THE (PERCEIVED) RETURNS TO EDUCATION AND THE DEMAND FOR SCHOOLING*

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Economists emphasize the link between market returns to education and investments in schooling. Though many studies estimate these returns with earnings data, it is the *perceived* returns that affect schooling decisions, and these perceptions may be inaccurate. Using survey data for eighth-grade boys in the Dominican Republic, we find that the perceived returns to secondary school are extremely low, despite high measured returns. Students at randomly selected schools given information on the higher measured returns completed on average 0.20–0.35 more years of school over the next four years than those who were not.

I. INTRODUCTION

How important are the returns to education in determining schooling decisions? Do students have accurate information about these returns when they choose whether to continue schooling? Becker's canonical model of human capital views education as an investment, where costs are compared to the discounted stream of expected future benefits, primarily in the form of greater wages. However, although there is a large literature estimating the returns to schooling with earnings data, as pointed out by Manski (1993), it is the returns *perceived* by students and/or their parents that will influence actual schooling decisions. Given the great difficulties in estimating the returns encountered even by professional economists using large data sets and advanced econometric techniques, it seems likely that typical students make their schooling decisions on the basis of limited or imperfect information. In this setting, there is little reason to expect the level of education chosen to be either individually or socially efficient.

This possibility is particularly important to consider for developing countries, where educational attainment remains persistently low despite high measured returns. For example, in the Dominican Republic, although 80%–90% of youths complete

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(compulsory) primary schooling, only about 25%–30% complete secondary school (Oficina Nacional de Estadística, República Dominicana 2002). Yet the mean earnings of workers who complete secondary school are over 40% greater than those of workers who only complete primary school[1] (estimates are from a survey conducted by the author in 2001, explored in more detail below). There are of course many potential explanations for this puzzle, such as poverty and credit constraints, high discount rates, or simply mismeasured returns on the part of the researcher (i.e., education is low because the true returns are low). However, if underestimates of the returns and thus a low demand for schooling are limiting factors for at least some subset of individuals, simply providing information on the returns may be the most cost-effective strategy for increasing their education. In this paper, we conduct an experimental intervention among eighth-grade male students in the Dominican Republic to test this hypothesis.

A handful of studies for the United States have found that high school seniors and college students are relatively well informed of the returns to a college education (Smith and Powell 1990; Betts 1996; Dominitz and Manski 1996; Avery and Kane 2004; Rouse 2004).[2] However, there is reason to believe that students and/or their parents in low-income countries may not be as well informed about the returns. For example, the decision to drop out of school is often made at a much younger age, when students have less information about the returns. And schools typically do not have guidance counselors to provide information about the returns.[3] Further, in general there may just be little or no information available at all on earnings, because labor market data may not be collected as regularly or comprehensively by governments or private organizations, or because the results may not be as widely disseminated.[4] As a result, the only data on earnings available to youths may come from the individuals

1. Assuming a discount rate of 0.05, the net present value of expected lifetime earnings, including forgone wages and the direct costs of schooling, is over 15% greater with secondary schooling.

2. Despite these apparently accurate perceptions, there is evidence that adolescents in the United States, as well as the United Kingdom and Canada, in effect "drop out too soon," forgoing substantial monetary and nonmonetary returns, perhaps because they ignore or heavily discount these returns (Oreopoulos 2007).

3. Betts (1996), for example, finds that over 60% of the U. S. college seniors surveyed reported using their school's career services center to obtain information about job prospects by field of study.

4. For example, for this study we had to conduct our own labor force survey to estimate the returns in the Dominican Republic because no data were available at the time, nor were there any available published estimates of the returns.

they can observe around them,[5] which could lead to inaccuracies. For example, youths in rural communities or small towns where few or no adults have any education will have little information from which to infer the returns, including the potential returns in the urban sector. In addition, if students rely almost exclusively on the earnings of workers in their own communities in forming their expectations of earnings, residential segregation by income could lead to underestimates of the returns to schooling. Although all these factors make it unlikely that youths in low-income countries have accurate information on the returns to schooling, until recently there has been no evidence on the perceived returns for such countries; the present paper, alongside Attanasio and Kaufman (2008) and Kaufman (2008) for Mexico and Nguyen (2008) for Madagascar, has begun to fill this gap.

The possibility that decision makers may not be well informed has been explored in several other areas of economic behavior.[6] However, only a handful of studies have examined whether providing information in these settings can change behavior. Dupas (2009) finds that providing age-disaggregated information on HIV prevalence rates affects the incidence of risky sexual behavior among girls in Kenya. Duflo and Saez (2003) find that retirement plan decisions respond to being given incentives to attend a session providing benefits information, and Hastings and Weinstein (2008) find that providing parents with simpler, more transparent and relevant information such as average test scores and admissions probabilities can affect school choice. Finally, applying a strategy similar to that used in the present paper,[7] Nguyen (2008) finds that providing parents in Madagascar with information on the returns to schooling improves their children's school performance and attendance in the first few months following the

5. For example, over 70% of students in our survey reported that their main source of information about earnings was the people they knew in their community. By contrast, Betts (1996) reports that the most widely used source of information on employment prospects among college students was newspapers and magazines.

6. For example, many studies find that individuals underestimate the costs of borrowing (see Stango and Zinman [2007] for examples) or are poorly informed of their own pension or social security benefits (Mitchell 1988; Gustman and Steinmeier 2005; Chan and Stevens 2008). Viscusi (1990) finds that individuals overstate the risks of lung cancer from smoking, and that these misperceptions actually reduce smoking behavior. McKenzie, Gibson, and Stillman (2007) find that potential emigrants underestimate the returns to migration.

7. Nguyen's paper extends the approach by considering the potential value of role models instead of, or in addition to, simply providing information on the returns, as in the present study (though she ultimately concludes that information alone appears in general to be the most effective strategy).

intervention. An advantage of the present study is that we follow students over a four-year period, so we can assess the long-term impact of this kind of intervention.

Using data from a panel survey of boys in the Dominican Republic in the eighth grade, the last year of compulsory schooling and the point at which most students terminate their education, we find that perceptions of the returns to secondary schooling are extremely low for most students, especially relative to returns measured with earnings data. Although many factors may affect or limit school attendance, such as poverty and credit constraints, these results raise the possibility that for at least some youths, school dropout may be the result of low demand due to low perceived returns. Thus, students at a randomly selected subset of schools were provided information on the returns estimated from earnings data. Relative to those not provided with information, these students reported dramatically increased perceived returns when re-interviewed four to six months later, and on average completed 0.20 more years of schooling over the next four years. And, consistent with the hypothesis that poverty and credit constraints limit schooling even when there is demand, we find that the program had a large effect among the least poor students, increasing schooling by 0.33 years, but no effect for the poorest students, despite the fact that both groups increased perceived returns by the same amount.

The remainder of this paper proceeds as follows: Section II discusses the data and experimental design and explores both the accuracy of student perceptions and whether measured perceptions predict actual schooling. Section III presents the results of the experiment, and Section IV discusses the policy implications and concludes.

II. Data and Methodology

II.A. Data

To estimate the returns to education, we conducted a household-based income survey in January 2001.[8] The survey of 1,500 households was conducted nationwide, but only in nonrural areas (comprising about two-thirds of the population) because of the greater difficulty in estimating earnings for agricultural

8. At the time the study began, there were no publicly available microdata on income.

households. The household sample was drawn in two stages. First, from the thirty largest cities and towns, we chose 150 sampling clusters at random,[9] with the number of clusters chosen in each town approximately proportional to that town's share of the combined population of the thirty cities/towns.[10] A listing of all dwellings in the cluster was then made, and twenty households were drawn at random from each cluster. The questionnaire gathered information on education, employment and earnings, and background demographic and socioeconomic characteristics for all adult household members. We will discuss the estimated returns to schooling using these data in more detail in Section II.D and the Online Appendix; for now, we note that the mean monthly earnings (including both workers and nonworkers)[11] among men thirty to forty years old (the group whose earnings will form the basis of our experiment) expressed in nominal 2001 Dominican pesos (RD$; RD$1 $\approx$ 0.06US$ in January 2001) are RD$4,479 for those who completed secondary school (only) and RD$3,180 for those who completed primary school (only). The RD$1,299 difference represents an approximately 41% overall return, or about 8% per additional year of schooling (provided there is no "sheepskin effect" or discrete jump at year twelve).

For the student survey, for each of the 150 household sample clusters, we selected the school where students from that cluster attend eighth grade.[12] From each school, during April and May of 2001, we interviewed fifteen randomly selected boys[13] enrolled in eighth grade, the final year of primary school and therefore the point right before the very large declines in enrollment.[14] All

9. Cities and towns were divided into a set of clusters with the help of community leaders and government officials.

10. For greater geographic variation, we undersampled the capital, Santo Domingo. The city contains roughly 45% of the total population of the thirty cities/towns but is only about 25% of our sample.

11. About 8%–10% of both groups (slightly higher for the primary school group) reported no earnings in the past month. However, the earnings gap by education is not substantially different if we focus on employed workers.

12. In six cases, two clusters primarily used the same school; for these cases, we also chose the nearest alternate school.

13. We did not interview girls because of difficulties in eliciting expected earnings. Due to a low female labor force participation rate in the Dominican Republic (about 40%), in focus groups most girls were unwilling to estimate their expected earnings because they felt they would never work.

14. Students were randomly selected from lists of currently enrolled students and interviewed individually at the school. If a student was not present on the day of the interview, enumerators returned to the school the following day, and then contacted the student at home if he was still not available. Fifty-eight students were interviewed in their homes, primarily due to extended illness. Students were not compensated for their participation.

2,250 students in the study were administered a survey gathering information on a variety of individual and household characteristics, as well as some simple questions on expected earnings by education (discussed below).

A second survey of the students was conducted after the beginning of the next academic year (October 2001), with respondents interviewed again (at home, school, or work) about perceived returns to education and current enrollment status. In addition, at this time parents were also interviewed to gather additional information on socioeconomic status, including household income. A third-round follow-up survey on schooling was also conducted in May and June of 2005, by which time students should have been finishing their last years of secondary school; for the approximately 120 students who were still enrolled in 2005 but not yet through their final years of school (due primarily to grade repetition), we conducted follow-ups for each of the next two years. For all follow-up surveys, if the respondents could not be found after two attempts, their parents, siblings, or other relatives were interviewed about the youths' enrollment status. If these relatives also could not be located, neighbors were interviewed about the youths. Overall, we were able to obtain follow-up information in the October 2001 survey directly from 93% of youths, with 2% from relatives and 5% from neighbors. By the 2005 survey, this had changed to 89% from youths, 4% from relatives, and 7% from neighbors. In all cases, we attempted to verify educational attainment by contacting the school that students were reported to be attending or had attended. We were able to do so for 97% of students in the second-round survey and 91% in the third round.

Quantifying perceptions of the returns to education is difficult, especially with young respondents (valuable summaries of methods for eliciting expectations for a range of outcomes can be found in Manski [2004] and Delavande, Giné, and McKenzie [2008], the latter focusing more on approaches applied in low-income countries). Therefore, the survey asked only some simple questions about perceived earnings, based on Dominitz and Manski (1996), though much more limited. In particular, students were asked to estimate what they expected they might earn under three alternative education scenarios:

> Suppose, hypothetically, you were to complete [this school year/secondary school/university], and then stop attending school. Think about the kinds of

> jobs you might be offered and that you might accept. How much do you think you will earn in a typical week, month or year when you are about 30 to 40 years old?

Students were also asked to estimate the earnings of current thirty- to forty-year-old workers with different levels of education:

> Now, we would like you to think about adult men who are about 30 to 40 years old and who have completed only [primary school/secondary school/university]. Think not just about the ones you know personally, but all men like this throughout the country. How much do you think they earn in a typical week, month or year?

Although own expected earnings are likely to be the relevant criterion for decision making, this second set of questions was included to measure perceptions of earnings that are purged of any beliefs students may have about themselves, their households, or their communities, such as the quality of their school or their own ability, or beliefs about factors such as race in determining earnings. The two sets of questions can therefore be used to determine in part whether students' perceptions differ from measured returns because (a) they have poor or inaccurate information on prevailing wages in the labor market (as captured by the second set of questions), or (b) they have information or beliefs about themselves (correct or incorrect) that influence what they expect earnings will be for them personally (as captured by the first set of questions). For example, even if some students believe (perhaps correctly) that they would not gain from education because of labor market discrimination based on race or because they attend a low-quality school, these beliefs about their personal returns should not be reflected in their perceptions of the average earnings of other workers.

These simple questions have several obvious and significant limitations.[15] First, they are not precise in specifying the meaning of "expected" earnings, such as referring to the mean, median, or mode.[16] In addition, they do not elicit perceived uncertainty

15. Our approach to eliciting expectations is similar to that of Nguyen (2008), but differs from those of Attanasio and Kaufmann (2008) and Kaufmann (2008). The latter two instead ask what individuals expect is the maximum and minimum they might earn under different education scenarios, as well as the probability of earning more than the midpoint of these two. With an assumption on the distribution of expectations, these data can be used to estimate various moments of the distribution.

16. Though even if these more precise definitions could have been elicited, it is unclear which quantity students actually use in decision making. The wording

(unlike, for example, Dominitz and Manski [1996], Attanasio and Kaufmann [2008], and Kaufmann [2008]) or the lifetime profile of earnings, nor do they address expectations of inflation.[17] Finally, the questions deal with abstract, hypothetical situations, are stated in fairly formal language, and are slightly lengthy and complicated; as a result, about 10% of students did not provide responses to these questions, or responded "don't know." Given the ages of the students and their degree of math literacy, these various limitations could not be overcome in our field testing.[18] Thus, we do not view these as perfect measures of youths' actual decision-making criteria, nor will we rely on them for our primary analysis (the impact of the information intervention on schooling outcomes). We present these data simply as a way of quantifying as well as possible the impressions of low perceived returns revealed in prestudy focus group discussions and to provide motivation for the intervention.

II.B. The Intervention

At the end of the student survey, each respondent at a randomly selected subset of schools was given information on earnings by education from the household survey and the absolute and percent return implied by those values, as reported above:

> Before we end, I would like to provide you with some information from our study. In January, we interviewed adults living in this community and all

was intended to elicit as well as possible the level of earnings students expect or associate with different levels of schooling. Delavande, Giné, and McKenzie (2008) discuss the weaknesses of this approach relative to more sophisticated strategies that for example elicit information on the distribution of expected earnings.

17. We are thus implicitly assuming that students are not taking inflation into consideration when providing expected future earnings. In focus groups during survey design, students did not reveal any awareness of the possibility of inflation. Further, the high correspondence between students' expected future earnings and their perceptions of current earnings (shown below) is consistent with ignoring inflation (though we can't rule out that they are considering inflation but also expecting other factors to lower the returns to schooling). Finally, we note that if students' responses incorporated expected inflation, this would lead them to report *greater* absolute differences in earnings by education than what they feel prevails at present; thus, inflation expectations could not account for students underestimating the returns to schooling, and would in fact lead to the opposite outcome (though it would of course leave the percent returns unchanged).

18. More recently, a number of studies have made progress on this problem by using visual or physical methods for eliciting expectations, such as asking respondents to assign a fixed number of objects (e.g., stones or beans) to a number of bins or categories representing different outcomes, with more objects to be allocated for outcomes perceived to be more likely. See Delavande, Giné, and McKenzie (2008) for a review.

over the country. We asked them about many things, including their earnings and education. We found that the average earnings of a man 30 to 40 years old with only a primary school education was about 3,200 pesos per month. And the average income of a man the same age who completed secondary school, but did not attend university, was about 4,500 pesos per month. So the difference between workers with and without secondary school is about 1,300 pesos per month; workers who finish secondary school earn about 41 percent more than those who don't. And people who go to university earn about 5,900 pesos per month, which is about 85 percent more than those who only finish primary school.

Although the statement is again perhaps a bit lengthy, formal, and complicated, the training of enumerators stressed that it was essential to emphasize the key elements of the statement, namely the earnings levels by education and the difference between them, by repeating them a second time after the statement was read to make sure students understood the findings (students were then also invited to ask any questions about the data and results that they might have).

We chose to provide the simple difference in mean earnings by education rather than estimates adjusted for other controls or using instrumental variables. As shown in Section II.D, these other approaches yield similar estimates of the returns, which are also broadly comparable to those found in other studies of the Dominican Republic and similar countries. Therefore, we chose to provide the information that would be easiest for students to understand.

Randomization was conducted blindly by the author, with each school having an equal likelihood of selection into the treatment and control groups. Compliance with randomization was ensured by providing enumerators with treatment-specific questionnaires (i.e., the questionnaires provided to enumerators visiting treatment schools included the paragraph above, and those provided to enumerators visiting control schools did not) and random auditing through visits during the survey process. Assignment of the treatment was done at the school level rather than for individual students within schools because students in the same school are likely to communicate, which would contaminate the control group. We cannot rule out that communication across schools occurred, though to the extent that such contamination took place, the true effect of the treatment would likely be even greater than what we estimate.

Table I provides means and standard deviations for key variables from the baseline survey (plus income, which was measured in Round 2); all estimates in this and subsequent tables are weighted to be representative of the thirty largest cities and towns. The average eighth-grade youth in our sample is just over 14 years old in the baseline survey and performs about average at school (as reported by teachers on a scale of 1 to 5—1. Much worse than average; 2. Worse than average; 3. Average; 4. Above average; 5. Much better than average). The average household income is approximately RD$3,500 per month, and 38% of youths have fathers who finished high school. At baseline, students expect earnings at age 30–40 of RD$3,516 if they only finish primary school and RD$3,845 if they finish secondary school, both of which are slightly greater on average than what they believe current workers aged 30–40 with those levels of education earn.

Table I also shows that there were no systematic differences in these baseline covariates for treatment and control groups. The differences are all small (less than 3% in all cases), and none are statistically significant. Thus, the randomization appears to have been successful in creating comparable samples of students in the treatment and control groups with respect to observable characteristics.

II.C. Do Expectations Predict Schooling?

Despite the limitations of the measures of expected earnings noted above, it is worth exploring whether they predict schooling. The first columns of Table II show regressions where the dependent variables are three measures of educational outcomes: whether the child returned to school for the academic year following the Round 1 survey (i.e., entered secondary school), whether he finished high school, and years of schooling (the latter two are measured as of Round 3, four years later). The independent variable of interest is the baseline implied perceived returns to secondary schooling for Round 1 (expressed here in thousands of 2001 Dominican pesos, RD$1,000), constructed as the difference between own expected earnings with secondary (only) and own expected earnings with primary (only) at age 30–40. For now, we use only the control group, because the information given later as part of the experimental intervention may cause students to update their expectations, weakening the link between baseline perceptions and eventual schooling.

TABLE I
MEANS, STANDARD DEVIATIONS, AND TEST OF TREATMENT–CONTROL COVARIATE BALANCE

	All	Control	Treatment	Difference
Age	14.3	14.3	14.4	0.02
	[0.79]	[0.79]	[0.79]	(0.04)
School performance	2.64	2.66	2.62	−0.04
	[1.45]	[1.46]	[1.45]	(0.06)
Father finished secondary	0.38	0.39	0.38	−0.01
	[0.49]	[0.49]	[0.49]	(0.05)
Log (income per capita)	8.16	8.17	8.15	−0.04
	[0.32]	[0.31]	[0.32]	(0.05)
Round 1 expected earnings (self)				
Primary (only)	3,516	3,548	3,484	−64
	[884]	(116)	(124)	(165)
Secondary (only)	3,845	3,884	3,806	−78
	[1,044]	(132)	(145)	(191)
Implied perceived returns (self)	329	336	322	−14
	[403]	(25)	(27)	(36)
Round 1 expected earnings (others)				
Primary (only)	3,478	3,509	3,447	−62
	[863]	(112)	(120)	(160)
Secondary (only)	3,765	3,802	3,728	−73
	[997]	(126)	(143)	(185)
Implied perceived returns (other)	287	293	281	−12
	[373]	(23)	(29)	(36)

Notes. Standard deviations in brackets in columns (1)–(3); heteroscedasticity-consistent standard errors accounting for clustering in parentheses in column (4). Data are from a survey of eighth-grade male students, conducted by the author. Data on age, school performance, and whether the father finished high school were gathered in Round 1 (April–May 2001); the number of observations is 1,125 for both treatment and control. School performance is teacher assessment of the student's performance, on a scale of 1 to 5 (much worse than average, worse than average, average, above average, much better than average). Implied perceived returns is the difference between own expected earnings at age 30–40 with primary and with secondary, measured in Round 1. Income per capita was gathered in Round 2 (October 2001), where there are 1,054 observations for the control group and 1,057 observations for the treatment group. All monetary figures are measured in 2001 Dominican pesos (RD$). Returned next year is measured in Round 2; finished school and years of schooling are measured in Round 3. "Difference" is a t-test of difference between treatment and control groups.

*Significant at 10%.

**Significant at 5%.

***Significant at 1%.

Overall, these baseline implied perceived returns do predict subsequent schooling. Regressions using only the implied perceived returns without additional controls (columns (1), (3), and (5)) show positive and statistically significant associations between perceptions and all three schooling outcomes. The point estimates decline considerably (25%–35%) but remain statistically significant even when we control for characteristics that may be correlated with both schooling and perceived returns,

TABLE II
IMPLIED PERCEIVED RETURNS AND SCHOOLING

	Panel A. Round 1 implied perceived returns (control group only)						Panel B. Round 2 implied perceived returns (full sample)					
										Instrumental variables		
	(1) Returned next year	(2) Returned next year	(3) Finished school	(4) Finished school	(5) Years of schooling	(6) Years of schooling	(7) Returned next year	(8) Finished school	(9) Years of schooling	(10) Returned next year	(11) Finished school	(12) Years of schooling
Implied perceived returns	0.11***	0.083**	0.14***	0.092**	0.53***	0.37**	0.095***	0.088***	0.37***	0.16**	0.096*	0.63***
	(0.030)	(0.034)	(0.036)	(0.038)	(0.13)	(0.14)	(0.21)	(0.019)	(0.075)	(0.071)	(0.055)	(0.22)
Log (inc. per capita)		0.090		0.25***		0.76***	0.044	0.18***	0.61***	0.023	0.18***	0.52***
		(0.062)		(0.063)		(0.24)	(0.045)	(0.048)	(0.17)	(0.049)	(0.051)	(0.17)
School performance		0.015		0.015		0.093**	0.014	0.021**	0.087**	0.013	0.021**	0.086**
		(0.014)		(0.011)		(0.045)	(0.010)	(0.008)	(0.034)	(0.010)	(0.008)	(0.034)
Father finished secondary		0.036		−0.014		0.045	0.067**	0.045	0.21*	0.066**	0.045	0.20*
		(0.041)		(0.044)		(0.16)	(0.032)	(0.029)	(0.12)	(0.032)	(0.029)	(0.12)
Age		−0.017		0.006		−0.045	−0.011	0.004	−0.006	−0.011	0.004	−0.003
		(0.024)		(0.025)		(0.093)	(0.019)	(0.016)	(0.066)	(0.019)	(0.016)	(0.067)
R^2	.008	.016	.017	.048	.016	.042	.027	.050	.053	.022	.050	.046
Observations	1,003	1,003	1,003	1,003	918	918	1,899	1,899	1,809	1,899	1,899	1,809

Notes. Heteroscedasticity-consistent standard errors accounting for clustering at the school level in parentheses. Data are from a survey of eighth-grade male students, conducted by the author. Returned next year is measured in Round 2; finished school and years of schooling are measured in Round 3. Implied perceived returns is the difference between own expected earnings at age 30–40 with primary and with secondary schooling, measured in thousands of 2001 Dominican pesos (RD$1,000). Columns (1)–(6) (Panel A) use Round 1 implied perceived returns as an independent variable and columns (7)–(12) (Panel B) use Round 2 implied perceived returns. Columns (1), (3), and (5) use no other control variables; all other columns add age, school performance, whether father finished secondary school, and log income per capita as additional controls. School performance is teacher assessment of the student's performance, on a scale of 1 to 5 (much worse than average, worse than average, average, above average, much better than average). Age, school performance, and whether father finished secondary were gathered in the Round 1; income was measured in Round 2. Regressions also include an indicator for whether income data were unavailable (these household are assigned the median sample income). In columns (10)–(12), implied perceived returns is instrumented using an indicator for having received the treatment.

*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

including the student's age and eighth-grade school performance, his household income,[19] and whether his father completed high school (income is measured in Round 2, and all other variables are measured at baseline). A RD$1,000 increase in implied perceived returns increases the likelihood of returning to school the next year by eight percentage points, the likelihood of completing high school by nine percentage points, and years of schooling by 0.37, with all coefficients significant at the 5% level. These results are consistent with Kaufmann (2008) and Attanasio and Kaufmann (2008), who find that measures of adolescents' perceived returns are also correlated with high school and college enrollment in Mexico.[20]

Of course, these regressions may be plagued by omitted variables bias (e.g., those with low perceived returns may attend lower-quality schools) or reverse causality (e.g., a "sour grapes" effect whereby those who want to go to school but are constrained from doing so by poor grades or low income report low returns). Although we would not want to attach a strong causal interpretation to these results, they provide an initial impression that measured perceptions do have some predictive value (though it should be noted that the perception measure alone can account for only 1%–2% of the total variation in the various schooling outcomes) and are at least consistent with the possibility that an intervention that increases perceived returns might lead to increases in schooling. However, we note that the magnitude of the effects suggests that information alone would not lead to universal high school completion; increasing perceptions by RD$1,000, which we will see below would close the gap between perceived and measured returns, would only increase secondary completion rates by nine percentage points, whereas 70% of students do not complete secondary school. Thus, perhaps not surprisingly, other factors certainly limit secondary school completion.

19. Data on income are missing for 139 observations, almost evenly split between treatment and control groups. We assign the median sample income to these observations, and include a dummy for these observations in the regressions. Dropping these observations from the regression instead does not change the results appreciably.

20. However, with their single cross section, they can only compare perceived returns with schooling decisions already made (i.e., the perceived returns to college among those who are of college age and already either in college or not). However, the authors argue that these results are still informative, because, for example, individuals who are the age of college freshmen (whether in college or not) should have roughly the same information they had at the time they made their college decisions a few months earlier. Further, they show that the distribution of perceived returns for adolescents of high school senior age is very similar to that of the college freshman–aged individuals.

TABLE III
MEASURED AND PERCEIVED MONTHLY EARNINGS, MALES AGED 30–40

	(1) Measured mean	(2) Perceived (self)	(3) Perceived (others)
Primary	3,180	3,516	3,478
	[1,400]	[884]	[863]
Secondary	4,479	3,845	3,765
	[1,432]	[1,044]	[997]
Tertiary	9,681	5,127	5,099
	[3,107]	[1,629]	[1,588]
Secondary − primary	1,299	329	287
		[403]	[373]
Tertiary − secondary	5,202	1,282	1,334
		[1,341]	[1,272]

Notes. All figures in 2001 Dominican pesos (RD$). Standard deviations in brackets. Column (1) provides the mean earnings among men aged 30–40 from a household survey conducted by the author in January 2001. The number of observations is 1,278 primary, 339 secondary, and 83 tertiary. Columns (2) and (3) provide data from the Round 1 survey of eighth-grade male students, conducted by the author in April/May 2001. Column (2) refers to what current students expect to earn themselves under different education scenarios when they are 30–40. Column (3) refers to what current students believe current workers 30–40 years old with different education levels earn. For both columns, there are 2,025 observations with responses for primary and secondary, and 1,847 responses for tertiary.

II.D. How Accurate Are Student Perceptions?

Table III provides data from the household and student surveys on measured and expected or perceived earnings by education. As noted above, the simple mean difference in earnings for those with primary only and those with secondary only is RD$1,299, or 41% (8% per year). We will use this benchmark for assessing the accuracy of students' perceptions; in the Online Appendix, we show that the estimated returns decline only slightly (about five percentage points) when additional covariates are controlled for, and then become 10%–20% larger when we use distance to school in childhood as part of an instrumental variables strategy due to Card (1995) and Kane and Rouse (1993), in an attempt to account for potential omitted variables and measurement error (see Tables A.1 and A.2 in the Online Appendix). Other studies of the Dominican Republic and broadly comparable countries have also found high returns to completing secondary school, typically in the range of 20%–80%; for example, data from the World Bank's Socioeconomic Database for Latin America and the Caribbean (SEDLAC) reports returns of 20%–30% from 2000–2006 in the Dominican Republic. Thus, although our estimates may not be purged of all econometric concerns, the best available evidence suggests that there are large returns to schooling in the country, consistent with what has been found almost universally in other low-income countries (see Psacharopoulos and Patrinos [2004], who in fact report the highest returns on average are found in Latin American and Caribbean countries), even in studies with more plausibly exogenous sources of variation in schooling with which to measure the returns (such as Duflo [2001]).

By contrast, in presurvey focus groups, it was evident that few students perceived significant returns to secondary school.[21] Column (2) of Table III shows that eighth-grade boys report on average that if they were to leave school at the end of the current year and not complete any more schooling, their (own) expected monthly wage at age 30–40 would be RD$3,516, which is greater than that actually measured in the household survey. By contrast, students on average expect monthly earnings of RD$3,845 if they complete secondary school, which is much lower than that observed in the earnings data. Thus, comparing to column (1), students overestimate earnings with primary schooling (by about RD$330, or 11%) and underestimate earnings with secondary schooling (by about RD$700, or 14%). Although they were not directly asked for the expected difference in earnings or the expected returns to schooling, the average implied perceived return is RD$329 (9%), which is only one-fourth as large as the estimate from the earnings data. About 42% of students report no difference in own expected earnings for the two levels of education, and 12% have implied returns that exceed those measured in the data. Using these expectations, if we assume that students expect to work until they are 65, and have a discount rate of 0.05, even if there were no direct costs of schooling, the implied net present value of the lifetime expected stream of earnings without secondary school is 11% *greater* than with secondary school. Thus, unless students believe there are high nonwage returns, completing secondary school would only be worthwhile for students with these expectations if they were extremely patient (i.e., a discount rate of 0.005 or less).

As noted above, any discrepancy between measured and own expected earnings could arise because students feel they have information about themselves that influences where they will fall in the earnings distribution, for example, because they attend poor-quality schools or because of their race. Thus, column (3) presents

21. Though most believed there were significant returns to completing primary school.

data on what students think current adult workers aged 30–40 earn. The means here are lower than own expected earnings for both levels of education, consistent with a general optimism bias. About 55%–60% of students report the same value for current workers as they expect for themselves for both levels of schooling, with about 25%–30% expecting higher wages for themselves and 10%–15% expecting lower wages. As with own expected earnings, the perceived mean difference in earnings for other workers by education is much lower (only about one-fifth as large) than that measured in the earnings data (and 13% lower than what they expect their own personal returns would be). The fact that this measure of perceptions is not influenced by beliefs about personal characteristics that affect earnings but instead just reflects general knowledge of labor market conditions suggests that students do not have accurate information on earnings and appear to underestimate the returns to schooling.

Table III also provides data on the measured and expected returns to completing college. As with secondary schooling, students' perceptions of earnings, and the implied perceived returns, are much lower than those measured in the household survey. Overall, students on average reported expected earnings of RD$5,127 for themselves and RD$5,099 for others with college education, implying returns over completing secondary school of 33% and 35%, respectively, compared to actual measured mean earnings of RD$9,681, implying a 116% return. However, it should be noted that because college is a rare outcome (less than 10% of adult males in our household survey have a college degree), this estimate of earnings is based on only 83 observations and is therefore likely to be fairly imprecise. Though for comparison, we also note that SEDLAC estimates returns to completing college of between 70% and 80% in the Dominican Republic from 2000–2006, which is also much greater than the difference in expected earnings reported by students. A final caveat is that, perhaps because college was perceived to be so unlikely an outcome or because so few students personally knew someone with a college degree, approximately 18% of students reported "don't know" or refused to answer this question. And those who do respond may not be a representative sample. Because only 13% of students at baseline reported planning to attend college, and only 6% had actually enrolled by the final survey, for the remainder of the paper we will focus on secondary schooling.

The fact that students have such low perceived returns to schooling raises the possibility that providing information on the higher measured returns may improve schooling. Of course, given the challenges both in estimating the true returns and in eliciting student perceptions of those returns, we cannot definitively conclude that students are "incorrect." For example, our measure of the returns may still be biased; alternatively, even if our estimates are the correct average returns for current workers, students might have reason to expect different returns for themselves[22] (though, again this would not explain why they perceive low returns among current workers). However, there are two final points worth making. First, students' implied estimates of the returns are so low (about 2% per year of secondary schooling) that unless we believe our estimates of the market returns are highly biased and that the true returns in the Dominican Republic are dramatically lower than the returns estimated for almost every other country (and in net present value terms, actually negative), it seems likely that students do in fact underestimate the returns to schooling. Second, our experimental intervention does not per se rely on estimating either the true returns or students' perceptions correctly. The expected effect does depend on whether the estimates provided are above or below the returns perceived by students; but again, focus groups consistently revealed that most students believed there was little or no return to schooling, so this was not a major concern for the study.[23] However, both the

22. And although there is likely to be heterogeneity in the returns (say by school quality or race) and students may be aware of that heterogeneity, this alone could not explain why students on average have low expectations for the returns they would personally face. For example, for every youth from a below-average school who knows he or she has low personal returns, there should be one from an above average school who knows that he or she has higher than average personal returns. The (correct) high estimates for those from good schools should offset the (correct) low estimates for those from bad schools, so the average perceived return should not be lower than the measured return. The same would hold for other factors, such as race: black youths may believe the returns are lower for them, but white youths should then also believe that the returns for them are higher than average, so the average across a representative sample of youths should still hit the correct average return. This may not hold, however, if only youths with the attributes that lower returns are aware that those attributes matter or if, for example, all youths think they go to a below-average school (which of course can't be true and thus would still suggest some youths have incorrectly low perceived returns). Further, this general hypothesis is not consistent with students on average expecting higher returns for themselves than for the average current worker, as in Table III.

23. We were concerned about providing misleading information, such as grossly overstating the returns, especially if they vary by race, region, or family background. However, the intervention was justified on the grounds of simply

appropriateness of such an intervention from a policy perspective and the long-term potential effectiveness of such a policy may well depend on the ability to provide accurate information to students. We discuss these issues further in Section IV.

Finally, we note that within the Becker human capital framework, there are reasons other than low returns for which specific individuals may receive low levels of education, such as the combination of poverty and credit constraints. Such constraints have long been considered significant impediments to schooling, especially in poorer countries. We therefore view the provision of information as an intervention that is likely to have an impact only on the specific subset of individuals for whom low perceived returns and correspondingly low demand for schooling are the only limiting factor, rather than on all students.

III. Results

III.A. Perceived Returns to Schooling

Table IV provides data on key outcome variables for the treatment and control groups in the pre- and postintervention survey rounds. As expected given randomization, in the baseline survey there was little difference between the two groups in own expected earnings with or without a secondary school degree, and thus little difference in the implied perceived returns (Table I shows that none of these baseline differences are statistically significant).

providing students the best available information, as well as informing them of the methodology and its limitations (as best as possible), and making it clear that the earnings data were national averages, not necessarily what they could expect for themselves: "We also used statistical methods to try to account for the fact that different kinds of people get different amounts of education; the results were similar. However, no method is perfect, and people differ in many ways that affect their earnings, and statistics can't always capture those differences. And of course, there is no way to predict anyone's future, so our results don't signify that this is what you yourself will earn, these are only averages over the population." Though the returns may vary by race, for example, so the returns are not as great for some students in our sample, we would only believe the intervention was potentially harmful to those students if we believed their current level of schooling was efficient, which we find unlikely. We also view our intervention as consistent with the numerous efforts under way in the country aimed at increasing educational attainment, especially for the most disadvantaged groups. Finally, we also note that it is even possible that the returns given to students may be an underestimate of the true returns, because we provided the OLS rather than the larger IV estimates and ignored the value of benefits (which we note in the Online Appendix adds RD$212 or six percentage points to the returns to secondary education) and other nonwage returns such as reduced variability in earnings or less hazardous conditions, plus the fact that the returns appear to increase with age, as shown in Table A.3 in the Online Appendix.

However, in the follow-up survey four to six months later, the treatment group reported on average greater expected earnings associated with secondary school completion, and lower expected earnings with only primary school. For the control group, there was an increase in expected earnings for both levels of schooling, though more so for secondary.[24] Thus the treatment group experienced a large relative decrease (RD$284) in expected earnings with only primary school and a smaller relative increase in expected earnings with secondary school (RD$80). Based on a simple difference-in-difference calculation in column (5), the intervention on average raised own perceived returns by a statistically significant RD$366. Overall, 54% of the treatment group had increased implied own expected returns between the two rounds, compared to about 27% for the control group. However, there was heterogeneity in response to the treatment. About 28% of the treatment group had increased implied returns of RD$1,000 or more, compared to 7% for the control group. The changes in students' estimates of the earnings of current workers by education are very similar to those for own expected earnings, with again a large and statistically significant increase in the implied perceived returns to schooling.

In light of these results, we can reestimate the relationship between perceived returns and schooling in Table II, using the treatment indicator as an instrument for perceptions. Provided there is no channel other than perceptions through which this intervention might influence schooling, this exercise can help validate that measured perceptions can serve as predictors of schooling. For this analysis, we use Round 2 perceptions for the full sample (in contrast to the earlier results using Round 1 perceptions, just for the control group).[25] The results are presented in the last six columns of Table II. For returning to school and years of schooling, the IV estimates are much larger than the corresponding OLS estimates (0.095 vs. 0.16 for

24. Although there may just have been an overall general increase in expected earnings due to changes in labor market or macroeconomic factors or because students grew older between the rounds, sample selection is also likely to cause an increase in the mean implied expected return to schooling for both treatment and controls. Students who returned to school in Round 2 (and thus who presumably had higher expected returns to schooling) were slightly more likely to be interviewed in that round than students who did not return, and thus we are more likely to have second-round data on expected earnings for these students.

25. The regressions where "returned to school" is the dependent variable reflects a decision already made at the time the perceptions used in these regressions are asked and may for example exhibit a greater degree of endogeneity (e.g., justification bias).

TABLE IV
EFFECT OF THE INTERVENTION ON EXPECTED RETURNS AND SCHOOLING: NO COVARIATES

	Panel A. Perceived returns to school				
	Round 1		Round 2		
	Control	Treatment	Control	Treatment	Difference-in-difference
Expected earnings (self):					
Primary (only)	3,548	3,484	3,583	3,230	−284***
	(116)	(124)	(118)	(92)	(43)
Secondary (only)	3,884	3,806	4,001	3,995	82*
	(132)	(145)	(132)	(114)	(44)
Implied perceived returns	336	322	418	765	366***
	(25)	(27)	(24)	(34)	(29)
Expected earnings (others):					
Primary (only)	3,509	3,447	3,546	3,204	−274***
	(112)	(120)	(113)	(92)	(41)
Secondary (only)	3,802	3,728	3,892	3,916	102**
	(126)	(143)	(120)	(111)	(45)
Implied perceived returns	293	281	346	712	377***
	(23)	(29)	(22)	(31)	(26)
Number of observations	1,003	1,022	922	977	1,859

TABLE IV
(CONTINUED)

	Panel B. Schooling					
	Round 2			Round 3		
	Control	Treatment	Difference	Control	Treatment	Difference
Returned to school?	0.55	0.59	0.042*			
	(0.02)	(0.02)	(0.025)			
Completed secondary school?				0.30	0.32	0.020
				(0.02)	(0.02)	(0.024)
Years of schooling completed				9.75	9.93	0.18*
				(0.070)	(0.073)	(0.098)
Number of observations	1,118	1,123	2,241	1,033	1,041	2,074

Notes. Standard errors, corrected for clustering at the school level, in parentheses. All measures of expected earnings are for earnings at 30–40, measured in nominal (2001) Dominican pesos (RD$). Data are from a survey of eighth-grade male students, conducted by the author. Round 1 was conducted in April and May of 2001; Round 2 was conducted in October of 2001; Round 3 was conducted in May and June of 2005.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

returned and 0.37 vs. 0.63 for years), with both IV estimates statistically significant at the 5% level or better. Instrumenting also increases the standard errors dramatically; as a result, we cannot reject that the OLS and IV coefficients are equal. For completing secondary school, the coefficients are identical for the two regressions, but the standard errors are again significantly greater. Overall, these results are further confirmation that survey measures of perceptions are useful predictors of schooling outcomes, supporting the conclusions of Kaufmann (2008) and Attanassio and Kaufmann (2008).

III.B. Schooling Outcomes

It is the large changes in expected returns observed above that we predict will affect schooling behavior, especially for students not constrained by other factors such as poverty and credit constraints. It is worth noting that because the change in the expected returns is driven to a great extent by a decline in expected earnings with only primary schooling, the intervention not only increased the expected future wage gap, but also lowered the opportunity cost of schooling, which is borne much sooner and thus not reduced as much through discounting. Thus, we might expect a bigger effect than if the increase in implied expected returns was driven more by an increase in expected earnings with secondary schooling.

As stated earlier, because schooling is compulsory only through the eighth grade, the students in our sample were not required to return to school in the academic year following the first survey. The bottom panel of Table IV provides data on subsequent school attainment; for now, we present data on reported schooling (by the student, their family or neighbors); below, we focus on results using only verified schooling data. The table shows that the treatment group was four percentage points (7%) more likely to have returned to school the following year, though the difference is only marginally statistically significant (p-value of .091). They also achieved on average 0.18 more years of schooling over the next four years. Finally, the difference in the likelihood of completing secondary school is positive, but small (two percentage points) and not statistically significant.

Table V presents regression estimates of the effects of the intervention, where we have regressed the schooling outcomes for individual i, S_i, on an indicator for having received the treatment, $S_i = \beta_0 + \beta_1 \text{Treatment}_i + \beta Z_i + \varepsilon_i$, and control for other variables,

TABLE V
EFFECTS OF THE INTERVENTION ON EXPECTED RETURNS AND SCHOOLING

	Full sample				Poor households				Least poor households			
	(1) Returned next year	(2) Finished school	(3) Years of schooling	(4) Perceived returns	(5) Returned next year	(6) Finished school	(7) Years of schooling	(8) Perceived returns	(9) Returned next year	(10) Finished school	(11) Years of schooling	(12) Perceived returns
Treatment	0.041*	0.023	0.20**	367***	0.006	−0.01	0.037	344***	0.072*	0.054*	0.33***	386***
	(0.023)	(0.020)	(0.082)	(28)	(0.034)	(0.026)	(0.11)	(41)	(0.038)	(0.031)	(0.12)	(41)
Log	0.095**	0.23***	0.79***	29.0	0.054	0.26***	0.69***	188**	0.047	0.10	0.51	23
(inc. per capita)	(0.040)	(0.044)	(0.16)	(47)	(0.068)	(0.062)	(0.23)	(87)	(0.12)	(0.13)	(0.45)	(133)
School	0.011	0.019**	0.086**	0.74	0.001	0.015	0.064	−9.5	0.025*	0.024*	0.10**	8.2
performance	(0.010)	(0.009)	(0.034)	(14)	(0.014)	(0.012)	(0.048)	(13.5)	(0.013)	(0.012)	(0.048)	(22)
Father	0.074**	0.050*	0.26**	−24	0.056	0.019	0.16	−29.1	0.096**	0.096**	0.36**	−3.8
finished sec.	(0.030)	(0.030)	(0.12)	(32)	(0.045)	(0.043)	(0.18)	(62)	(0.038)	(0.038)	(0.14)	(40)
Age	−0.010	0.004	−0.006	−42*	−0.042	0.002	−0.071	−46	0.005	0.005	0.025	−35
	(0.016)	(0.015)	(0.059)	(21)	(0.030)	(0.019)	(0.088)	(32)	(0.025)	(0.035)	(0.087)	(29)
R^2	.016	.040	.049	.090	.007	.019	.014	.094	.020	.020	.029	.090
Observations	2,241	2,205	2,074	1,859	1,055	1,055	1,007	920	1,056	1,056	1,002	939

Notes. Heteroscedasticity-consistent standard errors accounting for clustering at the school level in parentheses. Data are from a survey of eighth-grade male students, conducted by the author. Returned next year is measured in Round 2; finished school and years of schooling are measured in Round 3. Perceived returns in columns (4), (8), and (12) is the change between Round 2 and Round 1 in the difference between what students expect to earn themselves with primary and secondary schooling when they are 30–40, measured in 2001 Dominican pesos (RD$). All regressions also include an indicator for whether income data were unavailable (these households are assigned the median sample income). In columns (5)–(12), youths are split according to whether they live in a household that is below (poor) or above (least poor) the median household income per capita; households with missing income data are excluded from both categories. School performance is teacher assessment of the student's performance, on a scale of 1 to 5 (much worse than average, worse than average, average, above average, much better than average). Age, school performance, and whether the father finished secondary were gathered in the first round; income was gathered in the second round.

*Significant at 10%.

**Significant at 5%.

***Significant at 1%.

Z, that are baseline predictors of schooling outcomes, as discussed above (child's age and eighth grade school performance, household income and whether the father completed high school).[26] All control variables were gathered in the first round, except income, which was gathered in the second round. Regressions for having returned to school the next year and having finished secondary school are estimated with linear probability models, though results using logits yield nearly identical conclusions (Table A.5 in the Online Appendix).

As with the simple treatment–control differences above, the results in columns (1)–(3) are somewhat mixed in terms of statistical significance. Overall, for the four-year period over which students were followed, the treatment caused a statistically significant 0.20 increase in years of schooling on average. However, the impact on the likelihood of returning to school the following year, although large, is only marginally statistically significant (p-value of .08), and the impact on completing secondary school, although positive, is not statistically significant. Most other variables have the expected sign, with higher socioeconomic status (income and whether the father finished secondary) and better school performance associated with increases in schooling.

As noted above, within the standard human capital framework, demand is not always sufficient for schooling. For some youths, even if they wanted to attend school, a combination of costs, low family income, and credit constraints will limit the effectiveness of the intervention. This is especially likely to be the case for completing secondary school, which requires a longer term and more costly investment. Therefore, columns (5)–(12) of Table V present separate regressions for youths in households below ("poor") and above ("least poor") the median household income per capita. Cases where the student's family was not interviewed in Round 2 lack income data and are excluded from this analysis (reclassifying households with missing income data as either all poor or all least poor does not change the results appreciably). It should be pointed out, however, that although the role of credit constraints was recognized as part of the study's conceptualization, the experiment itself was not explicitly designed to account for this (for example, by randomizing the intervention within wealth strata).[27] Therefore, the results of this

26. Specifications controlling for baseline perceived returns to schooling yield nearly identical results; see Table A.4 in the Online Appendix.

27. The ability to stratify the experiment by income or wealth was limited by the fact that the initial survey had to be conducted at schools so that we could

stratification, although potentially informative, and motivated by the considerable literature documenting the role of poverty and credit constraints in limiting schooling in low-income countries, should be interpreted with somewhat more caution. Means and tests of covariate balance for treatment and control groups within the poor and least poor subsamples are provided in Table A.6 in the Online Appendix (this table also contains the estimated treatment effects excluding other covariates, which yield very similar results). Overall, despite not being explicitly stratified, the randomization still appears to have achieved covariate balance between the treatment and control groups within these subsamples.

For the poorest households, the effect of the treatment is extremely small and not statistically significant for all three measures of schooling. This is despite the fact that in column (8), the treatment appears to have had a large effect on perceived returns to schooling for these students. By contrast, for youths from wealthier (though still quite poor) households, the effects are large, and statistically significant at the 10% level or better for all three education measures (though the effect for finished secondary is not statistically significant without the additional covariates (Table A.6 in the Online Appendix), and only marginally significant with them). For this group, the intervention increased the average years of schooling over the four-year period by 0.33. There was a seven–percentage point (11%, from a base of 56%) increase in the likelihood of returning to school the academic year following the intervention, and a five–percentage point (13%, relative to a base of 40%) increase in secondary school completion. The differences between the poor and least poor are all the more notable given that the intervention had a similar impact on perceived returns for the two groups. Though we would only reject equality of the schooling treatment effects for the poor and least poor samples for years of schooling, the fact that the point estimates for the poor sample are so small (0.006 for returning to school, −0.01 for finishing secondary, and 0.037 for years) is consistent with the treatment being limited in impact for poor households, despite having increased potential demand just as

get a large enough sample of eighth-grade boys. Surveying at schools meant we could not measure students' household income at baseline. A survey of their home households was possible only for the second round, when more resources became available.

much as for the least poor group. This suggests at least some role for poverty and credit constraints in limiting schooling.[28]

Overall, the effects for the least poor students are large and striking. The magnitudes compare favorably with large-scale programs implemented elsewhere, such as Mexico's PROGRESA, which provided direct cash incentives to increase school attendance.[29] And many of these other programs are extremely expensive,[30] whereas in the present case, information could potentially be provided at low cost. Though, again, we only expect information to have an impact when students are misinformed about the returns and when no other constraints prevent students from attending school, whereas other programs may be effective for a wider group of students.

28. Though we can't rule out that because perceived returns for poor youths are lower on average (231 vs. 417), increasing them by the same amount does not move as many over the margin to where it is worthwhile to go to school. However, the results in Table A.4 in the Online Appendix show that perceived returns have much smaller impacts on schooling for the poor sample, supportive of the conclusion that perceived returns are delinked from schooling for the poor, consistent with poverty and credit constraints explaining the poor vs. least poor treatment differences (though in Table A.4 we would not reject equality of the coefficients for least poor and poor, and only one of the coefficients for the least poor sample is statistically significant). This conclusion is also consistent with Attanasio and Kaufmann (2008) and Kaufmann (2008), who find that perceived returns only predict schooling for the least poor students in Mexico. Further, estimates of the education impacts using logit models (Table A.5 in the Online Appendix), which do not force the effects of the treatment to be small for individuals far from the margin, yield nearly identical estimates to the least squares estimates above.

29. PROGRESA, whose payments also were conditioned on other requirements and also provided other benefits, increased enrollments for ninth grade boys from 60 to 66 percentage points (Schultz 2004), close to what was found here for wealthier students. For other comparisons, Duflo (2001) finds that a program in Indonesia that built approximately 61,000 primary schools (effectively doubling the stock) resulted in a 0.25–0.40 increase in years of schooling, or 0.12–0.19 years (comparable to the results found here for the full sample) for each additional school built per 1,000 students. Angrist, Bettinger, and Kremer (2006) find that a large voucher program in Colombia increased secondary school completion rates by five to seven percentage points (a 15%–20% gain), similar to what we find for wealthier students. Of course, these results are not directly comparable; for example, Indonesia was in 1973 (and still is) a much poorer country than the Dominican Republic today, PROGRESA started from a much higher enrollment base, and both it and the Colombian voucher program targeted the poorest students, so improvements in schooling may have been harder to achieve in these other cases.

30. For example, PROGRESA cost nearly 0.2% of Mexico's GDP to provide benefits to about one-ninth of all households. Indonesia's program cost about 1.5% of 1973 GDP, or about 750 million 2007 dollars. And the Colombian vouchers came at a cost of about $190 per year of attendance (though for the government some of the cost would likely be offset by savings in expenditures for public schools). There are of course other interventions that have also been shown to be very cost-effective, such as the deworming program studied by Miguel and Kremer (2004), which achieved gains at a cost of about $3.50 per additional year of schooling.

III.C. Robustness

To this point, we have used data on education as reported by the students (or their families or neighbors). The primary concern is that students may inflate the amount of education they achieved, especially if they received the treatment. A second concern is a general decline in accuracy when students or their relatives could not be interviewed (typically because the family had moved) and schooling data were obtained instead from neighbors. As stated, we attempted to verify schooling data for all students, but were unable to do so for 3% of students in the second round and 9% in the third round. Most of the cases where data could not be verified were due to obtaining schooling information from neighbors or more distant relatives, because they often did not know which school the youth attended. Before turning to these results, we make two observations. First, there were very few cases (27) where a youth reported schooling that differed from what his school reported. This is largely because students were typically interviewed during the daytime on school days (at home, work, or school), so students not in school would be unlikely to misreport that they did attend school. Second, to an extent, the results in columns (5)–(12) of Table V already eliminated many of the nonverified households, because if a neighbor had to report on the youth's schooling, we would also not have income data for that household and it would have been dropped from the analysis. However, the overlap is not perfect, as there are some households where neighbors provided schooling data that could be verified.

Table A.7 in the Online Appendix reveals that using only the verified data reduces the sample sizes slightly, but does not change the results appreciably. The effect of the treatment for wealthier households is still positive for all three measures of education, though slightly smaller for years of schooling and having completed secondary school; and in the latter case, the significance level declines (p-value of .12) so that it no longer falls within conventional levels. However, in terms of both returning for ninth grade and total years of schooling, the results indicate that the schooling gains were real, rather than reporting bias. However, we must maintain the assumption that enrollment among students whose data could not be verified is not negatively correlated with the treatment.[31]

31. For example, if we make the strong assumption that all control students whose data could not be verified had the best educational outcome (returned;

A second issue we consider is whether just by students being asked to form their expectations of earnings for various levels of schooling, they acquire information or begin to think about the schooling decision in a way they would not have otherwise; alternatively, there may be an effect of just being interviewed by a research team as part of a project from an American university. Because both treatment and controls were administered the same survey except for whether they were provided with information on returns at the end, this does not affect our interpretation of the effect of the treatment.[32] However, one issue to consider is whether the control group was influenced by the interview. Therefore, in column (1) of Table VI, we compare the full-sample control group to a "shadow" control group of fifteen randomly selected students at each of thirty randomly selected nonsample schools (chosen to obtain approximately the same population distribution as the original student sample). These students were identified but not interviewed until the second round (unfortunately, they were not followed after this round). However, we only gathered data on enrollment status for this group, so in the regression we only include an indicator for being in the control group that was interviewed. The results show that the original, interviewed control group experienced no differential change in enrollment relative to the noninterviewed control group; the coefficient is positive, but small and not statistically significant. Thus, the provision of information on the returns to schooling appears to be the critical factor for achieving schooling gains.

Finally, although the results suggest that the increased schooling was due to the impact of the intervention on perceived returns to schooling, we are unable to rule out that some of the effect was due to other factors, such as reducing the uncertainty

finished; twelve years of school) whereas all treatment students whose data could not be verified had the worst educational outcome (not returned; not finished; eight years of school), the treatment effects are smaller, and not statistically significant (columns (7)–(9) of Table A.8 in the Online Appendix, for the full sample). Although we have no reason to believe nonverified treatment students are less likely to be enrolled than nonverified control students, this assumption is not testable. If we instead assume either all students whose data could not be verified had the worst outcomes or all had the best outcomes, there are slight increases in the magnitudes and statistical significance of the treatment effects (columns (1)–(3) and (4)–(6) in Table A.8 in the Online Appendix). This result is expected, because attrition is slightly higher for the control group (column (10)).

32. Unless we believe that the intervention would not have been effective without students first going through the interview, or without the presence of our research team.

TABLE VI
ADDITIONAL TESTS

	Shadow controls	Δ implied return (self) < RD$1,000		
	(1) Returned next year	(2) Returned next year	(3) Finished school	(4) Years of schooling
Treatment		0.028	0.012	0.13
		(0.026)	(0.021)	(0.084)
Log (inc. per capita)		0.091*	0.20***	0.77***
		(0.047)	(0.047)	(0.17)
School performance		0.020*	0.021**	0.091**
		(0.012)	(0.009)	(0.037)
Father finished secondary		0.064**	0.044	0.22*
		(0.031)	(0.030)	(0.12)
Age		−0.014	0.006	−0.008
		(0.019)	(0.017)	(0.066)
Interviewed	0.014			
	(0.027)			
R^2	.00	.014	.031	.036
Observations	1,575	1,664	1,664	1,577

Notes. Heteroscedasticity-consistent standard errors accounting for clustering at the school level in parentheses. Data are from a survey of eighth-grade male students, conducted by the author. Returned next year is measured in Round 2; finished school and years of schooling are measured in Round 3. Column (1) uses only students from the main sample control group, plus a secondary administrative control group consisting of students from schools where no interviews took place. Columns (2)–(4) focus on the subsample of students in the study whose reported changes in implied perceived returns at age 30–40 were less than RD$1,000. School performance is teacher assessment of the student's performance, on a scale of 1 to 5 (much worse than average, worse than average, average, above average, much better than average). Age, school performance, and whether the father finished secondary were gathered in the first round; income was measured in the second round. All regressions also include an indicator for whether income data were unavailable (these households are assigned the median sample income).

*Significant at 10% level.

**Significant at 5% level.

***Significant at 1% level.

of students' estimates,[33] or that when providing information on the returns, enumerators provided additional information or encouragement to students to remain in school. However, we can provide a limited exploration of whether increased perceptions of the returns played at least some role in schooling improvements by considering whether there was any effect for those youths who did not significantly update their beliefs. Columns (2)–(4) of Table VI restrict the sample to youths whose perceptions of

33. For example, if students were initially more uncertain of their estimates for earnings with secondary school than their estimates for earnings with primary school, reduced uncertainty due to the treatment might in itself have had an independent effect on the decision to stay in school, even if estimates of the returns were unchanged.

the returns increased between the first and second rounds by less than RD$1,000. For this group, the coefficients are positive, but extremely small and not statistically significant. Although this is only a limited test, and even though we could not reject the hypothesis that the effects for this sample are equal to those for the full sample, the very low point estimates are consistent with the effect of the intervention being limited to only those youths who significantly updated their beliefs about the returns to schooling.

IV. Discussion and Conclusions

We find that despite high measured returns to secondary schooling in the Dominican Republic, the returns perceived by students are low. This finding suggests a possible inefficiency and may even a reflect a potential development trap, as the relative skill composition demanded by the labor market is not transmitted to youths in the form of greater perceived returns, resulting in an undersupply of skilled labor, which in turn inhibits the development of domestic skill–intensive industries or the ability to attract foreign direct investment.

An intervention that provided information on the measured returns increased both perceived returns and schooling. The results suggest that demand appears to be a limiting factor in schooling attainment in the Dominican Republic. The effects of the treatment on schooling are large and striking; there are few examples of policies or interventions that result in a 0.20- to 0.35-year increase in schooling, much less interventions that are as potentially inexpensive as this one. An additional advantage of information-based programs such as that applied here is that they may result in students who are more committed to school and provide greater effort than under other programs, because they stimulate the demand for schooling itself, rather than, say, the cash incentives to be obtained through attendance. For example, Nguyen's (2008) findings that providing information on the returns to schooling improves school performance in Madagascar support this hypothesis. And in a regression for the sample of students enrolled at the time of the second round survey in our study, we find that the treatment increased time spent on homework by about 11 minutes per week on average.

However, the intervention undertaken here may be limited in its potential scope and applicability, as it will only be effective in cases where the perceived returns are low relative to the true returns, and no other constraints such as poverty limit investment in schooling. Thus, the optimal strategy may involve a combination of stimulating demand by providing information on the returns and lowering the barriers to attendance by reducing school fees or providing financial support. Another limitation on our study is that we focused on boys, so we have no information on how accurate girls' perceptions of the returns to schooling are or what impact the intervention might have on them. However, given the large and striking results found here, studies in other settings are worth consideration. Already in this spirit, Nguyen (2008) finds significant effects on children's attendance and test scores in the months after parents in Madagascar are provided with information on the returns to schooling.

As noted in the Introduction, there are several potential explanations for why students might underestimate the returns to schooling. In the Online Appendix, we explore the hypothesis that residential segregation by income, coupled with residential mobility (akin to the argument of Wilson [1987]), may be playing a role in the Dominican Republic. In particular, if youths are only able to observe the earnings of workers who live in their neighborhoods, residential segregation will lead to lower estimates of the returns due to differential selection by education. For example, poor neighborhoods may contain most of the workers with low levels of education, but only those more highly educated workers who had the worst income draws, so that within these neighborhoods the more highly educated workers do not earn much more than those with less education. The opposite form of selection will arise in rich neighborhoods, with the net result that within all segregated communities, the local mean difference in earnings by education will be less than the difference by education for the country as a whole, obscuring the returns to schooling. Such segregation would present a case where a strong argument could be made for the type of information intervention undertaken in the present study. In the Online Appendix, we find some supportive evidence of this hypothesis, though our tests are limited and we are unable to rule out alternative interpretations of the results. Further research could explore this hypothesis and its implications in more detail.

Of course, the desirability of such information-based programs will depend on the ability to provide accurate information on the returns to schooling, which may often be difficult. Further, even with accurate estimates, there may be reasons that the returns for the marginal child may not be as large as the currently measured average return.[34] This is made all the more complicated by the fact that even if current estimates of the returns are correct, we would in effect need to forecast the returns to be expected in the future.[35] Although there may be some public good or spillover effects of education that make the social returns higher than the private returns, it is unlikely to be desirable public policy to provide information known to be incorrect, even if it leads to outcomes deemed socially desirable. Further, doing so may undermine the effectiveness of the program in the short run (i.e., students may be less likely to believe information that differs markedly from the available evidence) and in the long run (i.e., if younger cohorts of students see older cohorts invest in schooling and not achieve the gains they are told to expect, they will no longer believe the information given). Such effects could even spill over and undermine other government interventions or institutions. But even though it may not be possible to provide students with the absolute certain value of the returns they will personally face, there may still

34. For example, if those with the highest returns are the ones who currently finish schooling, the marginal child may have lower returns than the current average. However, if other factors also influence schooling (such as income, distance to school, costs, geography, rates of time preference, attitudes toward risk, or knowledge of the returns), then the prediction is more ambiguous. To take one extreme example, if high school completion rates were near-universal in the big cities but low elsewhere, the marginal youth from outside of the cities may have higher ability (and thus potentially, higher returns) than the average person who currently completes secondary school in the cities, because the former is still likely to be high in the ability distribution, whereas the latter includes almost the full distribution of ability, high and low. This is not to say that marginal students may not have lower returns on average, only that the prediction depends on a number of factors. We note here also, however, that even if the returns for the marginal student were lower, this could not explain why students underestimate the returns to schooling in our study, because Table III shows that their estimates of the earnings of current workers with different levels of schooling are also low. Of course, this does not rule out that the returns for the marginal student may actually differ from the current average, which reinforces the difficulty in providing students with accurate estimates of the returns they will face.

35. Though rates of completion of secondary school have been increasing over time, which all else equal might be expected to lower the returns, the evolution of future returns for the Dominican Republic is uncertain. As Goldin and Katz (2008) note in their analysis of the United States, a great deal depends on the growth of the supply of skilled labor relative to the demand for skilled labor, as well as technological change (potentially, in a nonmonotonic way). It is also worth noting that the supply of workers with primary school has increased even more rapidly, which might at least in the near term depress the wages of such workers (or more generally, workers with just the basic skills of literacy and numeracy). Further, there may be spillover effects whereby the supply of educated workers actually increases the demand for skilled labor, such as by spurring greater innovation, making firms more competitive, or attracting foreign investment. Unfortunately, there are not sufficient data to examine how the returns have been changing over time in the Dominican Republic. And the estimates for the few points in time that are available are not comparable, due to differences in data and methodology.

be a value to providing the best available current estimate of the returns, which students can use as the basis for forming their own expectations, especially if provided alongside the appropriate caveats about uncertainty about how returns will evolve in the future.

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