates with administrative data					
Number of students with matched June absenteeism data	3,600	1,992	1,608	884	724
Fraction baseline students with matched absenteeism data, June	0.58	0.63	0.53	0.58	0.48
Number of students with matched September absenteeism data	3,615	1,998	1,617	887	730
Fraction of baseline students with matched absenteeism data, September	0.58	0.63	0.53	0.58	0.48
Number of students with matched grade 7 scores data	5,492	2,822	2,670	1,321	1,349
Fraction baseline students with matched grade 7 scores	0.88	0.89	0.87	0.86	0.89
Number of students with matched grade 8 scores	6,181	3,145	3,036	1,529	1,507
Fraction baseline students with matched grade 8 scores	0.99	0.99	0.99	1.00	0.99
Number of students with matched grade 9 enrollment data	5,860	2,982	2,878	1,437	1,441
Fraction baseline students with matched grade 9 enrollment data	0.94	0.94	0.94	0.94	0.95

The Table provides summary statistics for schools, students, and parents participating in the project. *Any Treatment* group is the combined *Student* and *Family* treatment groups. 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 whom we can match with administrative data based on national identification number. Differences in match rates not statistically significant except for *Family* Treatment students, where match rates for June and September absentee data are significantly lower (10% significance level).

relevant ways of delivering the information. We discuss further issues of comparison (including cost comparisons) of these two treatments in section V.

C. Data

The baseline survey and intervention was implemented in late July and early August 2009 with students responding to self-administered questionnaires in class and each child in every group taking a parent questionnaire home. They were asked to return the parent survey to school a week later, at which time our enumerators collected these surveys.[13] 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. Only one school refused to participate in the follow-up, leaving us with follow-up data for 225 schools. We also collected class registers, baseline grades, and school absenteeism data directly from the schools.[14]

After the 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 the type of school the student was enrolled in the following year. These administrative data allow us to follow key outcomes for our sample after the end of our survey. This unique and rich survey data set, along with the administrative data, allows us to construct a comprehensive picture of how information sets, behaviors, and expectations were affected by exposure to *Abre la Caja*.

D. Summary Statistics

Tables 1 and 2 provide summary statistics for the full sample as well as separately for students assigned to the control group, the *Student* and *Family* treatments, and *Any Exposure* (a pooled group of *Student* and *Family* treatments). We conducted 6,233 student surveys at baseline, and 5,009 students responded at follow-up (80% of baseline). Importantly, the 20% attrition from the baseline sample is balanced across each treatment and control group. Parent response rates are higher at baseline (75%) than at follow-up (58%) but 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.[15]

The second panel of table 1 shows the fraction of the baseline student sample with matched administrative data: almost all students present at baseline have eighth-grade

[13] 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). This follows a solution to selected survey nonresponse suggested in Dinardo, McCrary, and Sanbonmatsu (2006).

[14] Schools were not expecting us to collect this information, making it highly unlikely that they could have tampered with retrospective records.

[15] Since school absenteeism is one outcome of interest and absenteeism is high at baseline and follow-up, we discuss attrition in more detail in appendix B in the online supplement 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.

TABLE 2.—SUMMARY STATISTICS AND BASELINE BALANCE TESTS

	Full Sample			*Treatment* Groups			
				Any (T)	*Student* (A)	*Family* (B)	*Control* group (C)
	Mean	SD	Number	Mean	Mean	Mean	Mean
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 educator indicator	0.15	0.35	6,233	0.15	0.17^{Λ}	0.14	0.14
Grade 7 score is low	0.31	0.46	6,233	0.33**	$0.34^{\Lambda\Lambda}$	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	$0.104^{\Lambda\Lambda}$	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
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 with scholarships	0.36	0.48	4,466	0.37	0.36	0.37	0.35
Pay for studies with loans	0.11	0.31	4,466	0.11	0.10	0.11	0.10
Family pays for studies	0.39	0.49	4,466	0.40	$0.42^{\Lambda\Lambda}$	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							
Fraction with 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
Fraction providing attendance data	0.78	0.41	226	0.78	0.85	0.73	0.77

The table shows summary statistics for variables collected in our survey or from administrative data (∼ from schools, $ from 2007 SIMCE data). The sample for student-level variables and absenteeism variables includes all students present at baseline; the sample for student expectations questions is further restricted to the analysis sample present at follow-up. We impute missing values of control variables using the mean values for nonmissing observations or a value of 0 for indicator variables. A missing indicator variable is included in all regressions to flag these imputed observations Missing values for outcome variables are not imputed. In comparing T and C, *** denotes difference significant at the 1% level, ** at the 5% level, and * at the 10% level. Similar notation is used to indicate statistically significant differences between A and C ($^{\Lambda\Lambda\Lambda}$, $^{\Lambda\Lambda}$, and $^{\Lambda}$) and between B and C ($^{+++}$, $^{++}$, and $^{+}$). There are no statistically significant differences in any means in the group A–group B comparison. See the online appendix tables for an analysis of balance in item nonresponse.

scores and ninth-grade enrollment data.[16] Match rates with seventh-grade scores and absenteeism data (both collected from schools) are lower, at 88% and 58%, respectively, but not significantly different across treatment and control schools.[17]

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. For most variables, means are computed over the sample of students present at baseline. For the outcomes related to education expectations and financial aid information questions, we further restrict the sample to students present at follow-up (the analysis sample for these outcomes).

According to the 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 have parents without any experience in graduating from high school, let alone continuing to postsecondary education.

The range of grades in our sample runs from 40 to 70. We group students into three grade groups: high grade if their seventh-grade score is between 60 and 70, medium grade if the score is between 50 and 60, and low grade if their seventh-grade score is less than 50. These cutoffs are the norm in Chile and represent, respectively, very good, good, and sufficient performance. High scores are in the eligible range for financial aid and college entry; medium scores are in the range where acceptance to college and financial aid eligibility is less certain but entry to vocational training should be feasible; low scores would not qualify for financial aid or for entry into most postsecondary institutions. Although these groupings are not a perfect measure of ability, they provide a useful within-school ranking of students and are directly relevant for financial aid eligibility. Twenty-nine percent of the control group is in the low-grade group, 58% in the medium-grade group, and 13% earn high grades at baseline.

Children in our sample come from families in the two lowest-income quintiles in Chilean society. Yet these are

[16] 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.

[17] June 2009 is the closest month prior to the baseline; September 2009 is the only postintervention month with complete attendance data. 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, and inaccuracies in recording of identification numbers.

urban households 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 our main student-level outcomes from the baseline, and we again focus on discussing control group means.[18] The first striking statistic is that a large fraction of students (77%) report wanting to study beyond high school. This is substantially higher than the 16% of young adults (18 to 24 years) from the lowest two income quintiles who were actually enrolled in any postsecondary education in 2009 (MIDEPLAN, 2009). The students who report that they want to continue with postsecondary studies are split evenly between wanting to continue studies at college (32%) and at vocational or technical schools (32%); the rest were unsure. At baseline, most students planned to finance their postsecondary studies with scholarships and family finance, and a very low fraction (10%) of students reported that they would use loans (multiple mentions were possible for this question). Forty-one percent reported they had no idea how to finance postsecondary education. This high fraction in the "no idea" category is disturbing when seen in conjunction with the high aspirations for further education in our sample. Our intervention is designed to address this financial aid information gap.

The only behavioral outcome data for which we have baseline data are for absenteeism. Average absenteeism reported at baseline (in June) underscores the importance of this measure 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.[19]

Table 2 also shows results of baseline balancing tests for outcome and control variables at baseline by treatment group assignment. We compare differences in means across the combined treatment (*Any Exposure*) with the control group and indicate significant differences by asterisks. we also compare each individual treatment to the control group (Λ denotes significant differences between *Student* treatment and control, $+$ denotes significant differences between *Family* treatment and control), and we compare differences in variables between treatment groups. 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 baseline difference in means is 0 in some cases (DVD ownership, low grade 7 score, and whether the student wants to study in a vocational school). We compute the Bonferroni test for joint significance across all of these balancing regressions and cannot reject the null that all coefficients are jointly equal to 0 or that the differences between *Student* and *Family* group means are jointly equal to 0. This gives us more confidence that treatment and control groups are the same across a range of observable characteristics at baseline.

IV. Empirical Framework

A. Comparing Student and Family Treatments

We compute the Intent to Treat (*ITT*) effects of the *Student* and *Family* treatments on education outcomes Y_{ij} for student i in school j measured at follow-up with

$$Y_{ij} = \delta + \lambda_A \times A_j + \lambda_B \times B_j + v_{ij}, \quad (1)$$

where A_j is an indicator for whether school j is randomized to the *Student* treatment and B_j indicates assignment to the *Family* treatment. v_{ij} is a person-specific error term, every 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 0, jointly and separately and whether they are different from each other.

It is important to note what types of effect sizes the design of the experiment allows us to detect. For our initial power calculations used in the research design, we were able to use only data on the initial distribution of 2007 SIMCE test scores to define appropriate sample sizes for each treatment group. Given these sample sizes and the intracluster correlations in baseline outcomes, we have the statistical power to detect individual *ITT* effect sizes of at least 0.2 standard deviations in SIMCE test scores. However, we have power to detect only much larger differences between the two *ITT* effects. For reference, we use our baseline survey data and the administrative data for each outcome to calculate the minimum detectable differences (MDD) between λ_A and λ_B given our sample size, and report these MDD in the main results table (table 3). The MDDs range from 0.11 to 0.15 standard deviations, depending on outcome. To illustrate what this means, suppose that the impact on absenteeism of being assigned to the *Student* treatment is $\lambda_A = 0.2$ standard deviations. Then as long as the impact of the *Family* treatment was $\lambda_B = 0.31$ standard deviations or higher, we would have power to detect a significant difference between these two effect sizes. However, we would not have the power to detect smaller differences.

[18] 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 nonresponse is balanced across groups at baseline and follow-up in appendix B, table 2, and find that it is for almost all variables. We cannot reject a joint test of the null that all differences in item nonresponse variables are 0.

[19] We check the quality of the administrative data by comparing them to our own records of whether a student was present on the day of our baseline and follow-up visits. 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 whether these students are marked present or absent by their teachers. On average, 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. Regardless of reason, attendance misreporting in school registers is balanced across treatment and control groups.

TABLE 3.—EFFECTS OF EXPOSURE TO *ABRE LA CAJA* ON EFFORT IN SCHOOL AND EDUCATIONAL OUTCOMES: OLS AND IV

	OLS (1)	IV (2)	OLS (3)	IV (4)
A.	Absent in September?		Number of Days Absent in September	
A: *Student* treatment	−0.079*	−0.079*	−0.245	−0.245
	(0.041)	(0.041)	(0.196)	(0.196)
B: *Family* treatment	−0.0970**	−0.2263**	−0.262	−0.612
	(0.046)	(0.107)	(0.204)	(0.478)
Number	3,615	3,615	3,615	3,615
Control group mean	0.64	0.64	2.10	2.10
p-value for test of A = B = 0	0.05	0.05	0.30	0.29
p-value for test of A < B	0.73	0.14	0.94	0.42
Minimum detectable difference: (A − B)	0.15 SD	0.15 SD	0.13 SD	0.13 SD
B.	Grades at the End of Grade 8		Enrollment in College Preparation High School? (Constrained Sample)	
A: *Student* treatment	−0.610	−0.610	0.061	0.061
	(0.396)	(0.396)	(0.042)	(0.042)
B: *Family* treatment	0.145	0.308	0.0641*	0.1319*
	(0.390)	(0.824)	(0.037)	(0.077)
Number	6,181	6,181	4,191	4,191
Control group mean	53.69	53.69	0.60	0.60
p-value for test of A = B = 0	0.21	0.21	0.16	0.16
p-value for test of A<B	0.11	0.25	0.94	0.33
Minimum detectable difference (A − B)	0.11 SD	0.11 SD	0.15 SD	0.15 SD

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Robust standard errors in parentheses, clustered at the school level. SIMCE stratum fixed effects included. IV regressions instrument for student watching the DVD at home (self-report), using assignment to *Family* treatment group as the instrument. Absenteeism data are from school administrative data; grades and enrollment in high school by type are from MINEDUC administrative data. 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 intracluster 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.

Comparing the *ITT* impacts using equation (1) answers the question, "What would happen to outcomes if we implemented this information intervention in this way in other similar schools?" We would also like to understand whether actually watching the DVD (rather than just being assigned to a treatment group) affects outcomes. Since all students in the *Student* group but only 60% of the students in the *Family* group watched the DVD, we instrument for watching the DVD at home using assignment to the *Family* group as the instrument.

Specifically, we estimate

$$Y_{ij} = \alpha + \lambda_A \times A_j + \gamma_B \times Watched\,DVD\,at\,Home_{ij} + e_{ij} \quad (2)$$

where $Watched\ DVD\ at\ Home_{ij}$ is an indicator for whether the student reported watching the program at home at all and is instrumented using $B_{ij} = 1$. This approach reproduces λ_A for the *Student* treatment and (without additional covariates) scales up λ_B by the inverse of the fraction of DVD watchers. We again test whether λ_A and γ_B are significantly different from 0 (separately and jointly) or different from each other.

As with any instrumental variables (IV) approach, equation (2) identifies the impact of watching the DVD at home for students who are "compliers" (Angrist & Krueger, 1999) and may not be representative of the average student. Not surprisingly, these "compliers" are students with higher baseline grades: among all students in the *Family* treatment, 50% of those with grades in the low range watched the DVD, 63% of those with grades in the medium range watched the program, and 68% of those with grades in the high range watched *Abre la Caja*. These differences (not shown) are statistically significant, and large, and they suggest that we should interpret γ_B as the local average treatment effect of watching the DVD at home for students initially performing well at school. These are likely the students for whom the information about financial aid for further education is most relevant.

While comparing λ_A with λ_B in equation (1) is a valid comparison of the *ITT* impacts of delivering information about financial aid in these two ways, we need to be a bit more careful with comparing λ_A and γ_B in equation (2) since this entails comparing the average treatment effect of watching the DVD at school (a weighted average of effects on students across all grade groups) to the local average treatment effect of watching at home (γ_B). Any significant differences between these two effect sizes could either be attributed to the difference that parental exposure to the information makes, or could result from the larger effects of the DVD among compliers.

In Dinkelman and Martínez (2011), we describe one way to adjust the average treatment effect for watching the DVD at school to better represent effects for students who look like compliers in the *Family* treatment group. Our approach, motivated by Horvitz and Thompson (1952), involves creating predicted synthetic probabilities of choosing to watch the DVD for those in the *Student* treatment and then using these predictions to inverse probability weight the estimate of λ_A to represent the impact of *Student* treatment among complier types. As it turns out, this exercise does not alter the conclusions of our comparison between the *Student* and *Family* treatments and so we confine our discussion to the results from equations (1) and (2).

B. *Intent to Treat Effects of Exposure to* Abre la Caja *and Heterogeneity with Respect to Baseline Test Scores*

Beyond showing the average impacts of exposure to financial aid information on outcomes, it is important to learn more about which students are marginal for this specific intervention. To do this, we examine whether DVD exposure affected behavior, information sets, and expectations differently for students with different baseline test scores. Since we could not randomize treatment at the level of the individual student based on their grades, we pool the *Student* and *Family* treatments to maximize power. A student's seventh-grade test score is our 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

$$Y_{ij} = \rho + \beta \times T_j + \varepsilon_{ij}, \tag{3}$$

where T_j 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, we interact T_{ij} with each of the three grade groups and control for medium and high grades at baseline:

$$\begin{aligned} Y_{ij} = \pi &+ \beta_1 \times High_{ij} \times T_j + \beta_2 \times Medium_{ij} \\ &\times T_j + \beta_3 \times Low_{ij} \times T_j + \theta_1 \times High_{ij} + \theta_2 \\ &\times Medium_{ij} + \mu_{ij}, \end{aligned} \tag{4}$$

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. Note that the main effect among low-grade students is absorbed in the constant. Examining the pattern of the interaction coefficients estimated in equation (4) 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.

V. Results

A. *ITT and IV Results for Educational Outcomes*

Table 3 shows the impact of assignment to treatment on school behaviors. The outcomes include an indicator for absenteeism in September and a count variable of the number of days absent in September (panel A), school grades at the end of grade 8 and an indicator for enrollment in a college preparatory high school in ninth grade (panel B). For each outcome, we present *Intent to Treat* comparisons from equation (1) (in columns 1 and 3), and then IV estimates of equation (2) (in columns 2 and 4). Sample size differs across columns because of differential match rates with administrative data or because we restrict the sample to students who attend a terminating primary school in grade 8.

The table shows that some educational behaviors were responsive to treatment assignment while others were not. Starting in panel A of table 3, assignment to *Student* treatment decreases the probability of being absent at all by about 8 percentage points, while assignment to *Family* treatment decreases absenteeism by 9.7 to 22.6 percentage points depending on whether we consider the ordinary least squares (OLS) or instrumental variables (IV) specifications. Watching the DVD at school has a negative but not significant effect on the total number of days absent in September (a reduction of 0.2 days), while being assigned to watch the DVD at home reduces the number of days absent by 0.26 (in the *ITT* estimates) and by 0.61 days per month (also not significant) if we instrument for actually watching the DVD at home.[20] If we scale the coefficients from the *ITT* estimates for number of days absent by moving the intervention earlier in the year to March (rather than midway through the school year), the impact would have been to increase attendance by about 2.5 days over the entire year.

Perhaps not surprisingly, these improvements in absenteeism do not translate into higher test scores by the end of grade 8. Panel B shows that neither treatment has an impact on school grades: not only are the estimated coefficients in columns 1 and 2 statistically insignificant, they are also very small relative to the control group mean. The lack of effect on test scores may have been because follow-up was too soon after the intervention to yield any impacts or because raising test scores requires inputs complementary to student effort, like quality teaching, or textbooks (as in De Fraja, Oliviera, & Zanchi 2010).[21]

However, the estimates in columns 3 and 4 of panel B reveal that students are about 6 percentage points more likely to enroll in a college preparatory high school in grade 9 if they were assigned to either *Student* or *Family* treatments. The point estimates on the *Student* treatment are very similar to those on the *Family* treatment, although only the *Family* treatment indicator is statistically significantly different from 0 at the 10% level.

Overall, exposure to *Abre la Caja* at school or at home reduced absenteeism by about 12% relative to the control group mean and increased enrollment in college preparatory high schools by about 10% relative to the control group mean. The point estimates from both *ITT* and IV comparisons suggest somewhat smaller effects of students receiving the information DVD at school (without parents) than of receiving the information at home (with parents). However, using an F-test, we cannot reject that the two ways of providing the information produce statistically similar beha-

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

[21] The school year in Chile runs from March through December, with various breaks during the year. Our intervention occurred more than halfway through the school year, and we measure the impact on grades after a very short time (five months at most).

TABLE 4.—EFFECTS OF TREATMENT ASSIGNMENT ON KNOWLEDGE: PARENT AND STUDENT SCORES ON ELIGIBILITY RULES TEST BY BASELINE GRADES, OLS

	OLS Regressions				Heckman-Selection Corrected Regressions				
	Student Scores on Eligibility Rules Test (0–5)				Parent Scores on Eligibility Rules Test (0–5)				
	Full Student Sample (1)	High-Grade Group (2)	Medium-Grade Group (3)	Low-Grade Group (4)	Parent Respondent-Sample (5)	Full Sample (6)	High-Grade Group (7)	Medium-Grade Group (8)	Low-Grade Group (9)
A: *Student* treatment	0.034	0.052	−0.003	0.112*	0.025	0.028	0.033	0.040	−0.034
	(0.041)	(0.103)	(0.054)	(0.058)	(0.034)	(0.033)	(0.199)	(0.046)	(0.060)
B: *Family* treatment	0.104**	0.119	0.141**	0.022	0.293***	0.290***	0.391**	0.297***	0.216**
	(0.043)	(0.107)	(0.055)	(0.066)	(0.034)	(0.035)	(0.132)	(0.046)	(0.079)
Number	5,009	664	2,873	1,472	4,664	6,233	756	3,554	1,923
Control group mean	1.23	1.28	1.25	1.18	0.89	0.89	0.85	0.89	0.92
p-value for joint test of A, B	0.05	0.52	0.03	0.14	0.00	0.00	0.00	0.00	0.00
p-value for test of A<B	0.08	0.30	0.01	0.10	0.00	0.00	0.03	0.00	0.00

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Robust standard errors in parentheses, clustered at the school level. All regressions 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. Outcomes are student or parent scores on DVD knowledge questions (scale of 0 to 5) asked at follow-up. Sample in column 5 includes only parents who returned surveys to schools at baseline; sample in columns 6 to 9 is entire baseline sample. Excluded variables used in the Heckman selection equations in columns 6-9 are indicators for the number of randomly assigned repeat visits to each school (two or three, relative to omitted category = 1 visit). These instruments are always jointly significant in the selection equation. The Mills ratio (indicating selection effects) is never significantly different than zero.

vioral impacts. Unfortunately, our survey design does not allow us to detect small differences (smaller than 0.1 standard deviations differences) in the two treatment effects. But since we do have sufficient power to reject *Family* treatment effects on absenteeism and enrollment that are more than 0.15 standard deviation larger than the impacts of *Student* treatment (see the MDD in table 3), we can conclude that exposure to the DVD at home does not substantially magnify the impact of *Abre la Caja* relative to watching the DVD in class.[22]

Given that the two treatments have similar effects on important educational outcomes, it is relevant to consider which method of delivering financial aid information to students was cheaper. We computed two measures of cost for each treatment: one based on the implementation costs of our intervention and another based on assumptions about how a national, government-led scale-up of the program would work. The cost estimates differ under these two scenarios largely because of how the fixed DVD production cost is spread out over more students and because of more flexibility in dissemination procedures under a national scale-up. In our evaluation, the per student cost of the *Student* treatment was $13.103 and this would fall to $0.26 assuming a full scale-up. The per student cost of the *Family* treatment in our experiment was $11.20 and would also fall substantially, to $1.50, in a national scale-up. Hence, for very similar impacts on school attendance and high school choice, the *Family* treatment was slightly cheaper than the *Student* treatment in the evaluation. At scale, however, delivering financial aid information to students, as we did using the *Student* treatment, would be significantly cheaper.

In table 4, we investigate whether the reason for no large differences between *Family* and *Student* treatments is that parents in both groups learn about the information from the DVD or because parents in the *Family* treatment learn nothing. The former might occur if students watching the DVD at school discuss the information with their parents; the latter might occur if parents in the *Family* group do not watch the DVD or do not find the information compelling. To measure how much students and their parents remembered about financial aid programs from the DVD, we asked everyone five questions about the DVD content.[23] Table 4 presents the results of regressions of the individual score on this DVD knowledge test on indicators for *Student* and *Family* treatments: columns 1 to 4 present student scores from OLS regressions and columns 5 to 9 present parent scores from Heckman selection corrected regressions that adjust for parental survey nonresponse.[24]

Focusing on columns 1 and 5 (full sample of students and parents), we see that assignment to the *Family* treatment raised the knowledge test scores by 0.1 points for students (significant at 5% level) and by a larger 0.29 points for parents (significant at 1% level). Each of these coefficients is statistically significantly different from the estimate of the impact of the *Student* treatment on the knowledge test. Students who watched the DVD in class score 0.03 points higher on the test, while parents in the *Student*

[22] In addition, we find no evidence that either treatment encouraged parents to spend more time with their children or to interact more with their children's school (results not shown).

[23] The questions were: (a) How many students do you think receive state grants or loans to continue studying? (b) What is the minimum PSU score you need for college scholarships? (c) What is the minimum grade you need to apply to a vocational training loan? (d) Is the PSU free for municipal or publicly subsidized school students? (e) How do government scholarships work? All questions had multiple-choice answers.

[24] Under a standard monotonicity assumption on the process governing nonresponse 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). As a check, we compute Heckman selection–corrected regression results for the same parent test score outcomes. The exclusion restrictions are two indicators for whether the school was visited two or three times for survey retrieval; the number of visits was randomly allocated to schools. Results from the first-stage selection equation are not reported, but the number of visits significantly predicts higher response rates, and the selection correction term is never statistically significant. This means that whatever selection there was driving parental nonresponse, it did not differ across treatment and control groups.

TABLE 5.—HETEROGENEITY IN EFFECTS OF EXPOSURE TO *ABRE LA CAJA* ON EFFORT IN SCHOOL AND EDUCATIONAL OUTCOMES: OLS

	Enrollment in College-Oriented High School				Absenteeism				Test Scores	
	Sample in Primary Schools without Continuing Grades 9–12		Sample in Primary Schools with Continuing Grades 9–12		Absent in September?		Number of Days Absent in September		Grades at End of Grade 8	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any exposure to *Abre la Caja*	0.063*		−0.082		−0.088**		−0.253		−0.235	
	(0.033)		(0.092)		(0.036)		(0.162)		(0.320)	
Any Exposure × Low Grade		0.037		−0.081		−0.067		−0.143		0.169
		(0.041)		(0.114)		(0.044)		(0.277)		(0.503)
Any Exposure × Medium Grade		0.082**		−0.072		−0.113***		−0.372***		0.102
		(0.037)		(0.088)		(0.040)		(0.181)		(0.290)
Any Exposure × High Grade		0.0404		−0.110		−0.082		−0.424***		0.115
		(0.052)		(0.121)		(0.056)		(0.177)		(0.406)
Medium grade		−0.0299		0.040		−0.029		−0.512**		5.411***
		(0.027)		(0.044)		(0.030)		(0.190)		(0.317)
High grade		0.0306		0.061		−0.183**		−1.179***		12.988***
		(0.040)		(0.066)		(0.044)		(0.216)		(0.356)
Number	4,191	4,191	1,462	1,462	3,615	3,615	3,615	3,615	6,181	6,181
Mean outcome for control group	0.60	0.60	0.75	0.75	0.64	0.64	2.10	2.10	53.69	53.69
p-value: Any × Low = Any × Med		0.23		0.91		0.26		0.41		0.90
p-value: Any × Low = Any × High		0.43		0.63		0.53		0.80		0.98
p-value: Any × Low = Any × Low		0.95		0.77		0.81		0.33		0.93

***$p < .01$, **$p < 0.05$, *$p < 0.1$. Robust standard errors in parentheses, clustered at the school level. SIMCE stratum fixed effects included in all regressions. The table presents OLS coefficients on an indicator for *Abre la Caja* exposure and (in even-numbered columns) indicators for whether baseline grades were medium, low, or high and interactions of grade group with treatment assignment, and a missing grade indicator. In the first two columns, the sample is restricted to students in primary schools that terminate in grade 8, and the third and fourth columns are restricted to students enrolled in primary schools that continue with grades 9 to 12. Enrollment and grade data are from MINEDUC administrative data; absenteeism is from school administrative records. *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.

treatment score only 0.02 points higher. Neither of these effects is statistically significant. Moreover, the pattern of coefficients for parent scores is similar across all grade groups: parents in the *Family* group consistently score higher on the knowledge test than parents in the *Student* group. This is evidence against the hypothesis that the financial aid information diffused to parents in the *Student* group despite their lack of access to the program, as well as evidence against the hypothesis that parents in the *Family* treatment group learned nothing.

A second important result from table 4 is that students score differently on the DVD knowledge test depending on their baseline grades. For high-grade students, both *Student* and *Family* treatment assignment raises the score on this test (differences between treatments are not significant); for medium-grade students, *Family* treatment assignment appears to have a significantly larger impact on score, while for the low-grade students, exposure to the DVD in class has the largest impact on scores. Some of these differences between medium- and low-grade students arise because lower-grade students were less likely to watch the DVD at home at all, but part of the impact on scores reflects the fact that different information was relevant for different types of children. In the next section, we investigate this heterogeneity with regard to education outcomes and future education expectations.

B. Heterogeneity with Respect to Baseline Grades

The previous section showed small and statistically indistinguishable differences in the impacts of exposure to the DVD at school and at home on attendance and high school enrollment. We now pool the two treatments and examine the impact of *Any Exposure* on outcomes using equations (3) and (4). Table 5 presents results for the type of high school enrolled in, separately for students attending terminal primary schools (columns 1 and 2) and for those with continuing high schools (columns 3 and 4); for absenteeism on the extensive (columns 5 and 6) and the intensive (columns 7 and 8) margins; and for scores at the end of grade 8 (columns 9 and 10).

The results in odd-numbered columns largely echo the differences in table 3. For students who are required to make a decision about enrolling in a new school for grade 9, there is a 10% higher chance of enrolling in a college preparatory high school (6.3 percentage points higher enrollment, *p*-value of 0.058) if they are exposed to treatment relative to the control group. There are no similar effects for the set of students who do not have to choose a different school for grade 9.[25] Absenteeism prevalence among students with any exposure falls by a significant 8.8 percentage points on average (*p*-value 0.016), while absenteeism on the extensive margin falls by 0.25 days (insignificant) and there is no large impact on test scores at the end of grade 8.

[25] We do not read much into the negative coefficient on the *Any Exposure variable* for this subset because within this group of children, only 12 (out of 1,462) switched schools at the end of grade 8, meaning there are very few observations identifying the coefficient. Most of the students in this sample (70%) are already in schools that provide scientific-humanistic education—preparation that will allow further study. This fact suggests that a fair amount of school choice might occur earlier, on entry to primary school.

TABLE 6.—HETEROGENEITY IN EFFECTS OF EXPOSURE TO *ABRE LA CAJA* ON EDUCATION EXPECTATIONS AND FINANCIAL AID PLANS AT FOLLOW-UP: OLS

	Do You Think You Will Study:						Expected Source of Postsecondary Finance Is:			
	After High School?		At College?		At a Vocational School?		Scholarship Finance		Loan Finance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any exposure to *Abre la Caja*	0.011		0.022		0.003		0.026		0.046***	
	(0.016)		(0.020)		(0.020)		(0.020)		(0.012)	
Any Exposure × Low Grade		0.013		−0.026		0.087**		0.032		0.042**
		(0.028)		(0.032)		(0.035)		(0.028)		(0.020)
Any exposure × Medium Grade		0.022		0.038		−0.026		0.020		0.042***
		(0.020)		(0.026)		(0.026)		(0.024)		(0.016)
Any exposure × High Grade		0.002		0.0768*		−0.049		0.094**		0.077**
		(0.030)		(0.045)		(0.045)		(0.044)		(0.034)
Medium grade		0.099***		0.058**		−0.006		0.119***		0.021
		(0.024)		(0.025)		(0.029)		(0.024)		(0.015)
High grade		0.240***		0.206***		−0.084**		0.391***		0.081***
		(0.033)		(0.042)		(0.038)		(0.038)		(0.025)
Number	4,918	4,918	3,301	3,301	3,301	3,301	3,372	3,372	3,372	3,372
Control group mean	0.68	0.68	0.36	0.36	0.42	0.42	0.32	0.32	0.10	0.10
p-value: Any × Low = Any × Med		0.76		0.08		0.01		0.73		1.00
p-value: Any × Low = Any × High		0.59		0.44		0.66		0.12		0.35
p-value: Any × Low = Any × Low		0.80		0.06		0.02		0.24		0.37

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. 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. The 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 as well as a missing grade 7 score indicator. All outcomes are binary, and sources of financial support outcomes are not mutually exclusive categories.

Examining coefficients on the grade interaction terms reveals an important pattern: the largest enrollment and extensive margin absenteeism effects are found for students with medium grades at baseline. Students with medium grades are 13.6% more likely to enroll in a college-oriented high school if exposed to the DVD at all relative to the control group. They are 17.6% less likely to be absent relative to the control group, and they significantly reduce the number of days absent in the last month by 0.3 days, or 17.7%. High-grade students exposed to the treatment also reduce absenteeism by a statistically significant 0.4 days.

Although we cannot reject that the coefficients on the interaction effects are statistically the same in magnitude, this pattern of coefficients suggests that students whose behaviors drive the impact estimates are those we might consider most marginal for the purposes of financial aid, that is, the students for whom financial aid eligibility is uncertain but for whom access to college and aid eligibility is most feasible—those with medium and higher grades. These are also the students we might expect to benefit the most from attending better high schools.

In table 6, we investigate whether exposure to *Abre la Caja* changed educational expectations or future financial plans for any of these students. We present results from estimating equations (3) and (4) for binary outcome variables: whether the student reports wanting to study beyond high school; conditional on wanting to study, whether they plan to study at college or a vocational school; and whether their expected source of financing will be scholarship or loan finance (multiple mentions were possible).

Given that three-quarters of students report wanting to continue with further studies at baseline (table 2), it is not surprising that *Abre le Caja* did not raise overall educational expectations of any postsecondary education by a significant amount (columns 1 and 2). There is little room for an adjustment of expectations upward and, importantly, no evidence of a discouragement effect among low-grade students. However, within grade groups, the new information may have shifted the types of schooling desired. Columns 3 and 4 show that students with higher grades are 7.7 percentage points more likely to report they will study at college compared to students with low grades, and students with low grades are 8.7 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. They suggest that *Abre la Caja* may have shifted the type of higher education that students with different abilities considered feasible for themselves.

The final results in columns 7 to 10 reinforce the idea that students retained information from the DVD that was directly relevant to them. More of the high-scoring students report wanting to use scholarships (9.4 percentage points) and loans for postsecondary schooling finance (7.7 percentage points), but there is also a significant increase in the fraction of students with medium and low grades (4 percentage points) reporting that they want to use loan finance.

Summing up, this heterogeneity in responses to *Any Exposure* reveals that all students learned something from the intervention (table 4), that students with different baseline grades retained information that was relevant to them and shifted expectations in line with what *Abre la Caja* suggested was feasible (table 6), but that behavioral responses were driven by those most marginal for the specific intervention: students earning medium and high grades in grade 7 (table 5).

VI. Conclusion

Despite the proliferation of financial aid programs for higher education in countries around the world, attainment of higher education is often highly correlated with the socioeconomic status of families. This is certainly the case in Chile, where massive improvements in general education and recent expansions of loan and scholarship programs have not been sufficient to reduce the inequality of access to postsecondary schooling for young adults from poor backgrounds. One reason for this is that such students may be academically unprepared to qualify for available financial aid at the time when they might apply for it.

In this paper, we investigated whether providing direct information about such loan and scholarship opportunities four years before the application process begins can improve educational behaviors among eighth graders in metropolitan Santiago. Our findings suggest that the answer is yes, regardless of whether the information is delivered to children at school or to children and their parents at home. And far from this information having discouragement effects on students, it seems that the students who respond the most to the new information are those for whom the DVD was most relevant: students with medium and high grades at baseline.

Although our survey respondents are still too young to apply for college or vocational training schools, these short-term impacts on their behaviors are important. The results of our experiment suggest that a small amount of relevant information provided at the right time can induce students to provide more effort in school and make different choices about high school enrollment. For policymakers trying to target more active school choices, providing information about financial aid for higher education may be an important addition to providing information about high school quality. The costs of providing this information are relatively low (especially at a national level), the information itself is standardized, and the intervention is easy to scale, regardless of delivery mode.

Our results add to a growing body of evidence showing how different types of imperfect information can lead to underinvestment in human capital (Nguyen, 2008; Jensen, 2010; Bettinger et al., 2012) and highlight the importance of considering how students of different abilities respond in different ways to the new information. Together these studies suggest caution in extrapolating from policy experiments conducted across different country settings for the purposes of cost-effectiveness comparisons: different information constraints are likely to be binding in different contexts and will matter for different types of educational outcomes.

Finally, our comparisons between the *Student* and *Family* interventions are provocative since they suggest that parental involvement does not substantially magnify the behavioral impacts of this information intervention. However, since we cannot reject small differences in the effects of these two ways of delivering information, our experiment cannot be definitive on the role of parents in human capital investment decisions. Certainly our results suggest that some children were able to retain and respond to the relevant information regardless of what their parents learned. However, in order to understand why parental involvement did not substantially magnify the impacts of the DVD, we would need to know a lot more about how enrollment and school attendance decisions are made within the household. This presents a promising avenue for future research into when adolescents become their own agents in the schooling investment decision.

REFERENCES

Andrabi, Tahir, Jishnu Das, and Asim Ijaz Khwaja, "Report Cards: The Impact of Providing School and Child Test Scores on Educational Markets," Harvard Kennedy School working paper (2010).

Angrist, Joshua, "The Effects of Veterans' Benefits on Education and Earnings," *Industrial and Labor Relations Review* 46:4 (1993), 637–652.

Angrist, Joshua, and Alan Krueger, "Empirical Strategies in Labor Economics" (pp. 1277–1366), in (Orley Ashenfelter and David Card (eds.), *Handbook of Labor Economics* (New York: Elsevier 1999).

Attanasio Orazio, and Katja Kaufmann, "Educational Choices, Subjective Expectations and Credit Constraints," *NBER working paper* 15087 (2009).

Avvisati, Francesco, Marc Gurgand, Nina Guyon, and Eric Maurin, "Getting Parents Involved: A Field Experiment in Deprived Schools," CEPR discussion paper 8020 (2010).

Baird, Sarah, Joan Hamory Hicks, Michael Kremer, and Edward Miguel, "Worms at Work: Long Run Impacts of Child Health Gains," University of California Berkeley working paper (2011).

Berthelon, Matias, and Diana Kruger, "Risky Behavior among Youth: Incapacitation Effects of School on Adolescent Motherhood and Crime in Chile," *Journal of Public Economics* 95 (2011), 41–53.

Berry, James, "Child Control in Educational Decisions: An Evaluation of Targeted Incentives to Learn in India," mimeograph, MIT Department of Economics (2009).

Bettinger, Eric, Bridget Long, Philip Oreopolous, and Lisa Sanbonmatsu, "The Role of Simplification and Information in College Decisions: Results from the H&R Block FAFSA Experiment," *Quarterly Journal of Economics* 127 (2012), 1205–1242.

Bobonis, Gustavo, Edward Miguel, and Charu Puri-Sharma, "Anaemia and School Participation," *Journal of Human Resources* 41 (2006), 692–721.

Bound, John, and Sarah Turner, "Going to War and Going to College: Did World War II and the G.I. Bill Increase Educational Attainment for Returning Veterans?" *Journal of Labor Economics* 4 (2002), 784–815.

Bravo, David, Sankar Mukhopadhyay, and Petra Todd, "Effects of School Reform on Education and Labor Market Performance: Evidence from Chile's Universal Voucher System," *Quantitative Economics* 1:1 (2010), 47–95.

Burztyn, Leonardo, and Lucas Coffman, "The Schooling Decision: Family Preferences, Intergenerational Conflict, and Moral Hazard in the Brazilian *Favelas*," *Journal of Political Economy* 120 (2012), 359–397.

Carneiro, Pedro, and James Heckman, "The Evidence on Credit Constraints in Post-Secondary Schooling," *Economic Journal* 112 (2002), 705–734.

Comisión de Financiamiento Estudiantil, "Análisis y Recomendaciones para el Sistema de Financiamiento Estudiantil," mimeographed, Ministerio de Educación, Chile (2012).

De Fraja, Gianni, Tania Oliveira, and Luisa Zanchi, "Must Try Harder: Evaluating the Role of Effort in Educational Attainment," this REVIEW 92: 3 (2010), 577–597.

Dinardo, John, Justin McCrary, and Lisa Sanbonmatsu, "Constructive Proposals for Dealing with Attrition in Surveys: An Empirical Example," mimeograph, University of Michigan (2006).

Dinkelman, Taryn, and Claudia Martínez A., "Investing in Schooling in Chile: The Role of Information about Financial Aid for Higher Education," *BREAD working paper* 301 (2011).

Dobbie, Will, and Roland Fryer, "Are High-Quality Schools Enough to Increase Achievement among the Poor? Evidence from the Harlem Children's Zone," *American Economic Journal: Applied Economics* 3 (2011), 158–187.

Dynarski, Susan, "The Behavioral and Distributional Implications of Aid for College," *American Economic Review* 92 (2002), 279–285.

——— "Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion," *American Economic Review* 93 (2003), 279–288.

Giustinelli, Pamela, "Uncertain Outcomes and Child-Parent Decision Making in Curriculum Choice: What Data Do We Need to Tell Them Apart?" mimeograph, Survey Research Center University of Michigan (2010).

Gruber, Jonathan, *Risky Behavior among Youths: An Economic Analysis* (Chicago: University of Chicago Press, 2001).

Hastings, Justine, Richard Van Weelden, and Jeffrey Weinstein, "Preferences, Information, and Parental Choice Behavior in Public School Choice," *NBER working paper* 12995 (2007).

Hastings, Justine, and Jeffrey Weinstein, "Information, School Choice, and Academic Achievement: Evidence from Two Experiments," *Quarterly Journal of Economics* 123 (2008), 1373–1414.

Horvitz, D. G., and D. J. Thompson, "A Generalization of Sampling without Replacement from a Finite Universe," *Journal of the American Statistical Association* 47 (1952), 663–685.

Hsieh, Chang-Tai, and Miguel Urquiola, "The Effects of Generalized School Choice on Achievement and Stratification: Evidence from Chile's Voucher Program," *Journal of Public Economics* 90 (2006), 1477–1503.

Jensen, Robert, "The Perceived Returns to Education and the Demand for Schooling," *Quarterly Journal of Economics* 125 (2010), 515–548.

Kaufmann, Katja Maria, "Understanding the Income Gradient in College Attendance in Mexico: The Role of Heterogeneity in Expected Returns," Stanford Institute for Economic Policy Research discussion paper 07-040 (2008).

Kremer, Michael, and Edward Miguel, "Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities," *Econometrica* 72 (2004), 159–217.

Lee, David, "Training, Wages and Sample Selection: Estimating Sharp Bounds on Treatment Effects," *Review of Economic Studies* 76 (2009), 1071–1102.

Marcucci, Pamela, and Bruce Johnstone, "Targeting Financial Assistance to Students in Higher Education: Means-Testing with Special Emphasis on Low- and Middle-Income countries," World Bank working document (2010), http://siteresources.worldbank.org/EDUCATION/Resources/278200-1099079877269/547664-1099079956815/547670-1276537814548/Marcucci_Johnstone_Means_testing.pdf.

Meneses, Francisco, Rodrigo Rolando, Manuel Valenzuela, and Maria Antonieta Vega, "Ingreso a la Educación Superior: La Experiencia de la Cohorte de Egreso 2005," Ministerio de Educación policy document (2010), http://www.mineduc.cl/usuarios/sies/File/ESTUDIOS/ESTUDIOSSIES/Ingreso-a-la-Educaci%F3n-Superior3.pdf.

MIDEPLAN, "CASEN: Encuesta de caracterización socioeconómica nacional" (Santiago, Chile, 2009).

MINEDUC, "Report of the Committee of the Student Funding for Higher Education" (2012), http://www.mineduc.cl/usuarios/mineduc/doc/201203291032500.InformeComisionAyudaEstudiantilmarzo2012.pdf.

Mizala, Alejandra, and Miguel Urquiola, "School Markets: The Impact of Information Approximating Schools' Effectiveness," NBER working paper 13676 (2007).

Nguyen, Trang, "Information, Role Models and Perceived Returns to Education: Experimental Evidence from Madagascar," mimeograph, MIT Department of Economics (2008).

OECD, "Reviews of National Policies for Education: Tertiary Education in Chile" (2009).

Pop-Eleches, Cristian, and Miguel Urquiola, "Going to a Better School: Effects and Behavioral Responses," NBER working paper 16886 (2011).

Solis, Alex,"Credit Constraints for Higher Education," mimeograph, Agricultural Economics Department, University of California, Berkeley (2010).

SIMCE, Ministerio de Educación "Manual de uso de la Base de Datos SIMCE 2007 para 8°Básico" (Santiago, Chile: Ministerio de Educación, 2008).

Urquiola, Miguel, and Eric Verhoogen, "Class Size and Sorting in Market Equilibrium: Theory and Evidence," *American Economic Review*, 99 (2009), 179–215.