Prospering through *Prospera*: A Dynamic Model of CCT Impacts on Educational Attainment and Achievement in Mexico*

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Abstract

This paper develops and estimates a dynamic model, which integrates value-added and school-choice models, to evaluate grade-by-grade and cumulative impacts of the Mexican *Prospera* conditional cash transfer (CCT) program on educational achievement. The empirical application advances the previous literature by estimating policy impacts on learning, accounting for dynamic selective school attendance and incorporating both observed and unobserved heterogeneity. A dynamic framework is critical for estimating cumulative learning effects because lagged achievements are important determinants of current achievements. The model is estimated using rich nationwide Mexican administrative data on schooling progression and mathematics and Spanish test scores in grades 4-9 along with student and family survey data. The estimates show significant CCT impacts on learning and educational attainment, particularly for students from poorer households. Results show that telesecondary schools (distance learning) play a crucial role in facilitating school attendance and in fostering skill accumulation.

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1 Introduction

Conditional cash transfer (CCT) programs aim to alleviate current poverty through transfers to poor families and to reduce future poverty by making these transfers conditional on investments in the human capital of children and youth. In 1998-2000, a large-scale randomized evaluation of the Mexican *PROGRESA* CCT program demonstrated substantial impacts on schooling enrollment and attainment, child work and family income (Parker and Todd, 2017). These findings contributed to a large scaling-up in Mexico and an impressive adoption of similar programs in more than 60 countries on five continents (Fiszbein and Schady, 2009).

This paper analyzes the understudied, substantive question of whether CCTs improve learning by developing and estimating a dynamic model of academic achievement and school progression. Several studies, using various methods including the original experiment, matching and structural dynamic models, have examined the impacts of *PROGRESA/Oportunidades/Prospera* on school enrollment and, in some cases, on longer-term schooling attainment (e.g., Schultz (2004), Behrman et al. (2005b), Behrman et al. (2005a), Behrman et al. (2009), Todd and Wolpin (2006), Attanasio et al. (2012) and Parker and Vogl (2023)). This literature demonstrated positive program impacts on school enrollment and attainment. However, a longstanding concern has been whether and to what extent this increased school enrollment translates into higher academic achievement, a likely determinant of the extent to which CCTs can improve earnings potential and other longer term outcomes. Most prior studies did not analyze academic achievement impacts, because the original evaluation data did not include achievement test scores.[1]

With newly available data, we are now able to examine the effects of the *Prospera* program (the program name during the time of our data collection) not only on school enrollment and attainment but also on academic achievement in mathematics and Spanish. Nationwide standardized longitudinal administrative test-score data (called the *ENLACE* data) as well as complete enrollment rosters were merged with administrative information on which students come from *Prospera* households and on school locations. They were also merged with survey information obtained from students and their parents. These data allow the study of how students' *Prospera* beneficiary status affects their school enrollment, school choice, grade progression and academic achievements over time.

Although our longitudinal administrative and survey data are rich and have the advantages of national coverage and large sample sizes, the data were not collected explicitly for the purpose

[1] In 2003, Woodcock-Johnson tests in mathematics and Spanish were applied to a single cross section. Using these data, Behrman et al. (2009) found no impacts of *PROGRESA* participation on achievement, based on comparing test scores of the original treatment and control groups. However, because the original control group was enrolled in the program 1.5 years after the original treatment group, the schooling differences between them were relatively small, at about 0.2 years of additional schooling.

of evaluating the *Prospera* program. There are at least five significant statistical challenges in using observational data of this kind to assess program impacts: (i) selective program participation, largely due to eligibility criteria that restrict access to high-poverty households, (ii) nonrandom school dropout, which mainly occurs after grade 6, (iii) grade retention in any grade, (iv) the availability of multiple school types and school choice, and (v) the presence of a small fraction of students suspected to have cheated on the tests. This paper develops a methodological approach for evaluating the effects of *Prospera* and illuminating the mechanisms through which program effects operate while accounting for these different features of the data and the context.

Some earlier studies consider the selection problem arising from nonrandom dropout in the context of analyzing educational outcome determinants (see, e.g.Cameron and Heckman (1998), Cameron and Heckman (2001) and Glewwe (2002)). As is common in administrative schooling data, including our data, the test scores are only observed for enrolled children who took the tests at school. The selection problem is dynamic as it occurs at each grade and the students at risk for dropping-out in a particular grade depend on the sample that stayed in school from previous grades. The *Prospera* CCT program induced students from high-poverty backgrounds who were at high risk for dropping-out to stay in school longer. If the CCT program induces weaker students to remain in school, then average test scores could fall as a result of more marginal students being included in the testing. This kind of selection problem arises whenever tests are administered in school, regardless of whether the data analyzed are experimental or nonexperimental.[2]

Our goal in this paper is to examine how the *Prospera* program affects schooling and academic achievement, accounting for the statistical problems noted in (i)-(v) in the penultimate paragraph. To this end, we develop and estimate a dynamic model of students' school progression that incorporates decision-making in each grade (4-9) with regard to enrollment, school choice, and dropping-out as well as grade- and subject-varying models for academic achievement. Specifically, our modeling framework combines value-added academic achievement models with school-choice models and links equations across ages/grades, allowing for both observed and unobserved heterogeneity. Valued-added models typically specify a relationship among academic achievement, key learning inputs in the current period, and lagged achievement, which is a sufficient statistic for past learning inputs under some assumptions about coefficients of past learning inputs following geometric patterns (Summers and Wolfe, 1977; Boardman and Murnane, 1979; Hanushek, 1979; Todd and Wolpin, 2003; Cunha et al., 2006, 2010).[3] School-choice models generally focus on the decision of what type

[2]This selection problem also affects cross-country comparisons of standardized tests, such as PISA test scores. The PISA tests are given in schools at age 15 and, in some countries, significant fractions of children have dropped-out by that age.

[3]There is some debate about whether value-added models with teacher fixed effects should be used to measure

of school to attend (Neal, 1997; McEwan, 2001; Altonji et al., 2005; Sapelli and Vial, 2002; Gallego and Hernando, 2009).[4] Value-added models and school-choice models are usually estimated in isolation, although there are a few papers that combine them (e.g., Hastings et al. (2012), Allende et al. (2019), and Schellenberg and Walters (2020).) Our model also incorporates dropout decisions and grade retention.

The model begins when students finish fourth grade and continues through the ninth grade.[5] Students in our sample differ in terms of their family backgrounds and their fourth-grade knowledge in mathematics and Spanish as measured by standardized test scores. The vast majority of children attend general primary schools close to home, but some families living in areas with high indigenous populations can choose between general and bilingual indigenous schools. At the end of each grade, students can progress to the next grade, repeat the same grade or (after grade six), drop out. Conditional on progressing to lower-secondary school (at the end of grade six), students/parents make a one-time choice of a lower-secondary school type, from up to three public school options–general, telesecondary or technical schools–all of which are academically oriented.[6]

In each period, we model skill accumulation in mathematics and Spanish using value-added production functions, with coefficients that vary by grade, school types, and grade-retention status. The dynamic-panel specification captures the notion that skill accumulation at one stage affects skill attainment at other stages, which has been shown to be essential to characterizing human-capital-skill formation processes (e.g. Cunha et al. (2006, 2010)). When students/parents choose from among different school types, they essentially choose a learning technology. The same inputs (including *Prospera* participation) may generate substantially different outcome trajectories depending on the school types that are available and are selected.

As previously noted, the model we estimate controls for selection arising from school-enrollment, dropout, grade-retention, and school-type choices, all of which potentially affect students' grade pro-

teacher effectiveness (see, e.g., Kane and Staiger (2008), Kane et al. (2013), Chetty et al. (2014a), Chetty et al. (2014b)). Our focus is rather on using these models to capture the cumulative learning process.

[4]Some studies use school-choice models to estimate school-voucher effects (Rouse (1998), Figlio and Rouse (2006), Hsieh and Urquiola (2006), Bravo et al. (2010), Angrist et al. (2002), and Angrist et al. (2006)), to study parents' preferences for school quality and to analyze the welfare effects of school policies (Epple et al. (2018), Hastings et al. (2009) and Neilson et al. (2019)).

[5]The *Prospera* cash transfers for attending school actually start in grade three. We start our model at grade four, because the data were collected over a six-year time frame and we want to follow students through grade nine, which is the last grade of lower-secondary school. There are no nationwide standardized tests in the 10th and 11th grades. By starting the model at grade four, we do not capture potential program impacts in prior grades and potentially understate program benefits.

[6]Technical schools differ from general schools by including vocational/technical educational curricular components. Telesecondary schools are distance-learning schools that largely serve rural communities and that enroll almost 20% of lower-secondary school students. *Prospera*-beneficiary family children attend telesecondary schools in greater proportions than average. We exclude private schools from the choice set as almost no Progresa beneficiaries attend private school. Section two provides more detail on how the school types differ.

gression and academic achievements. It also accounts for selective program participation arising from the fact that only high-poverty households are eligible to participate in *Prospera*. In particular, we limit our analysis subsample to households that are estimated to have positive probability of being a program beneficiary and, in addition, control for observed heterogeneity between *Prospera* and non-*Prospera* students using a rich set of family demographics. Our dataset contains information on most of the variables used to determine *Prospera* eligibility, but we also allow for the possibility of selection on some unobserved factors.[7] The unobserved heterogeneity is modeled as discrete latent multinomial types, as in Heckman and Singer (1984) and Cameron and Heckman (1998). These types enter multiple model equations and, in doing so, allow for across-equation correlated error structures. We allow the unobserved-type distribution to vary by *Prospera* beneficiary status and an index measure of local poverty.

The data we analyze also contain information on small percentages of students in each grade that are suspected to have copied answers on the multiple-choice standardized tests. In the context of a value-added test-score model, copying induces one-sided measurement errors in the dependent variables, the lagged dependent variables, or both, which we explicitly take into account in our maximum likelihood estimation procedure. The outcomes at different ages/grades are school enrollment, school choices, mathematics and Spanish test scores, dropping-out, and grade retention. We do not know of previous research that estimates value-added models accounting for possible cheating, although cheating on standardized tests is a ubiquitous problem.

We use our estimated model framework to evaluate how *Prospera*-beneficiary status affects schooling progression and academic achievements in different grades. In particular, we simulate school-choice decisions, school-enrollment decisions and academic achievements with and without the *Prospera* program, for children from different family backgrounds. There are multiple channels through which *Prospera* participation can affect these outcomes. First, past participation may increase lagged achievement, which can facilitate present learning. For example, greater comprehension of sixth-grade mathematics can facilitate learning and comprehension of the seventh-grade curriculum.[8] Second, contemporaneous program participation can directly affect learning if the program encourages regular school attendance, student engagement and study efforts. There are two reasons why we might expect the *Prospera* program to influence students in this way. *Prospera* program rules stipulated that children must attend school at least 85% of days and can only fail a

[7] As discussed in Todd and Wolpin (2003) and Rivkin et al. (2005) for value-added models and in numerous other studies of schooling (e.g., Behrman et al. (1980); Behrman and Rosenzweig (1999); Altonji et al. (2005); Rothstein (2009)), it is important to control for unobserved inherent student abilities, personality traits, or motivation, that matter for children's achievement growth.

[8] Cunha et al. (2006) term this feature of cognitive achievement production functions "self-productivity."

grade once to receive the cash transfers. Additionally, *Prospera* transfers may reduce the pressure on children/youth to work in labor markets while in school and thereby allow for greater focus on schoolwork (Skoufias and Parker (2001)).

Our analysis yields a number of findings regarding *Prospera*-program effects and the effectiveness of different school types. First, we find that *Prospera* participation reduces lower-secondary school dropout rates by 0.06-0.09 percentage points. This effect is most pronounced during the transition from sixth to seventh grade and appears similar for both girls and boys.[9] In primary-school grades (grades 5 and 6), our findings do not show any significant impact of the program on test scores. However, in lower-secondary grades (grades 7 to 9), we observe positive and statistically significant improvements in test scores. The effects are more substantial in mathematics, with a cumulative effect of 0.21 standard deviations by the 9th grade, compared to 0.04 in Spanish. Girls tend to have higher gains in mathematics, whereas boys show greater gains in Spanish. Therefore, the *Prospera* program narrows existing gender test score gaps that favor boys in mathematics and girls in Spanish. Additionally, and as expected, the effects of program participation accumulate over time and intensify with prolonged exposure. Intriguingly, children from more-disadvantaged backgrounds exhibit significantly larger improvements in test scores.

Our results contribute to the understudied yet substantive question of whether CCTs improve learning. Fiszbein and Schady (2009) and Baird et al. (2014) systematically reviewed a range of CCT programs worldwide and concluded that the effects of CCTs on achievement tests were disappointingly "small, at best." One caveat is that these conclusions are mostly drawn from relatively short evaluation periods and based on relatively small sample sizes.[10] When evaluating CCT programs over longer horizons, some studies report statistically significant effects on academic achievement. For example, Barham et al. (2013) used the randomized phase-in of the *Red de Proteccion Social* CCT program in Nicaragua to study effects on schooling attainment and learning for boys ten years later. They found a half-grade increase in schooling and substantial gains (approximately 0.25 standard deviations) in mathematics and language achievement scores. Comparing two cohorts (2007 and 2013), Hadna and Kartika (2017) found statistically significant effects of a CCT program called *Program Keluarga Harapanin* in Indonesia on three subjects (Bahasa Indonesia, mathematics and English) as well as national mathematics examinations for junior-high-school students. Our results can reconcile some of the mixed findings in the literature by highlighting the accumulative feature

[9] Some studies have suggested a greater impact of *PROGRESA* on secondary-school enrollment for girls (Schultz, 2004; Parker and Vogl, 2023), but other research finds similar effects for both genders in lower-secondary education (Behrman et al., 2005b; Todd and Wolpin, 2006).

[10] Due to data limitations, there are many fewer studies of achievement than there are of enrollment. For instance, Snilstveit et al. (2017) reviewed 38 studies of the effects of transfers on enrollment; only 11 of the programs analyzed effects on achievement.

of the CCT program effects.

Our study sheds light on the efficacy of various types of Mexican schools in enhancing test scores. This includes telesecondary schools, which predominantly utilize video-based teaching methods and are often the only accessible option for students in rural areas. Our findings indicate that telesecondary schools are, in many instances, as effective or even more so than regular public schools for their attendees. When we use the estimated model to analyze the effect of removing the telesecondary option from the choice set, we find that the dropout rate would be substantially higher and average schooling attainment lower without these schools. Model simulations also show that telesecondary schools are also important determinants of *Prospera*-program impacts. Our finding that these schools play an important role in fostering education in Mexico is consistent with the difference-in-difference analysis of (Fabregas and Navarro-Sola, 2023) that found that the expansion of telesecondary schools led to substantial increases in schooling attainments for local students.[11]

Lastly, we also explore how inferences based on our model depend on specification assumptions. As a benchmark, we estimate a simpler value-added model grade-by-grade, without controlling for selection from multiple sources (dropout, school choice, grade retention). Comparing the results to those derived from our richer model, the cumulative program impacts are noticeably smaller. Thus, failing to control for dynamic selection would lead to underestimation of *Prospera's* impact. We identify three potential reasons for downward biases. First, the program causes students at the margin of dropping-out to stay in school longer and failure to control for this changing composition of students would lead to a downward bias in the impact estimates. Second, the simpler model does not allow heterogeneous impacts across different types of schools and, therefore, does not capture that telesecondary schools are particularly effective for *Prospera* beneficiaries. Lastly, the simpler model ignores the negative selection of unobserved types, which also leads to an underestimation of program impacts. We find that a richer modeling framework is required to capture heterogeneous program impacts and to control for multiple sources of selection bias.

The paper develops as follows. Section two briefly describes the Mexican school system and the datasets used in this study. Section three describes the model and section four the estimation approach. Section five presents the empirical results. Section six presents the estimated cumulative *Prospera* program effects. It also performs model simulations where the telesecondary schooling option is removed, examines robustness of program impact estimates to alternative modeling assumptions and explores longer-term program impacts (up to grade 12). Section eight concludes.

[11] A recent paper by Borghesan and Vasey (2024) also studies the effectiveness of Mexican telesecondary schools using a marginal treatment effects (MTE) estimation approach applied to a Roy model and using the same data we analyze but focusing on 7th graders.

2 Background

2.1 Mexican educational system and child-labor laws

The Mexican educational system consists of three levels: primary, secondary, and tertiary education. Formal basic education includes preschool, primary school (grades 1-6), and lower-secondary school (grades 7-9), all of which are compulsory. However, compulsory schooling laws are not well-enforced. Many children dropout before completing grade 9, particularly children from lower-SES families, indigenous backgrounds and rural areas.

Our analysis focuses on public schools. Although *Prospera* beneficiaries may choose which school to attend, in practice, almost all attend public schools (in our data only 0.28 percent of beneficiaries in 6th grade are enrolled in private schools). Public primary and lower secondary schools, as part of "educacion basica," are free of charge. The Secretariat of Public Education (SEP) standardizes curriculum content, which includes Spanish, mathematics, natural sciences, history, geography, art, and physical education.[12] Secondary school is divided into lower-secondary school (grades 7-9) and upper-secondary school (grades 10-12). Lower-secondary school is free and students may follow either a general academic track or a technical track, which has more of a vocational focus. Both tracks are designed to prepare students for further education. There are fewer lower-secondary schools than primary schools and attending lower-secondary schools often requires traveling some distance from home, particularly for children living in more-remote areas. Public schools do not generally provide transportation. Upper-secondary education (grades 10-12) did not become compulsory until 2012. Some upper-secondary schools are affiliated with large public universities, while others are SEP or state-controlled. At the tertiary level, the Mexican educational system has many different programs and degree options.

The Mexican Constitution prohibits child labor for minors under 14 years of age. However, the child-labor laws are not well enforced. 8% of children age 12 report working for pay in the 2010 Mexican census data.[13]

2.2 *ENLACE* test-score data and additional survey data

From 2006 to 2013, the SEP applied the *Evaluación Nacional de Logro Académico en Centros Escolares*, called the *ENLACE*. The test evaluated student performance in mathematics, Spanish and a rotating subject for all third-to-ninth graders at the end of each academic year. The test is directly based on the curriculum (see SEP (2010)) and intended to be an assessment that is

[12] The National Institute for Assessment of Education (INEE) monitored standards during our period of study.

[13] Based on the authors' tabulations.

informative about learning outcomes to SEP and to parents. In primary school and in lower-secondary school, the grades studied in this paper, the test has no bearing on students' GPA or grade progression, so it can be considered to be low stakes. Beginning in 2008, *ENLACE* was also given to students in their final year of upper-secondary school (grade 12).[14] The exams were designed to have a mean of 500 and a standard deviation of 100 in their first year of implementation, and subsequent test years were calibrated to allow measurement of changes in learning over time (see SEP (2010)). The test-completion rate is close to 90%. As described by De Hoyos et al. (2018), 15.1 million students in 136,000 schools took the examination in 2013, the last year the test was applied. In addition to test scores, the ENLACE data (merged with school roster data) also contain information on the age, gender, *Prospera* beneficiary status, whether the child attended the day of the test, school ID, and school type for each student. We examine a cohort of students who were in grade 4 in 2007/08 for whom ENLACE test scores are available for up to six years (i.e. from fourth grades through ninth grades for students who progressed one grade each year).

We link the ENLACE data with survey information on student, parent, and school characteristics that was obtained from a random sample of schools each year. The survey data provide detailed information on parental schooling, monthly family income, home infrastructure, number of siblings, and other household characteristics. The combination of the ENLACE data with the student/parent/school surveys produces an exceptionally rich data set that is representative of Mexican school-age children. The detailed survey information available on housing characteristics is useful for reliably approximating the *Prospera* program eligibility criteria, as will be discussed below.

In addition, we gather geocode information (latitude and longitude) for each primary and secondary school. We use such information to characterize the number of local primary schools (within 5 km radius) and number of local secondary schools (within 10 km radius). We then further calculate the distance (in kilometers) between the primary school and the nearest secondary school of each type. Some further details are provided in appendix A.

2.3 Descriptive statistics

Table 1 shows summary statistics for the students in our sample. The columns show the means and standard deviations for children whose families are *Prospera* beneficiaries or non-beneficiaries. The average age of the children is around 10 and 49% are female. The parents of beneficiary students have much less schooling; 65% of the fathers and 68% of the mothers in beneficiary families have

[14] The ENLACE exams have been used as a means of evaluating educational interventions by several papers (Avitabile and De Hoyos (2018); De Hoyos Navarro et al. (2019); De Hoyos et al. (2017)). Scores on these exams have been shown to have predictive power on important life outcomes including university enrollment and wages (De Hoyos et al. (2018)).

Table 1: Descriptive Statistics

	Prospera Mean	*Prospera* Std	Non-*Prospera* Mean	Non-*Prospera* Std		*Prospera* Mean	*Prospera* Std	Non-*Prospera* Mean	Non-*Prospera* Std
Age (at grade 4)	10.06	0.69	9.88	0.56	Number of household members				
Female	0.49	0.50	0.50	0.50	*≤3 people*	0.21	0.41	0.33	0.47
Schooling cat. (dad)					*4 people*	0.21	0.41	0.27	0.44
Below primary school	0.41	0.49	0.13	0.33	5 people	0.19	0.39	0.15	0.36
Primary school completed	0.24	0.43	0.14	0.34	≥6 people	0.39	0.49	0.24	0.43
Secondary or below	0.26	0.44	0.37	0.48	Number of preschool years				
College or above	0.07	0.25	0.34	0.47	0 year	0.03	0.16	0.02	0.13
Schooling cat. (mom)					*1 year*	0.11	0.31	0.04	0.20
Primary school	0.42	0.49	0.14	0.35	2 years	0.26	0.44	0.24	0.43
Primary school completed	0.26	0.44	0.17	0.38	3 years	0.31	0.46	0.45	0.50
Secondary or below	0.27	0.44	0.36	0.48	4 years	0.30	0.46	0.25	0.43
College or above	0.05	0.21	0.32	0.47	Internet at home	0.11	0.32	0.31	0.46
Working status (dad)					Computer at home	0.17	0.38	0.45	0.50
Part time	0.22	0.41	0.16	0.37	First language				
full time	0.74	0.44	0.80	0.40	*Spanish*	0.90	0.30	0.98	0.14
Working status (mom)					*Indigenous*	0.06	0.24	0.01	0.09
Housework	0.77	0.42	0.55	0.50	*Both*	0.04	0.19	0.01	0.10
Part time	0.13	0.33	0.28	0.45	Region				
Full time	0.09	0.29	0.17	0.37	*North*	0.19	0.40	0.16	0.37
Father at home	0.85	0.36	0.85	0.35	*North-center*	0.33	0.47	0.45	0.50
Mother at home	0.96	0.19	0.98	0.14	*Center*	0.14	0.35	0.18	0.38
Urban dummy	0.44	0.50	0.89	0.31	*South*	0.33	0.47	0.21	0.41
Marginality dummy	0.19	0.39	0.75	0.43					
Obs	50,857		126,246		Obs.	50,857		126,246	

Source: Authors' calculations using ENLACE merged with student- and parent-context questionnaires.

primary school (6 grades) or less in comparison to 27% and 31% for non-beneficiaries. Beneficiary fathers are less likely to work full-time, whereas mothers are less likely to work in the labor market at all. Beneficiary students are slightly more likely to have mothers who do not live at home. The two groups differ substantially in terms of geographic residence; 89% of non-beneficiary students live in urban areas in comparison to 44% for beneficiaries.

Prospera families tend to be larger, with 39% having six or more household members in comparison to 24% for non-beneficiaries. Children from beneficiary families have fewer years of preschool education on average. They are also much less likely to have access to computers or the internet at home. 17% of beneficiary students have computers at home and 11% have access to the internet in comparison to 45% and 31% for non-beneficiaries. In terms of languages spoken at home, 4% of children from beneficiary families speak indigenous languages (sometimes in combination with Spanish) in comparison to 1% of non-beneficiary children. Also, a third of the *Prospera*-beneficiary children live in southern states that are relatively poor compared to less than one fourth for non-beneficiaries.

Figure 1: Mathematics and Spanish test-score distributions (CDFs) by grade and *Prospera* status

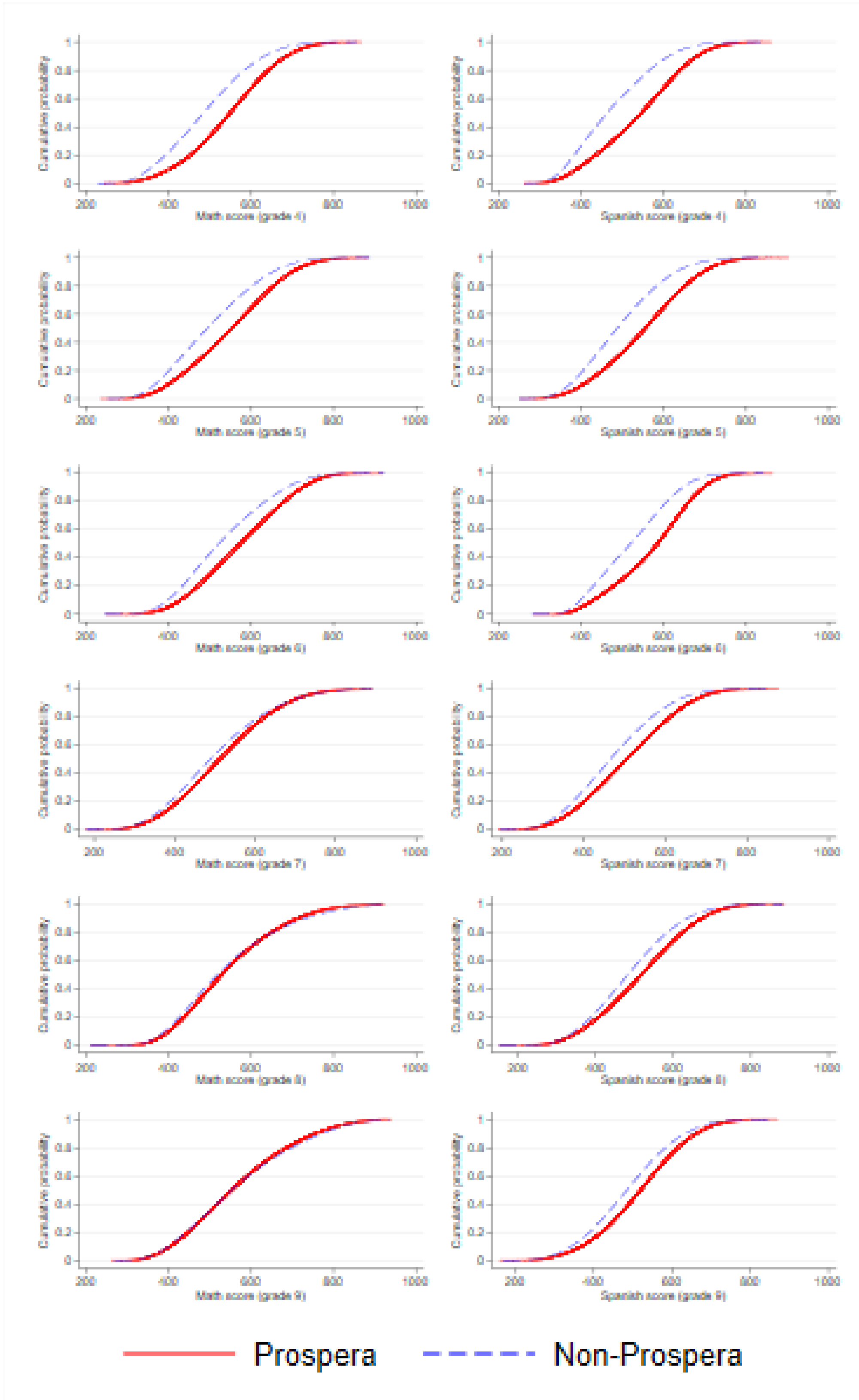


Figure 1 shows the average test scores by grade and by *Prospera*-beneficiary status, where the solid line denotes that the family participates in *Prospera*. In fourth grade, there are considerable gaps in average mathematics and Spanish test scores. Comparing the test-score distributions across grades, we see a pattern of diminishing test-score disparities between *Prospera* and *non-Prospera* students. By grades 8 and 9, *Prospera* and *non-Prospera* students have similar mathematics test-score distributions but there is still a gap for Spanish test scores. The observed narrowing of the gaps may not necessarily reflect relative improvements for *beneficiaries*, however, because it may be due to selection that occurs at various stages, which we next consider.

2.4 Selection problems at different stages of schooling

As described in section one, assessing the educational impacts of the *Prospera* program requires controlling for multiple sources of selection, including selective program participation, grade retention, drop-outs, school choices and cheating. We next present evidence on the empirical relevance of each of these factors.

***Prospera*-program selection.** Households' participation in the *Prospera* program is subject to stringent eligibility criteria that restrict access to high-poverty households. The vast majority of families who are eligible to participate opt to do so, reflecting (i) the transfers that families receive under the program are large, accounting for a 20% increase in family income on average; (ii) the program provides transfers in grades 3-6 when school attendance is nearly universal; and (iii) the program has been in operation since 1997 and is well-known (see Parker and Todd (2017). However, there may still be some nonparticipating families, particularly ones that are unaware of their eligibility. Also, families may receive partial benefits if they send some but not all of their children to school. We consider a student to be in a beneficiary family if they are listed in the *Prospera* administrative data as being enrolled in sixth grade.[15] As was seen in Table 1, the average *Prospera* student is not directly comparable to the average *non-Prospera* student. Beneficiary students come from higher poverty backgrounds and are more likely to live in rural areas.

Grade retention. The presence of grade retention has the potential to alter the student composition within each grade cohort, another potential source of selectivity bias. Table 2 shows the percentage of students retained by grade, school types, and *Prospera*-beneficiary status, where $P = 1$ denotes being a beneficiary, else $P = 0$. It is clear that grade retention is more prevalent in primary-school grades, with notably higher rates for *Prospera* children. For lower-secondary schooling, a

[15] The administrative data on participation in *Prospera*-was linked with the test-score database when children were in grade 6. The overwhelming majority of children enroll in grade 6, although there is a small fraction that drops-out prior to entering grade 6 that we do not include in our analysis sample.

Table 2: Percentages of retained students by school type, grade and *Prospera* status (P)

Primary school	General		Indigenous			
	$P=0$	$P=1$	$P=0$	$P=1$		
4th year	1.2%	2.5%	2.0%	3.2%		
5th year	0.8%	1.5%	1.2%	2.5%		
6th year	0.1%	0.2%	0.0%	0.1%		
Lower-secondary school	**General**		**Telesecondary**		**Technical**	
	$P=0$	$P=1$	$P=0$	$P=1$	$P=0$	$P=1$
1st year	0.6%	0.3%	0.9%	0.4%	0.4%	1.1%
2nd year	0.7%	0.6%	0.3%	0.3%	0.7%	0.7%

Note: this table displays the percentages of students retained by grade, school types, and Prospera-beneficiary status ($P = 1$ denotes *Prospera* participation).

retention rate of approximately 1% is observed, with no discernible pattern by beneficiary status.

Nonrandom school dropout. The Mexican Basic Education system mandates compulsory education from Grades 1 to 9 and Mexican law explicitly prohibits child labor for children under the age of 14. Table 3 shows the grade distribution and school enrollment status by age. As can be seen, conditional on age, children attend a variety of grades. This grade dispersion occurs both because of delayed school enrollments and because of grade retentions. There is a pattern of increasing dropout rates with age as one might expect if the opportunity costs of attending school is to work and older children receive higher wage offers. According to the data, 5% of 13-year-olds and 10% of 14-year-olds are not currently enrolled in school. The dropout rate climbs to 17% for children aged 15. For students who remain enrolled in lower-secondary school (grades 7 to 9) at the ages of 16 and 17, half of them opt to discontinue their education. Given significant observed dropout rates and considering the fact that the *Prospera* provided monetary incentives to stay in school and not drop out, it is essential that our model account for dropout decisions.

School type and choice. In Mexico, families can choose among a few different types of primary and secondary schools, which introduces another source of selection. As previously noted, school choice is more limited in rural areas and, for lower-secondary schooling, some families may only have access to telesecondary schools. Table 4 shows the fractions of *Prospera* beneficiary ($P = 1$) and non-*Prospera* beneficiary ($P = 0$) students attending different types of schools. Only 1% of non-*Prospera* beneficiary children and youth attend indigenous primary schools, whereas 10% of *Prospera* beneficiaries opt for such schools. At the secondary level, the majority of children from non-beneficiary families are enrolled in general schools, whereas about 40% of beneficiary families are enrolled in telesecondary schools. In contrast, only 10% of non-*Prospera beneficiary*

Table 3: Grade and enrollment distribution by age

Age	G4	G5	G6	G7	G8	G9	Dropout	Obs.
9	1.00	-	-	-	-	-	-	35,346
10	0.78	0.22	-	-	-	-	-	157,931
11	0.10	0.71	0.20	-	-	-	-	172,709
12	0.03	0.10	0.68	0.19	-	-	0.01	176,581
13	-	0.03	0.10	0.65	0.18	-	0.05	175,984
14	-	-	0.03	0.09	0.61	0.17	0.10	176,237
15	-	-	-	0.03	0.10	0.70	0.17	141,657
16	-	-	-	0.01	0.15	0.43	0.41	19,026
17	-	-	-	-	0.04	0.42	0.54	3,962

Note: The final column shows the number of observations for each age group. Excluding the last column, the sum of each row's values equals 1.

children/youth attend telesecondary school in grade 7.

The last column of Table 4 reports the cumulative fraction of dropouts at every grade during lower-secondary school. 20% of *Prospera*-beneficiary youth dropout prior to grade 9 in comparison to 16% of non-beneficiaries. The higher dropout rates for the $P = 1$ group could, in part, explain the declining test-score gaps, if dropouts come disproportionately from the lower tails of the test-score distributions. For this reason, it is important to control for selective dropout in evaluating achievement test-score program impacts.

In Figure 2, we compare the test score distributions by grade and by lower-secondary-school types. The left column shows the distributions for the mathematics tests and the right column for the Spanish tests. Each row shows test scores for a different grade, from 6 to 9. There is a larger variance in mathematics test-score performance across school types than in Spanish performance. At grade 6, students who later enroll in telesecondary schools have a notably lower initial mathematics score. However, this gap begins to reverse by grade 7, ultimately resulting in substantially higher mathematics scores for *Prospera* students by grade 9. A comparison of the different school types shows that the general and technical schools consistently have higher Spanish scores than telesecondary schools at grade 6. However, this advantage diminishes as students progress through the grades. These graphs suggest that students attending telesecondary schools tend to demonstrate more significant improvements in test scores across grades compared to students in other school types. However, these comparisons do not imply causation, as they do not account for the compositional differences among students in different school types and the different dropout rates across grades and school types. Our model will control for these different sources of selectivity.

Cheating behavior. A potential concern in any assessment of student test scores is student

Table 4: Enrollment distribution by school type, grade and *Prospera* status (P)

Primary school	General	Indigenous		
Beneficiary (P=1)				
Grade 4	0.89	0.11		
Grade 5	0.89	0.11		
Grade 6	0.89	0.11		
Non beneficiary (P=0)				
Grade 4	0.99	0.01		
Grade 5	0.99	0.01		
Grade 6	0.99	0.01		
Secondary school	General	Telesecondary	Technical	Dropout
Beneficiary (P=1)				
Grade 7	0.26	0.44	0.21	0.09
Grade 8	0.24	0.41	0.20	0.15
Grade 9	0.23	0.39	0.18	0.20
Non beneficiary (P=0)				
Grade 7	0.52	0.10	0.32	0.06
Grade 8	0.50	0.09	0.31	0.11
Grade 9	0.47	0.08	0.29	0.16

Note: This table displays the school-type enrollment distribution by beneficiary status and grades, where $P = 1$ denotes *Prospera* participation. The dropout only matters for lower-secondary school and is measured prior to entering that grade and is cumulative. Each row totals add to 1.

cheating. Most studies ignore the issue of cheating, because researchers commonly lack access to information on who may have cheated. However, SEP employs a statistical algorithm designed to flag potential instances of cheating in the form of copying on the ENLACE exams. This information is integrated into our test-score database.[16]

Table 5 presents the impact of potential cheating behavior on test scores. When comparing students identified by the cheating indicator with those who did not, average test scores in both mathematics and Spanish are higher for those flagged as cheating. The average test-score disparities are more pronounced in secondary schools, suggesting that gains from cheating might be greater for older children. A comparison between non-*Prospera* students and *Prospera* students reveals that cheating behavior is more prevalent among the *Prospera* students. Moreover, cheating is generally associated with less test score inflation among non-*Prospera* than *Prospera* students.

[16] In particular, SEP estimates a student's probability of cheating using both the K-index and the Scrutiny method. If the software detects a possible cheating case from the response patterns, a note is added to the student's report (Gonzalez, 2015). Note that the exam score is not deleted, and there are no consequences to the students. (SEP, 2010).

Figure 2: Mathematics and Spanish test scores distributions by grade and lower-secondary school types

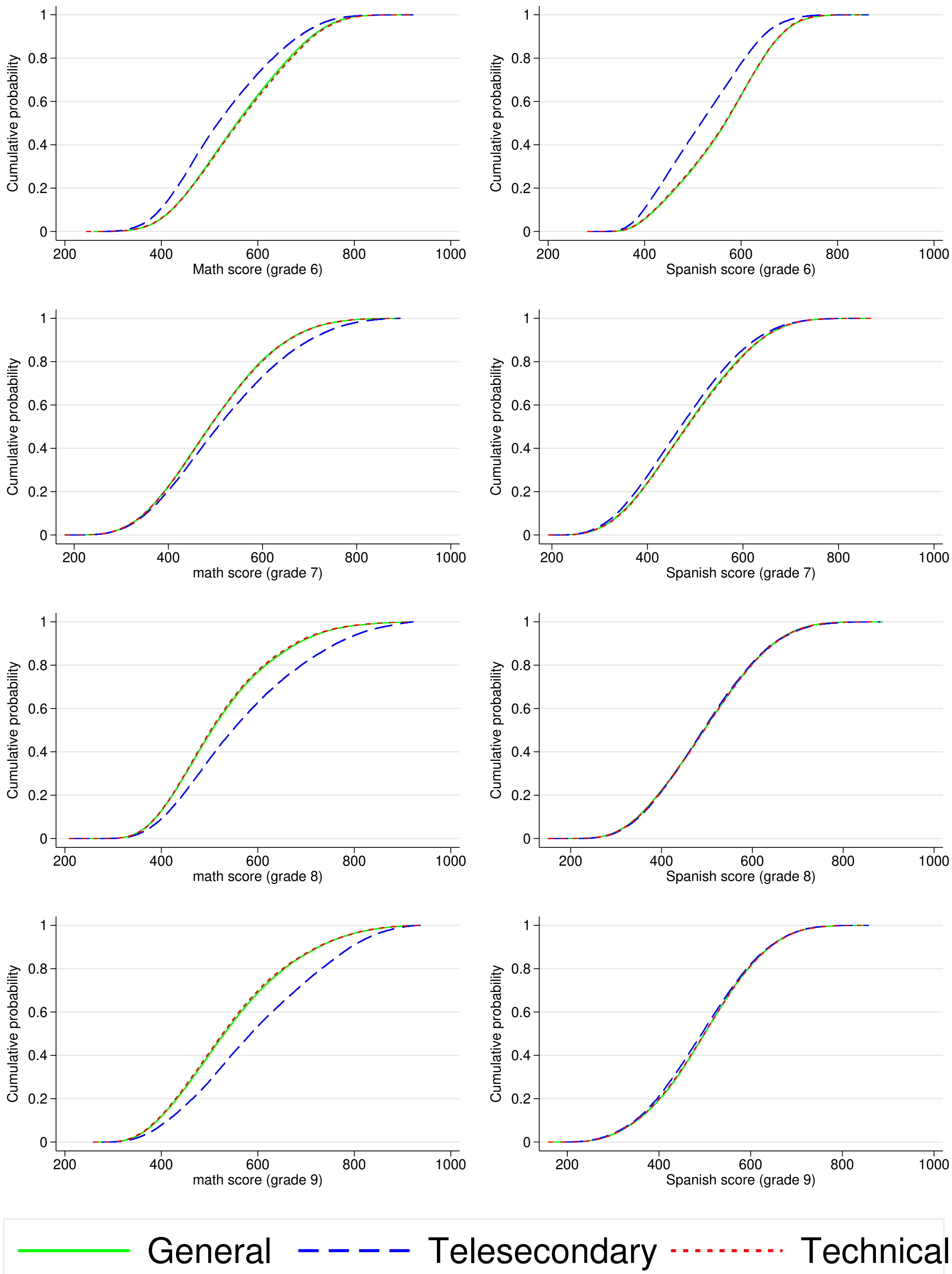


Table 5: Average test scores and proportion cheating, by *Prospera* status

		Non Prospera		*Prospera*	
Grades		No cheating	Cheating	No cheating	Cheating
Grade 4	Fraction	93.8%	6.2%	90.1%	9.9%
	Math	529	554	480	519
	Spanish	521	538	468	496
Grade 5	Fraction	96.6%	3.4%	94.6%	5.4%
	Math	533	585	494	557
	Spanish	534	568	488	533
Grade 6	Fraction	96.9%	3.1%	95.1%	4.9%
	Math	560	616	525	594
	Spanish	557	590	515	561
Grade 7	Fraction	98.2%	1.8%	96.5%	3.5%
	Math	500	578	492	602
	Spanish	490	542	465	534
Grade 8	Fraction	96.2%	3.8%	92.7%	7.3%
	Math	524	627	526	683
	Spanish	497	567	476	579
Grade 9	Fraction	97.2%	2.8%	95.1%	4.9%
	Math	548	636	562	654
	Spanish	501	541	481	532

2.5 Local school supply and quality

As previously noted, Mexican families can choose among different types of schools, but their options depend on the local supplies. We next examine the supplies of different types of schools and also their quality characteristics. Table 6 provides information on the supplies of local schools of different types at the individual level. In Mexico, multiple school sessions are often held in the same building, such as a morning and afternoon session. The different sessions may have different principals and teachers, so in the dataset they are considered to be different schools.[17] Primary schools tend to be small, with an average enrollment of less than 200, and, consequently, there are a large number of primary schools. Their small size partly reflects that the school systems do not usually provide transportation and students typically walk to school. Also, 77% of children do not have access to indigenous schools, which are typically located in areas with significant indigenous populations. At the lower-secondary level, there are fewer schools and they are larger.

Table 7 compares the different school types in terms of some average school quality characteristics, including pupil-teacher ratios and teacher educational levels. The first two columns show

[17] In Appendix Figure B1, we show one illustrative example of local primary-school sessions in Aguascalientes, a city in central Mexico. It has 316 school sessions distributed in 250 unique coordinates within 10 kilometers.

Table 6: Number of local schools of different types

	Mean	Std	p10	p50	p75	Not available
Primary school (within 5 km)						
General	65	80	9	26	104	3.4%
Indigenous	5	8	1	3	6	76.9%
Secondary school (within 10 km)						
General	48	69	4	18	63	14.6%
Telesecondary	13	12	4	10	20	8.4%
Technical	10	12	2	6	16	15.9%

Note: Columns 3-5 report selected percentiles. The last column gives the percentages of individuals for whom a given school type are not locally available.

characteristics for general and indigenous primary schools. Indigenous primary schools have on average 94 students in comparison to 174 students in general primary schools. The percentages of students who are disabled ranges from 1-2%. Despite having overall fewer students, the student-teacher ratio in indigenous schools is higher - 33 in comparison to 24. Another difference is that teachers in indigenous schools are more likely to have only upper-secondary school degrees (17% in comparison to 3%). At the same time, the fraction of teachers with an undergraduate or higher degree is 7 percentage points higher. Thus, teacher schooling attainment exhibits higher variance in indigenous schools.

The last three columns of Table 7 compare the average school characteristics for general, technical and telesecondary schools. Technical schools tend to be larger, with an average enrollment of 395 in comparison to 296 for general schools and 75 for telesecondary. Again, the proportion of disabled students across all types of schools is 1-2%. The student-teacher ratio is 14 in general schools, 19 in technical schools and 24 in telesecondary schools.[18] Thus, we see a general pattern of the smaller schools in rural areas having higher student-teacher ratios, which could either reflect that video learning is less teacher-intensive or that teacher or resource shortages are more common in rural areas. Comparing teacher educational profiles across the different kinds of secondary schools, we see that average characteristics are fairly similar. The main difference is that general-school teachers are more likely to have undergraduate degrees rather than teaching-college degrees, compared to teachers in technical and telesecondary schools.

[18]These tabulations are based on regular teachers and exclude art and music teachers who often teach at multiple schools.

Table 7: Mean primary and lower-secondary school characteristics by school types (with standard deviations in parentheses)

	Primary		Lower Secondary		
Characteristic	General	Indigenous	General	Telesecondary	Technical
Number of students	174	94	296	75	395
	(175)	(95)	(244)	(64)	(247)
Proportion disabled	0.02	0.01	0.01	0.01	0.02
	(0.06)	(0.07)	(0.04)	(0.03)	(0.05)
Student-teacher ratio	24	33	14	24	19
	(18)	(12)	(7)	(9)	(8)
Teachers with HS degree	0.03	0.17	0.03	0.03	0.02
	(0.09)	(0.30)	(0.07)	(0.16)	(0.06)
Teachers with teacher college	0.47	0.26	0.34	0.41	0.42
	(0.35)	(0.34)	(0.34)	(0.42)	(0.32)
Teachers with undergraduate degree	0.47	0.56	0.54	0.44	0.47
	(0.34)	(0.39)	(0.34)	(0.42)	(0.32)
Teachers with post-grad degree	0.03	0.01	0.07	0.11	0.08
	(0.10)	(0.07)	(0.12)	(0.23)	(0.11)

Note: Tabulations based on a school census dataset called the 911 data.

3 Model

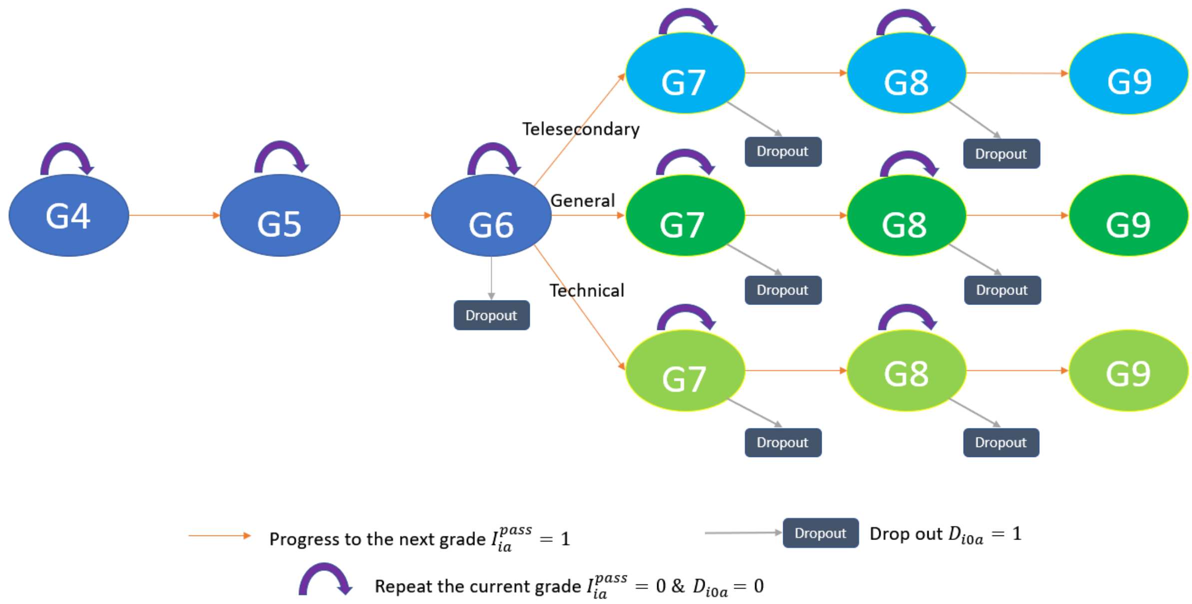
Our modeling framework combines a school-choice model of attendance decisions at different types of schools with models of academic achievement in mathematics and Spanish that are linked across ages/grades. In particular, we specify test-score gains from year-to-year using a value-added framework that relates current achievement to lagged achievement, family and school inputs into the learning process, and student unobserved heterogeneity (e.g. arising from ability or preferences). Our framework also allows technology for producing test-score gains to vary by type of school. It also incorporates drop-out decisions and allows for grade retention, as described below.

Our modeling framework can be considered quasi-structural. The educational production function has a structural interpretation as a technology relating inputs to outputs. However, the school-choice model is reduced form, likely reflecting the decisions of students, parents, and school administrators. As discussed below, the outside option in the school-choice model is to drop out.

3.1 General environment and sequential outcomes

Individuals are indexed by i, $i = 1, .., n$ and each model period corresponds to one school year. In the initial period (a_f, corresponding to the age at grade 4), students/parents can choose to attend

Figure 3: Potential sequential outcomes from grade 4 to 9



one of two types of primary schools: general ($j = 1$) or indigenous (bilingual) ($j = 4$), depending on the types locally available (within 5 km). At the end of grade 6, students simultaneously make school-enrollment decisions (with $j = 0$ indicating non-enrollment) and school-type choices from up to three options: general ($j = 1$), telesecondary ($j = 2$), or technical ($j = 3$), depending on the types locally available (within 10 km). We can summarize the choice set J_{ia}^{g} at different grades as:

$$j_{ia} \in J_{ia}^{g} = \begin{cases} \{1,4\} \cap M_i^1 & G_a = 4 \ \& \ a = a_f \\ \{0,1,2,3\} \cap M_i^2 & G_{a-1} = 6 \ \& \ I_{i,a-1}^{Pass} = 1 \\ \{0, j_{i,a-1}\} & G_{a-1} \geq 7 \end{cases} \tag{1}$$

where a_f is the age when the student enters into the sample at grade 4. M_i^1 denotes the available local primary-school types and M_i^2 denotes the available local lower-secondary school types.[19]

Let $D_{ija} = 1$ if the individual i is enrolled in school type j ($j \in \{1, 2, 3, 4\}$) at age a, else $D_{ija} = 0$. Let $D_{i0a} = 1$ if the individual does not enroll in school at age a, else $D_{i0a} = 0$. Let $I_{ia}^{Pass} = 1$ if the individual passes the grade in which she is enrolled at age a, else $I_{ia}^{Pass} = 0$. We assume the passing outcome I_{ia}^{Pass} is realized at the end of the school year, prior to the other decisions being made. The potential sequential outcomes from grade 4 to grade 9 are illustrated in figure 3:

As seen in figure 3, until grade 6 the possible outcomes are whether a student is retained in the current grade ($I_{ia}^{Pass} = 0$) or progresses to the next grade ($I_{ia}^{Pass} = 1$), conditioning on their primary

[19]In particular, $M_i^1 \in \{\{1\}, \{4\}, \{1, 4\}\}$ and $M_i^2 \in \{\{1, 2, 3\}, \{1, 2\}, \{1, 3\}, \{2, 3\}, \{1\}, \{2\}, \{3\}\}$.

school types when entering into the model at grade 4, their family background and their *Prospera*-beneficiary status. Upon passing grade 6 ($I_{ia}^{Pass} = 1$), students simultaneously make enrollment decisions D_{i0a} and school choices D_{ija}, depending on locally available school types.[20] Once enrolled in a secondary school, students decide in each period whether to drop out or to stay in school. In each grade, there is a probability of passing or having to repeat the grade. Let G_{ia} denote the grade that the individual is eligible to attend at age a, which increases by one if the student passes the current grade:

$$G_{i,a+1} = G_{ia} + I_{ia}^{Pass}.$$

3.2 Accounting for selective program participation

Our aim is to use our estimated model to assess the impacts of *Prospera* participation on schooling progression and academic achievement, where we treat *Prospera* beneficiary status as a family characteristic. $P_i = 1$ denotes that a child/youth comes from a *Prospera*-beneficiary family, else $P_i = 0$. Although the survey data we use were not collected for the purpose of ascertaining Prospera eligibility, the data are rich and contain information on most of the eligibility determinants.[21] Program eligibility is not means-tested by income, because income can be difficult to measure in a country with a significant informal sector and where many low-income individuals are engaged in agricultural work. The program-eligibility criteria rather depend mainly on households' assets (such as car ownership), on characteristics of the household's residence (such as whether it has dirt floors, piped water and how many rooms there are per person living in the house) and on household demographics, such as number of children and numbers of dependents per worker.[22] The vast majority of eligible households opt to participate, reflecting that the cash transfers are substantial.[23]

Our empirical strategy in evaluating the impact of *Prospera* on student test scores and educational progression is to first limit the comparison group subsample to children whose families meet at least a subset of the eligibility criteria. We do so by first estimating a probit model for the probability that each family is eligible for and participates in the *Prospera* program given the available information. That is, we use information on housing characteristics and demographics that was gathered through the student and parent surveys to estimate a household's probability

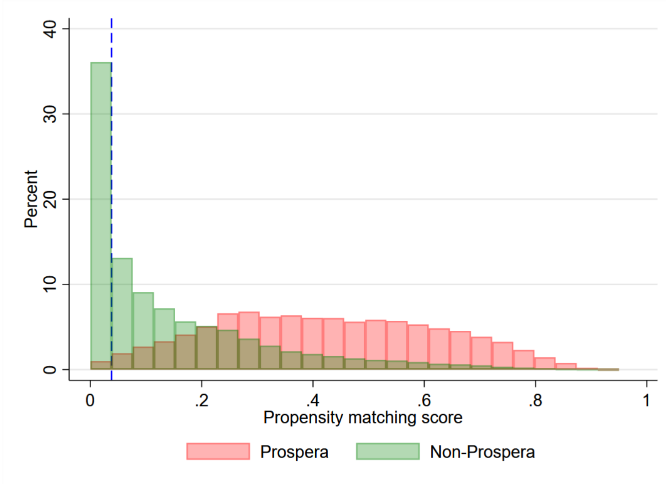
[20]Because school enrollment is very high during primary school, we assume students do not drop-out during primary grades. Therefore, they do not make choices about continuing in school until the end of grade 6.

[21]The precise eligibility criteria are not made public, but some of the authors of this paper were involved in the design of the criteria.

[22]Families who apply to the program typically fill out a questionnaire to determine their eligibility and their answers on the questionnarie may be checked through home visits.

[23]As discussed in Parker and Todd (2017) they represent on average about a 20% increase in household income.

Figure 4: The propensity score distribution by *Prospera* status (P)



Note: The red histogram represents the propensity score distribution for *Prospera* children/youth and the green histogram for *Non-Prospera* children/youth.

of being eligible and participating in the program (a propensity score). The estimated coefficients from this probit regression are shown in Appendix A.3. The percentage correctly classified as being beneficiaries or not under the estimated model is high (90%).

Figure 4 plots the propensity-score distributions for children from *Prospera*-beneficiary households (in red) and non-beneficiary households (in green). As seen in the figure, a large fraction of non-beneficiaries fall in the first histogram bin, meaning that they have extremely low probabilities of participating in *Prospera*, generally because their characteristics make them ineligible. To increase comparability between the *Prospera* and the comparison-group subsamples, we impose a common support restriction and exclude in our impact analysis *Prospera* beneficiaries and the non-beneficiaries with propensity scores below the 1% quantile (the lowest bin of the histogram).This threshold excludes 383 children from *Prospera* families and 50,798 non-beneficiary children.[24]

In addition, our school-choice/dropout and value-added models also include observed covariates to further control for differences between *Prospera* and non-*Prospera* households (such as parents' schooling attainment). We also allow for the possibility that children/youth from *Prospera* beneficiary families may differ in unobserved ways by including latent unobserved heterogeneity, specified as four discrete multinomial types that enter into all the model equations.[25] Let $\mu_{il} = 1$ denote that

[24]This type of trimming is common in the application of matching estimators as a way of imposing "common support." Heckman et al. (1997) showed, in the context of evaluating a job-training program, that having a highly comparable comparison group is important to producing reliable non-experimental impact estimates that replicate experimental estimates.

[25]See, e.g. Heckman and Singer (1984) and Cunha and Heckman (2008). Alternatively, we could impose a continuous distribution for the unobserved heterogeneity, for example, a mixture of normal distributions. Mroz (1999) shows the discrete-type assumption performs as well as the normal assumption when the true distribution is

individual i is of type l, =0 else, where $l \in \{1, ..., L\}$ and $L = 4$.

Ideally, one could allow for the unobserved types to be arbitrarily correlated with the observed variables. However, identifying such a model poses challenges, as it can be difficult to distinguish the direct effects of these observed variables from their indirect effects operating through the unobserved type distribution. For this reason, we restrict the type probability distribution to depend only on a child's *Prospera* status ($P \in \{0, 1\}$) and a binary marginality indicator ($M \in \{0, 1\}$), which is a measure of the poverty level in the locality where the household lives. We denote the conditional type probability as $\rho_l(P, M) \equiv \Pr(\mu_{il} = 1|P, M)$; it represents the fraction of type l among students with Prospera status P and marginality index M. Note that $\sum_l \rho_l(P, M) = 1, P \in \{0, 1\}, M \in \{0, 1\}$.

3.3 The model

As described in section 2.4, our modeling and estimation approach is designed to address several statistical challenges that arise in evaluating the *Prospera* program academic-achievement impacts. First, we address the problem of selective program participation by restricting our analysis sample to children estimated to have a positive probability of participating in the program.[26] Second, our school-attendance and school-choice framework, which models the sequential choices shown in figure 3, explicitly addresses dynamic selection in school choices and dropout decisions. These decisions are permitted to depend both on observed family background characteristics as well as on unobserved factors that are assumed to follow a multinomial distribution (i.e. discrete types). We also incorporate exogenous variables, such as imputed local hourly wages, distances to the nearest school of each type, and the number of local schools of each type, as exclusion restrictions that affect school-type choices and dropout decisions but do not enter the test score outcome equations directly. Third, our model explicitly accounts for grade retention to capture that children may be observed multiple times in the same grade. Fourth, as described later in section 4.1, we address potential test-score distortions caused by a small fraction of students suspected of cheating (copying).

We next describe our multi-equation model of academic achievement over multiple grade levels, which includes the following components: value-added models in each grade, the school-choice/dropout models in primary school and at the start of secondary school, and the grade-repetition process.

Value-added model: Achievements in mathematics and Spanish evolve over time with school attendance. Let $m = 1$ denote mathematics, $m = 2$ Spanish and g denote the grade level. The

normal. When the true distribution is not normal, however, he finds that the discrete-type method performs better.

[26]The value of imposing common support on the propensity score distribution in the context of social program evaluation is emphasized in Heckman et al. (1997).

value-added model is grade-specific and school-type (j) specific. It also depends on whether the student passed the previous grade $I_{i,a-1}^{Pass} = 1$ or is repeating the grade $I_{i,a-1}^{Pass} = 0$.[27] Let Z_{ia}^{A} denote the vector of observed characteristics of the youth and of the family that enter the achievement production function.

$$A_{ia}^{m} = \delta_{0jl}^{mgI} + A_{i,a-1}\delta_{1j}^{gI} + \delta_{2j}^{mgI} P_i + Z_{ia}^{A}\delta_{3j}^{mgI} + \omega_{ija}^{mgI}. \tag{2}$$

In this equation, δ_{0jl}^{mgI} is the type-specific intercept that allows for unobserved heterogeneity (l denotes the type). $A_{i,a-1} = \left\{A_{i,a-1}^{1}, A_{i,a-1}^{2}\right\}$ is a 2×1 vector including both the mathematics score and the Spanish score from the previous period $a-1$. The lagged test-score terms are assumed to be sufficient statistics for the impacts of past inputs in the learning process. This specification allows for cross effects between Spanish and mathematics. For example, better Spanish skills may enhance student's understanding in their mathematics classes, implying a positive effect of past Spanish scores on current mathematics scores. δ_{2j}^{mgI} captures the impact of the *Prospera* program. We assume that the error terms ω_{ija}^{mgI}, conditional on the unobserved types, are i.i.d. and normally distributed. Most of the literature considers learning technology to be exogenous. By combining a school-choice model with value-added models that vary by school type, we allow students/parents to select from different available learning technologies.

School-choice model: We next specify how individuals make their schooling choice D_{ija} from the available options, J_{ia}^{g}(depending on his/her grade and geographic location, defined in equation 1). Assuming a random-utility model with Type I extreme-value errors (taste heterogeneity) yields a multinomial logistic model for the probability of choosing option j_{ia}:

$$\Pr(D_{ija} = 1|\tilde{\Omega}(a), \mu_l) = \begin{cases} \dfrac{\exp\left(\mu_{0jl}^{g} + A_{ia-1}\phi_{1j}^{g} + P_i\phi_{2j}^{g} + f_j(Z_{ia}^{D}, w_{ia}, S_{ija}^{\text{distance}}, S_{ija}^{\text{number}})\right)}{\sum_{j' \in J_a^g} \exp\left(\mu_{0j'l}^{g} + A_{ia-1}\phi_{1j'}^{g} + P_i\phi_{2j'}^{g} + f_{j'}(Z_{ia}^{D}, w_{ia}, S_{ij'a}^{\text{distance}}, S_{ij'a}^{\text{number}})\right)} & \text{if } j_{ia} \in J_a^g \\ 0 & \text{if } j_{ia} \notin J_a^g \end{cases} \tag{3}$$

where μ_{0jl}^{g} is a type-specific intercept (l denotes the type). ϕ_{1j}^{g} captures the effect of test scores on schooling choices. ϕ_{2j}^{g} captures the impact of *Prospera* on schooling choices. $Z_{ia}^{D} \in \tilde{\Omega}(a)$ includes demographic and family-background characteristics. There are three additional variables that enter the school-choice equations but not other parts of the model: (i) the imputed hourly wage w_{ia}; (ii) the distance to the closest school of each type $S_{ija}^{\text{distance}}$; and (iii) the local supply of schools of each type S_{ija}^{number}.

[27] If a student repeats a grade, then the lagged test score pertains to the same grade as in the current time period and would therefore have a different associated coefficient from the case where the lag pertains to the previous grade.

As previously noted, the outside option in our school-choice model is to drop out of school, which could be a more attractive option in areas that pay higher wages to child labor. Due to the absence of wage information in our test-score databases, we rely on data from the 2010 Mexican census to impute wages for individuals based on characteristics such as their age, gender, schooling level, and geographical region of residence. Additionally, our imputation procedure incorporates a selection correction mechanism to account for selective labor-force participation.[28] The imputed wage, denoted as w_{ia}, represents the opportunity costs associated with being enrolled in school. Two other important variables in the school-choice model are the distance to the nearest school ($S_{ija}^{\text{distance}}$) and the log number of local schools of various types (S_{ija}^{number}). These variables are included to capture the effect of local school availability on individuals' schooling decisions.

We assume that the three exogenous variables $\{w_{ia}, S_{ija}^{\text{distance}}, S_{ija}^{\text{number}}\}$ affect school-choice decisions but do not directly enter the test-score equations (i.e. exclusion restrictions). These variables are helpful to identify separately the parameters in the value-added models from parameters in the school-choice/dropout model.[29] However, the exclusion restriction could be invalid, for example, if higher wages provide incentives for students to work part-time while enrolled in school, which directly affected their test-score performance. Another potential threat to validity is that the travel distance may directly affect the commuting time required to attend schools. Both channels may negatively impact academic performance through fatigue or reduced ability to concentrate on studying. To examine the empirical relevance of such concerns, we also estimated a specification in which the variables $\{w_{ia}, S_{ija}^{\text{distance}}, S_{ija}^{\text{number}}\}$ are added to the value-added equation . We examined (i) whether the coefficients associated with *Prospera*-beneficiary status change and (ii) whether the estimated coefficients associated with the variables $\{w_{ia}, S_{ija}^{\text{distance}}, S_{ija}^{\text{number}}$ are significantly different from 0. We did not find evidence of direct impacts of $\{w_{ia}, S_{ija}^{\text{distance}}, S_{ija}^{\text{number}}\}$ on test scores, conditional on the other model covariates.

As we will elucidate in the section 3.5, one way of interpreting our school-choice probability equation is an an approximation to a policy function derived from a full dynamic version of our structural model (for related discussion see, e.g., Heckman et al. (2016)). To account for potential nonlinear effects of the state variables on decision-making, we adopt a flexible functional form, denoted as $f(Z_{ia}^D, w_{ia}, S_{ija}^{\text{distance}}, S_{ija}^{\text{number}})$.[30]

Grade retention: Lastly, we specify a probabilistic model for whether a student passes a grade,

[28] For a more detailed explanation, please refer to the appendix A.2.

[29] In a parametric setting, exclusion restrictions are not strictly required. Nonetheless, independent variation in the determinants of dropout and school-choice decisions provides additional sources of identification that do not rely on functional form restrictions.

[30] In Appendix D, we describe how we use Bayesian Information Criterion(BIC) to determine our final econometric specification.

which depends on the unobserved type μ_l, the current school-type attended j_{ia}, academic knowledge as proxied by the achievement scores A_{ia}, the grade level G_{ia}, *Prospera* beneficiary status P_i as well as some demographic and family-background characteristics, $Z^I_{ia} \in \tilde{\Omega}(a)$:

$$Pr(I^{Pass}_{ia} = 1|\tilde{\Omega}(a), \mu_l) = \Phi(\gamma^g_{0l} + A_{ia}\gamma^g_1 + \gamma^g_2 P_i + j_{ia}\gamma^g_3 + Z^I_{ia}\gamma^g_4). \quad (4)$$

The coefficients of the passing probability probit model are grade-specific andγ^g_{0l} is a unobserved type-specific intercept.

3.4 Treatment-effect heterogeneity

Prospera may have heterogeneous impacts for students from different backgrounds. Inspired by the marginal-treatment-effect (MTE) literature, we divide the analysis sample into quartiles based on the *Prospera*-eligible propensity scores and allow the *Prospera* impact to vary by quartile.[31] Families in the highest quartile - i.e. with characteristics that make them most likely to be eligible for *Prospera* - tend to be the most disadvantaged. Figure 5 shows the distribution of *Prospera*-beneficiary children across quartiles. 83% of *Prospera* children/youth are concentrated in quartiles 3 and 4 (30% and 53%), reflecting the fact that the program is targeted at the poorest families.

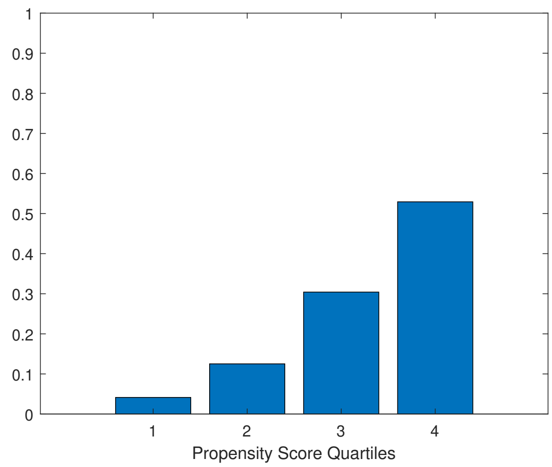
Additionally, we also consider the possibility that the treatment effects vary by gender. Girls receive a higher conditional-cash-transfer subsidy than boys in secondary-school grades (see, e.g. (Parker and Todd, 2017)), so it is reasonable to explore whether *Prospera* impacts on academic achievement and attainment differ for girls and boys. We therefore allow, in estimation, for the coefficients associated with *Prospera* to vary by both gender and propensity-score quartiles. Specifically, we redefine the coefficients $\left\{\delta^{mgI}_{2j}, \phi^g_{2j}, \gamma^g_2\right\}$ to be $\left\{\delta^{mgI}_{2js\eta}, \phi^g_{2js\eta}, \gamma^g_{2s\eta}\right\}$, where $s = \{f, m\}$ denotes female and male, and $\eta = \{1, 2, 3, 4\}$ represents the quartile to which each individual belongs. As reported below, the most-disadvantaged children and youth experience substantially larger effects and there is evidence of heterogeneous impacts by gender.

3.5 Approximate decision-rule interpretation

As previously noted, the value-added achievement function has a structural interpretation as a production-function technology. Our school-choice model, shown in equation 3, can be interpreted

[31] We capture potential heterogeneity more parsimoniously than a standard approach in the literature, which is to estimate the treatment effect nonparametrically as a function of the propensity-matching score. (e.g. Heckman and Vytlacil (2001, 2005, 2007)) However, most literature implements MTE in a static set-up whereas our model is dynamic.

Figure 5: Distribution of *Prospera* students across propensity-score quartiles



as an approximation to a decision rule derived from a dynamic discrete-choice decision model. In a dynamic model, students' schooling decisions are age/grade-dependent and their educational choices in early grades affect their school attendance options in later grades.[32] The threshold-crossing indices associated with the school-choice probabilities at each age represent the differences between the value functions of alternative schooling-work options.

For example, consider students' choices at the end of grade 6 about whether to continue going to secondary school and, if so, which type of school to attend. The value function associated with each choice consists of the flow utility plus the expected future utility (the so-called Emax), which is typically a non-linear function of the state variables (see equation 3). We can approximate the value function using a flexible polynomial power-series. However, there are a large number of state variables and interaction terms that could potentially be included in such approximation. As described in Appendix D, we use a Bayesian Information Criterion (BIC) in selecting our final model specification, which approximates the value function of individual i choosing option j at age a as:

$$\begin{aligned} V^*_{ija} = \mu^g_{0jl} + A_{ia-1}\phi^g_{Aj} + P_i\phi^g_{pj} + Z^D_{ia}\phi^{1g}_{Zj} + w_{ia}\phi^{1g}_{wj} + S_{ija}\phi^{1g}_{Sj} + w^2_{ia}\phi^{2g}_{wj} + S^2_{ija}\phi^{2g}_{Sj} \\ +\phi^{1g}_{Ij} \times \text{Region}_i \times \text{Urban}_i + \phi^{2g}_{Ij} \times \text{Region}_i \times S^{distance}_{ija} + \phi^{3g}_{Ij} \times \text{Region}_i \times S^{number}_{ija} \\ +\phi^{4g}_{Ij} \times \text{Region}_i \times w_{ia} + \phi^{5g}_{Ij} \times \text{Urban}_i \times w_{ia} + \epsilon_{ija} \end{aligned} \tag{5}$$

The coefficients $\{\phi^{1g}_{Ij}, \phi^{2g}_{Ij}, \phi^{3g}_{Ij}, \phi^{4g}_{Ij}, \phi^{5g}_{Ij}\}$ are associated with the nonlinear effects arising from the

[32]Todd and Wolpin (2006) and Attanasio et al. (2012) estimate structural schooling models to analyze the effects of the *PROGRESA* program on enrollment. Leite et al. (2011) develop a model to evaluate impacts of the Brazilian *Bolsa Escola* CCT program. Those models consider school-enrollment/working decisions but do not consider the choice of school type or academic achievement. For discussion of how to approximate the decision rules, see, e.g. Keane et al. (2011); Heckman and Navarro (2007); Heckman et al. (2018).

interaction terms among variables $\{\text{Region}_i, \text{Urban}_i, S_{ija}^{distance}, S_{ija}^{number}, w_{ia}\}$. We considered also interaction terms between Z_{ia}^D and other state variables, but these terms were not selected by the BIC criterion.

As previously noted, the variables $\left\{S_{ija}^{distance}, S_{ija}^{number}, w_{ia}\right\}$ constitute exclusion restrictions, which enter the school-enrollment/school-choice equations but not the test-score equations. Our specification allows the effects of the exclusion restrictions to vary across different regions and between urban and rural areas. For example, the same commuting distance may imply different commuting costs due to the availability of different local-transportation options. We include interaction terms to capture these geographic sources of heterogeneity.

There are a few advantages of adopting this kind of "quasi-structural" approach vis-à-vis estimating a fully structural dynamic model.[33] First, if multiple individuals are involved in the school-choice decisions (students, parents, school administrators), then our approach avoids having to model the decision processes of all of these agents and how they interact. Second, estimating a structural dynamic schooling model with academic achievement is complicated, because the state space would include two continuous lagged test scores, which usually necessitates the use of Emax approximation methods. Lastly, our model specification allows for considerable flexibility in how variables affect decision-making at different ages. In particular, it allows for heterogeneous impacts of local wages and school-type availability on school-enrollment and school-choice decisions.

A limitation of this type of quasi-structural approach, however, is that the model does not distinguish between current and future utility components and therefore cannot be used to study effects of policies that might be implemented in the future, such as the effects of cash transfers given only upon completing certain future grades. This limits the range of counterfactual policies that can be considered. In the analysis reported below, we consider the effects of two time-invariant policy changes, eliminating *Prospera* and removing telesecondary schools from the choice set.

3.6 Identification

In our model for primary education, families make a one-time binary decision about what type of school the child attends, from up to two kinds of schools (general or indigenous). Many families do not live near indigenous schools, so those parents do not have any choice to make. Upon completing primary school, students/parents face a one-time multinomial choice: they can either drop out to work or enroll in one of three different types of schools (general, technical or telesecondary). In grades 7 and 8, they only make the binary decision about whether to drop out or continue in the

[33] See, also, related discussion in Heckman et al. (2016)

same type of school. Below, we discuss identification of the model parameters at these different stages. Our model is parametric and model parameters are estimated via maximum likelihood. Standard identification arguments for parametric models therefore apply, namely, that the model parameters are identified as long as the Hessian matrix exists and is invertible (See, e.g, Amemiya (1985)). In our discussion below, though, we consider whether model parameters are identified under weaker functional form assumptions.

3.6.1 Identification of model parameters pertaining to primary-school choices

Manski (1988) considers semiparametric identification of binary-choice index models in static and dynamic settings.[34] He showed that binary-choice index-model parameters are identified (up to scale) under an assumption that unobserved variables are fully independent of the regressors or under a weaker quantile (e.g. median) independence assumption.[35] His identification arguments apply to our primary-school-choice model and to our probabilistic grade-retention equation.

At the end of grade 6, students make a decision about what type of secondary school to enter or whether to drop out, with the decision being observed for everyone. Our multinomial-logistic model with discrete unobserved types falls within the more general class of mixed logit models. Fox et al. (2012) show nonparametric identification in such models.

3.6.2 Identification of model parameters pertaining to lower-secondary school grades

Students face the decision to drop out at the end of 7th and 8th grades, with this choice only being visible for those who have not dropped out in earlier grades, which is essentially a dynamic selection process. Heckman and Navarro (2007) establish semiparametric identification of a class of dynamic discrete-choice models and they consider a model of optimal stopping time of schooling as their running example.[36] Their framework allows for general forms of duration dependence and unobserved heterogeneity. Let γ^t denote the parameters of the period t index equations and let F_ν^t denote the distribution of the period t unobserved shocks. As discussed in Heckman and Navarro (2007), the set (γ^t, F_ν^t) is identified relative to all other sets of parameters $(\gamma^{*t}, F_\nu^{*t})$ if there exists a sequence of past choices such that the probability of observing a choice under the true parameters differs from that under alternative parameter values.[37]. Their identification proof

[34] Also, see Heckman et al. (2016), Heckman et al. (2018) for other applications and extensions.

[35] Honoré and Kyriazidou (2000) extend the identification results to a binary-choice panel-data model with lagged dependent variables and individual fixed effects.

[36] They call this model a time-to-treatment model, where the treatment is stopping school.

[37] Their theorem allows for general error structures F_ν^t that can be correlated over time for each individual but are assumed to be independent across individuals and of regressors in the initial time period. Our permanent-transitory error structure is a special case. (see footnote #16 in Heckman and Navarro (2007))

(Theorem 1) follows an identification-in-the-limit strategy that conditions on large values of the indices associated with preceding choice probabilities. Applying this identification argument in our setting, the dropout probability function at any grade can be identified using the subset of individuals who attain that grade level with probability close to 1. Although their proof does not require conventional exclusion restrictions, they note that the assumptions of their Theorem 1 will be satisfied if there are transition-specific exclusion restrictions (that satisfy certain rank and support conditions). In our context, distance to different types of schools, the number of schools available, and the hourly wage w_{ia} imputed from the census data (which varies with age, educational attainment, and region of residence) constitute exclusion restrictions.

3.6.3 Identification of value-added model parameters

The value-added achievement model is a standard dynamic-panel-data model, of the kind discussed in Arellano and Bond (1991) with two modifications. First, the model incorporates discrete unobserved types to control for unobserved variables. Second, there is the potential for dropout after grades 6, 7, and 8, and the test score outcomes are only available for students who enroll in school. Heckman and Navarro (2007) also consider the identification of continuous outcomes and counterfactual outcomes in a stopping-time model, again using an identification-in-the-limit strategy based on sets of students with characteristics such that they advance to the next grade with a probability close to 1. Our permanent-transitory specification of the error term in the value-added equation is analogous to the one-factor error structure that they present. They note that one needs to have at least three outcome measures to identify a one-factor structure. In our case, we have 12 continuous outcomes (mathematics and Spanish test scores, from grade 4 through grade 9). They establish nonparametric identifiability of both the factor and error-term distributions, although we implement a parametric model.[38]

4 Estimation

4.1 Some measurement issues

In this section, we discuss two issues related to test-score measurement. First, some enrolled students are missing test-score data because they were absent the day of the test, perhaps due to illness. We

[38]Heckman and Navarro (2007) assume the factors are independent of covariates. We assume the type distribution can vary by *Prospera* status (an initial condition) and by the marginality index, which is a measure of local poverty. The Heckman and Navarro (2007) identification strategy can still be applied under this slightly more general formulation by first stratifying the data by *Prospera* status and marginality level.

use the notation $I_{ia}^{miss} = 1$ if the student's test score at age a is missing, else $I_{ia}^{miss} = 0$.

Second, students' true test-score performances may be mismeasured if there is cheating. To account for possible test-score distortion due to cheating, we specify a measurement equation for the relationship between the true test score A_{ia}^m and the observed test score $\tilde{A}_{ia}^m$:

$$\tilde{A}_{ia}^m = \begin{cases} \left(1 + c_{ija}^{mgI} \zeta_{ia}^m I_{ia}^{cheat}\right) A_{ia}^m & \text{if } I_{ia}^{miss} = 0 \\ \text{Not observed} & \text{if } I_{ia}^{miss} = 1 \end{cases} \tag{6}$$

where the effect of cheating is captured by the term $c_{ija}^{mgI} \zeta_{ia}^m$. c_{ija}^{mgI} captures the average cheating effect for a given subject m, grade g and retention status I, while $\zeta_{ia}^m \sim \log\text{normal}(-0.5\sigma_\zeta^2, \sigma_\zeta^2)$ captures the randomness of the cheating effect.[39] When students do not cheat ($I_{ia}^{cheat} = 0$) and the test scores are not missing ($I_{ia}^{miss} = 0$), under this specification, the observed test score equals the true test score:

$$\tilde{A}_{ia}^m = A_{ia}^m \quad \text{iff} \quad I_{ia}^{cheat} = 0 \text{ and } I_{ia}^{miss} = 0$$

4.2 The likelihood function

The outcomes observed for individuals are the enrollment choices at each age D_{ija}, whether the student passes the grade attended at age a I_{ia}^{Pass}, the grades completed for each age G_{ia}, and the observed achievement test scores in mathematics and Spanish at each age a, $\tilde{A}_{ia} = \{\tilde{A}_{ia}^1, \tilde{A}_{ia}^2\}$. Let a_i^f and a_i^l denote the first and the last ages at which we observe the individual i in the dataset. The initial conditions include family background, achievement test scores at grade 4 (when test scores first become available for this sample), age, gender, a marginality index describing the local poverty level, urban/rural residence status, the state of residence, the set of primary and secondary schools available to each child, the minimum distances needed to travel to different types of schools and whether the family is participating in *Prospera*.[40] We use notation $\Omega_i(0)$ to denote the set of these initial state variables. The time-varying state-space elements in any given time period, $\Omega_i(a)$, consist of whether attended school last year, type of school attended, grades completed thus far, whether retained in the last grade, and lagged achievement-test scores in mathematics and Spanish.

Given the vector of the observed test scores $\tilde{A}_i = \{\tilde{A}_{ia_f}^1, \tilde{A}_{ia_f}^2, ..., \tilde{A}_{ia_l}^1, \tilde{A}_{ia_l}^2\}$, the vector of the true test scores $A_i = \{A_{ia_f}^1, A_{ia_f}^2, ..., A_{ia_l}^1, A_{ia_l}^2\}$, and the vector of initial conditions and time-varying

[39] We choose this particular functional form so that $\zeta_{ia}^m > 0$ and $E(\zeta_{ia}^m) = 1$.

[40] A full list of family-background variables includes parental-schooling attainments, parental-employment statuses, whether both mother and father are present in the household, number of household members, first language spoken at home, internet accessibility, computer accessibility and years of preschool education.

state-space elements $\tilde{\Omega}_i = \{\Omega_i(0), \Omega_i(a_f), ..., \Omega_i(a_l)\}$, the individual likelihood can be written as:

$$L_i(\Theta, \mu, \rho; \tilde{A}_i, A_i, \tilde{\Omega}_i) = \textstyle\sum_{l=1}^{4} \rho_l(P, M) \Bigg\{ \underbrace{\prod_{j \in J^4_{ia_f}} \Pr(D_{ija_f} = 1 | \Omega_i(0), \mu_l)^{1(D_{ija_f}=1)}}_{\text{Primary-school choice at initial period}}$$

$$\textstyle\prod_{a=a_f+1}^{a_l} \Big\{ \underbrace{\Pr(I^{Pass}_{i,a-1} = 1 | A_{i,a-1}, D_{ij,a-1}, \mu_l)}_{\text{Prob of passing the last grade } g_a} \Pi_{j \in J^g_{ia}} \underbrace{\Pr(D_{i0a} = 1 | A_{ia}, \mu_l)^{1(D_{i0a}=1)}}_{\text{Prob of dropout at grade } g_a}$$

$$\underbrace{\big[\Pr(D_{ija} = 1 | A_{ia}, \mu_l) \phi(A_{ia} | A_{i,a-1}, D_{ij,a-1}, I^{Pass}_{i,a-1}, \mu_l) \phi(A_{ia} | \tilde{A}_{ia}, D_{ija}, I^{Pass}_{ia-1})^{1(I^{Miss}_{ia-1}=0)} \big]^{1(D_{ija}=1)} \Big\}^{1(I^{Pass}_{i,a-1}=1)}}_{\text{Prob of observing test score } \tilde{A}_{i,a} \text{ when passing grade } g_{a-1} \text{ in a school of type } j}$$

$$\underbrace{[\Pr(I^{Pass}_{i,a-1} = 0 | A_{i,a-1}, D_{ij,a-1}, \mu_l) \phi(A_{ia} | A_{i,a-1}, D_{ij,a-1}, I^{Pass}_{i,a-1}, \mu_l) \phi(A_{ia} | \tilde{A}_{ia}, D_{ij,a-1}, I^{Pass}_{ia-1})^{1(I^{Miss}_{ia-1}=0)}]^{1(I^{Pass}_{i,a-1}=0)}}_{\text{Prob of observing test score } \tilde{A}_{ia} \text{ when repeating the grade in a type } j \text{ school}} \Bigg\}$$

where Θ defines the vector of model parameters and we suppress the dependence of all the probabilities on the state space $\tilde{\Omega}_i$ to simplify notation. Pr(.) represents the logit or multinomial-logit probabilities of school-choice and retention decisions defined in equations 3 and 4. $\phi(.)$ represents the conditional density function of test scores derived from equation 2 and equation 6. J^g_{ia} is the available school-choice set at grade g_a defined in equation 3.3. The vector $\rho = \{\rho_1, ..., \rho_4 | P, M\}$ denotes the vector of unobserved-type probabilities conditioning on *Prospera* status P and marginality index M, where $\rho_l(P, M) = \Pr(\mu_l = 1 | P, M)$.

As described in the previous section, the true test scores are not observed in cases of cheating or when students are absent on the day of the test. In those cases, we integrate over the possible true test-score outcomes to obtain the individual likelihood function[41]:

$$L_i(\Theta, \mu, \rho; \tilde{A}_i, \tilde{\Omega}_i) = \int ... \int L_i(\Theta, \mu, \rho; \tilde{A}_i, A_i, \tilde{\Omega}_i) dA_{ia^1_i} ... dA_{ia^N_i}$$

where $\{a^1_i, a^2_i, ..., a^N_i\}$ are the ages that the test scores are either contaminated by cheating or missing for individual i. We obtain standard errors of the parameter estimates from the inverse of the average of the product of the score matrices, where the derivatives of the log likelihood are evaluated numerically.[42]

[41]This integration is performed numerically by taking 50 random draws of the shocks.

[42]This estimator is known as the BHHH estimator (Berndt et. al., 1974). To obtain the numerical derivatives needed to implement the estimator, we use a step-size parameter equal to 1% of the parameter estimates.

4.3 Some simplifying assumptions

We impose three assumptions with regard to the choice problems that simplify the estimation and are largely consistent with the data. First, we assume students do not switch to a different type of school once enrolled. Therefore, the primary-school type is chosen once at the start of primary school and the lower-secondary school type is chosen once at the start of lower-secondary school. Second, we assume that individuals who drop out of school do not afterwards re-enroll.[43] Third, because the fraction of students who repeat grades is fairly small (see Table 2), we estimate a separate value-added model for retained students but restrict the model coefficients to not vary across grades, separately within primary school and lower-secondary school grades.[44]

4.4 Evaluating the effects of *Prospera*-program participation

Prospera provides cash transfers for children of beneficiary households who are enrolled in school in grades 3-12. For children in grades 3-9, the transfers typically go to the mothers. Whether a transfer is received for each child depends, however, on whether that child regularly attends school (at least 85% of school days). We consider the family's *Prospera* status as a time-invariant characteristic, so it is contained in the initial state space $\Omega(0)$. Once families are enrolled in the program, they rarely lose their eligibility. Even if one child is not attending school, the family may still receive transfers for other children and is still considered to be participating. We use the estimated schooling model to simulate school-going and test-score outcomes for *Prospera* families' children had they not participated in *Prospera*. In this way, we are able to assess the grade-specific program impacts as well as the cumulative impacts of participating in *Prospera* for multiple years.

5 Model estimates

We estimate the model parameters by maximum likelihood. The key parameters, specifically those linked to *Prospera* effects, are shown in Appendix C, whereas the complete set of parameters can be found in Appendix D. In this section, we focus on two aspects: examining score distortion stemming from potential copying behavior, and evaluating the model's goodness of fit based on our adjusted test scores that account for test-score inflation resulting from cheating behavior.

[43]In our raw dataset, a mere 0.2% (378 individuals) switched schools to a different type during their primary-school years, while 3.2% (6,220 individuals) changed school types in lower-secondary school. Additionally, we note that 1.99% (3,770 individuals) re-enrolled in school after initially leaving.

[44]That is, we restrict $\delta_{kj}^{m4I} = \delta_{kj}^{m5I}, \delta_{kj}^{m6I} = \delta_{kj}^{m7I} = \delta_{kj}^{m8I}, k = \{1, 2, 3\}$ in the value-added equation 2 and $\gamma_k^4 = \gamma_k^5 = \gamma_k^6, \gamma_k^7 = \gamma_k^8, k = \{1, 2, 3, 4\}$ in equation 4 in the periods when $I_{ia}^{pass} = 0$. But the intercept terms δ_{0jl}^{mgI} and γ_{0l}^{g} are grade-specific.

5.1 Test-score measurement equation

A unique feature of our dataset is that it contains information on which students were flagged by the SEP as potential copiers. Our test-score measurement equation allows the true test score to differ from the measured test score in the event of copying and also allows for heterogeneity in the gains from copying across grades and types of schools (to reflect potential differences in monitoring). In the context of a value-added model, copying can lead to a one-sided measurement error in either the dependent variable (the test score) or in an independent variable (lagged test scores) or in both variables, but only for students who copied. Table 8 shows the percentages of students suspected of copying, which ranges from a low of 1.7% in seventh grade in general schools to a high of 9.9% in eighth grade in telesecondary schools. At the primary-school level, indigenous schools exhibit higher copying rates. We estimate that copying distorts average test scores by 20-118 points for copiers. Our earlier-described estimation approach accounts for this potential distortion.

5.2 Model goodness-of-fit

Our model involves a substantial number of parameters, in part due to our deliberate choice not to impose constraints in the value-added model coefficients across various grade levels. These parameters are mostly precisely estimated due to our large sample sizes. Tables 9 and 10 provide evidence on the model's goodness-of-fit. In Table 9, we compare average test scores across grades and by *Prospera* beneficiary statuses in the data and based on model simulations. The data averages closely align with the model-simulated averages, with few exceptions. The estimated model reproduces the observed pattern where *Prospera* beneficiaries have lower average test scores in primary grades for both subjects. It also captures the trend of test-score disparities between beneficiaries and non-beneficiaries diminishing in lower-secondary grades, and even reversing in mathematics.

Table 10 shows how well our model fits the school type distribution. The model's predicted proportions closely match the data, with differences of no more than 0.02. Our model effectively captures two important data features: the higher probability of *Prospera*-beneficiary children attending telesecondary lower-secondary schools and their higher rates of dropping out. Additionally, it reproduces the observed dropout patterns across different grades.

Table 8: Estimated test-score distortion from copying, by grade and school type

	Percentages	Math			Spanish		
		Raw	True	Diff	Raw	True	Diff
Grade 5							
General	4.2%	570	525	45	549	521	28
Indigenous	7.0%	540	474	66	515	468	46
Overall	4.4%	568	521	47	546	517	29
Grade 6							
General	4.0%	606	554	51	578	545	32
Indigenous	6.3%	566	510	56	536	499	37
Overall	4.1%	603	551	52	575	542	33
Grade 7							
General	1.7%	558	499	59	523	489	34
Telesecondary	4.1%	619	525	94	540	486	54
Technical	2.5%	573	498	74	536	489	47
Overall	2.6%	588	510	78	534	488	46
Grade 8							
General	3.3%	614	529	85	555	508	47
Telesecondary	9.9%	698	580	118	585	517	68
Technical	4.1%	613	528	84	554	511	43
Overall	5.3%	656	554	101	570	513	57
Grade 9							
General	2.6%	631	549	82	531	505	27
Telesecondary	5.7%	667	607	60	529	509	20
Technical	3.8%	621	553	68	536	510	26
Overall	3.8%	642	574	69	532	508	24

Note: The percentages in the second column give the percentages of students suspected of copying in either mathematics or Spanish tests.

6 Assessing cumulative *Prospera*-program effects

6.1 The cumulative *Prospera*-program effects

Average treatment effect on treated. As previously described, educational production functions typically assume that knowledge acquisition in mathematics and Spanish is a cumulative process. The value-added model specification allows lagged knowledge to have an effect on contemporaneous knowledge accumulation, so that the history of inputs into the learning process matters. If *Prospera* participation increases knowledge at a particular grade, then this benefit can have a persistent effect on learning in future grades. That is, program participation can have both direct effects on current test scores as well as indirect effects operating through lagged test scores.

In Table 11, we use our estimated model to simulate the effects of being a *Prospera* beneficiary

Table 9: Goodness-of-fit for average test scores by *Prospera* status (P)

Prospera	Mathematics score				Spanish score			
	$P=0$		$P=1$		$P=0$		$P=1$	
	Data	Sim	Data	Sim	Data	Sim	Data	Sim
Grade 5	521	522	495	496	520	519	490	490
Grade 6	550	551	526	527	545	543	515	517
Grade 7	489	489	492	491	477	477	465	465
Grade 8	516	515	529	526	487	487	480	480
Grade 9	540	539	564	563	490	489	481	481

Note: We simulate the test scores 100 times for each individual. In this table, we adjust for any copying in both the simulation and the data.

Table 10: Goodness-of-fit to school-type distribution

Lower-secondary choice	General Data	General Sim	Telesecondary Data	Telesecondary Sim	Technical Data	Technical Sim	Dropout Data	Dropout Sim
Non beneficiary (P=0)								
Grade 7	0.49	0.47	0.13	0.13	0.31	0.30	0.08	0.09
Grade 8	0.46	0.44	0.12	0.12	0.29	0.28	0.13	0.15
Grade 9	0.41	0.40	0.11	0.11	0.26	0.26	0.23	0.23
Prospera beneficiary (P=1)								
Grade 7	0.26	0.24	0.43	0.44	0.21	0.22	0.10	0.10
Grade 8	0.24	0.23	0.41	0.41	0.20	0.20	0.16	0.16
Grade 9	0.21	0.20	0.36	0.36	0.17	0.18	0.26	0.26

over multiple grades, starting with grade 4. Our estimation procedure allows for *Prospera* effects that operate through all of the different channels of our school progression and achievement model and that may differ for girls and boys. Columns labeled $P = 1$ show the outcomes for *Prospera*-beneficiary children/youth with their participation in the program. Columns labeled $\tilde{P} = 0$ show the simulated (counterfactual) outcomes were they not to participate in the program.[45]

It is only possible to assess test-score impacts for children/youth who would attend school both with and without *Prospera*. Therefore, our reported program impacts on test scores in column "Diff" represent lower bounds, as they do not include potential academic achievement gains for children/youth who in the absence of *Prospera* would not be attending the grade.[46] Our results show positive benefits of being a *Prospera* beneficiary in lower-secondary grades but essentially no effect for math and Spanish in primary grades. In lower-secondary school, the cumulative *Prospera* impact in mathematics increases with the grade level and reaches a high of 0.21 standard deviations

[45] Our simulation keeps the distribution of unobserved types for *Prospera* beneficiaries fixed.

[46] As we show in Figure 6 and Figure 7, absent children are disproportionally from most-disadvantaged family backgrounds and therefore tend to have larger program gains on average.

Table 11: Cumulative program impacts

	Mathematics score				Spanish score			
	$P=1$	$\tilde{P}=0$	Diff	S.E.	$P=1$	$\tilde{P}=0$	Diff	S.E.
Grade 5	495	496	-0.2	0.7	490	491	-0.9	0.7
Grade 6	527	522	4.3	0.8	517	521	-4.1	0.7
Grade 7	492	478	13.7	2.0	465	459	6.3	1.6
Grade 8	527	513	14.2	1.8	481	476	4.8	1.5
Grade 9	562	541	20.8	2.2	482	478	4.0	1.8
	Dropout rate				Retention rate			
	$P=1$	$\tilde{P}=0$	Diff	S.E.	$P=1$	$\tilde{P}=0$	Diff	S.E.
Grade 5	-	-	-	-	0.04	0.04	-0.004	0.002
Grade 6	-	-	-	-	0.02	0.03	-0.003	0.001
Grade 7	0.11	0.17	-0.07	0.01	0.003	0.002	0.000	0.000
Grade 8	0.16	0.23	-0.08	0.01	0.003	0.004	-0.001	0.001
Grade 9	0.25	0.33	-0.08	0.01	0.003	0.004	-0.001	0.001

Note: We report the test scores for children/youth who would attend school both with and without *Prospera*. They are obtained by a parametric bootstrap with 100 replications. For each bootstrap iteration, we draw the model parameters from their estimated distributions and re-simulate the cumulative impacts and derive the standard errors from the empirical distributions. The columns "Diff" capture the test-score gain of these subgroups. The columns "S.E." report the standard errors of the test-score gains from the program.

by grade 9. In Spanish, the cumulative gains are substantially smaller - about 0.04 standard deviations.[47]

We might expect the estimated program effects to be larger in lower-secondary school than primary school because the transfer amounts that families receive for school attendance are much larger.[48] Also, older children typically have more demands on their time that compete with schoolwork than do younger children, such as taking care of younger siblings, housework, working for family businesses or working for pay after school. The *Prospera* cash transfers may reduce these outside time uses, allowing them to focus more on schoolwork.

The lower panel of Table 11 reports the *Prospera* impact on the dropout rate (cumulative) and on the probability of repeating a grade. The results show that *Prospera* reduces the dropout rate by 0.08 before the start of grade 9, with the most pronounced effect during the primary to lower-secondary school transition. We also find that *Prospera* primarily reduces the retention probability during primary school but has no significant effect on retention during lower-secondary school.

Comparison of program effects for female and male students The *Prospera* program

[47]The average effect on the Spanish score displays substantial heterogeneity among *Prospera* beneficiaries, as will be shown below.

[48]in the fall semester of 2008 the transfers ranged from 130 to 265 pesos for primary school and 405 to 495 (385 to 430) for females (males) in lower-secondary school (US1= 11 pesos in 2008).

provides greater subsidies to girls than boys for attending school in post-primary grades. Table 12 examines whether program effects differ by gender. The test-score impacts are significantly positive for both girls and boys in all grades except for grade 5. The largest impacts are observed in lower-secondary school grades and impacts are larger in mathematics than in Spanish.

Interestingly, the estimates indicate that participation in *Prospera* leads to a slight reduction in gender academic achievement gaps. Male students have a 9-point advantage in average mathematics scores over females at grade 6. Participation in the *Prospera* program through grade 9 boosts female students' mathematics scores by 21.8 points in comparison to 19.6 for males and reduces the gender gap by 3.0 points (=(564-560)-(542-541)). In terms of Spanish test scores, female students have on average a 27 point advantage over males at grade 5. *Prospera* participation is also associated with a reduction in the gender gap in Spanish scores, albeit by a slightly smaller margin of 2.0 points (=(495-467)-(492-462)).

The program also narrows the gender gap in dropout rates. For the current *Prospera* beneficiaries, the cumulative dropout rate by grade 9 is 23.3 percentage points for females and 27.0 percentage points for males. When we simulate the model taking away the *Prospera* program, we find that the gender difference would have increased from 3.7 (=27.0-23.3) percentage points to 6.3 (=36.4-30.1) percentage points. Turning to the bottom panel of the table, we do not find significant gender differences in the *Prospera* effect on grade retention. In summary, our results show that both females and males benefit from *Prospera* participation and that the program generally reduces gender disparities in mathematics and Spanish test scores and in dropout rates.

***Prospera* effects by propensity-scores quartiles.** We next explore the heterogeneous *Prospera* impacts for students from different backgrounds in Figures 6 and 7. Figure 6 shows the effects of *Prospera* participation on test scores broken down by propensity-scores quartiles. As described in section 3.4, the propensity score is a summary statistic of students' family background, with quartile 1 denoting the most-advantaged families and quartile 4 denoting the most-disadvantaged families. Our estimates show larger impacts in later grades and smaller impacts in earlier grades, regardless of propensity-score quartiles. These patterns are consistent with the *Prospera* effects being cumulative with greater exposure associated with greater impact. Among the four quartiles, we observe the largest estimated cumulative impacts for students in the highest quartile, who are the ones from the most-disadvantaged backgrounds. *Prospera* increases their test scores in mathematics by 0.29 standard deviations and their test scores in Spanish by 0.09 standard deviations and both effects are statistically significant. Although the overall average cumulative impact of *Prospera* on Spanish is not significant, we find statistically significant positive impacts for the subgroup of students in the top propensity-score quartile, which contains the majority (52.7%) of the *Prospera*

Table 12: Gender differences in cumulative *Prospera* effects (by grade 9)

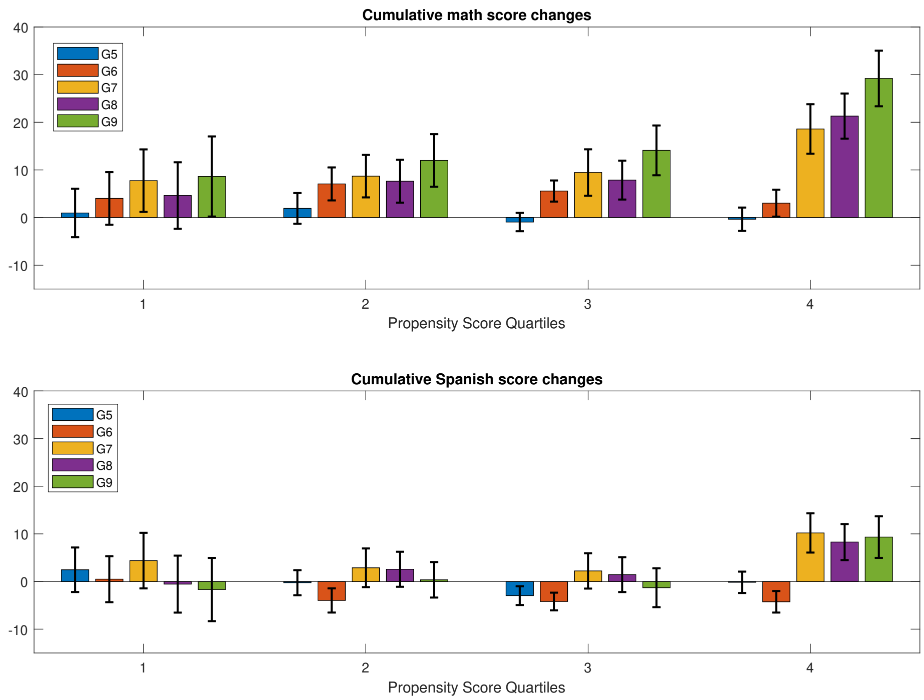
	Female				Male			
	$P=1$	$\tilde{P}=0$	Diff	S.E.	$P=1$	$\tilde{P}=0$	Diff	S.E.
Mathematics score								
Grade 5	500	499	0.7	0.9	491	492	-1.1	1.1
Grade 6	530	525	4.7	1.1	523	519	3.9	1.2
Grade 7	495	481	14.2	2.4	488	475	13.3	2.2
Grade 8	525	511	14.9	2.0	530	516	13.5	2.0
Grade 9	564	542	21.8	2.5	560	541	19.6	2.5
Spanish score								
Grade 5	503	505	-1.3	0.9	476	476	-0.5	1.0
Grade 6	531	536	-4.5	0.9	502	506	-3.6	1.0
Grade 7	484	480	4.6	1.9	446	437	8.1	1.7
Grade 8	498	494	4.2	1.8	462	456	5.5	1.7
Grade 9	495	492	3.2	1.9	467	462	5.0	2.1
Dropout rate								
Grade 7	0.10	0.16	-0.06	0.01	0.11	0.18	-0.07	0.012
Grade 8	0.15	0.22	-0.07	0.01	0.16	0.25	-0.09	0.011
Grade 9	0.23	0.30	-0.07	0.009	0.27	0.36	-0.09	0.009
Retention rate								
Grade 4	0.03	0.03	-0.003	0.002	0.05	0.06	-0.004	0.003
Grade 5	0.01	0.02	-0.002	0.002	0.03	0.04	-0.003	0.002
Grade 6	0.001	0.001	0.000	0.000	0.004	0.004	0.000	0.000
Grade 7	0.001	0.002	-0.001	0.001	0.005	0.006	-0.001	0.002
Grade 8	0.002	0.002	-0.001	0.001	0.005	0.007	-0.002	0.001

beneficiaries.

Figure 7 displays the cumulative effects of *Prospera* on three key outcomes: (1) Panel (a) presents grade attainment; (2) Panel (b) presents the cumulative dropout rate; and (3) Panel (c) presents the total number of retentions during primary school. The first two outcomes are evaluated at the end of a six-year period, corresponding to the end of grade 9 (for students who do not experience grade retention or drop out). The third outcome, the cumulative number of retentions, is assessed upon completion of primary school. The estimated impacts are shown conditional on the propensity score quartile. Notably, the most substantial impacts (with the exception of cumulative retentions) are observed in the fourth quartile, comprised of the most disadvantaged students. Because students opting to drop out are generally lower-performing, disregarding the selection bias associated with dynamic dropouts tends to exaggerate the downward bias when estimating the program effects, particularly for students in the fourth quartile compared to those in other quartiles.

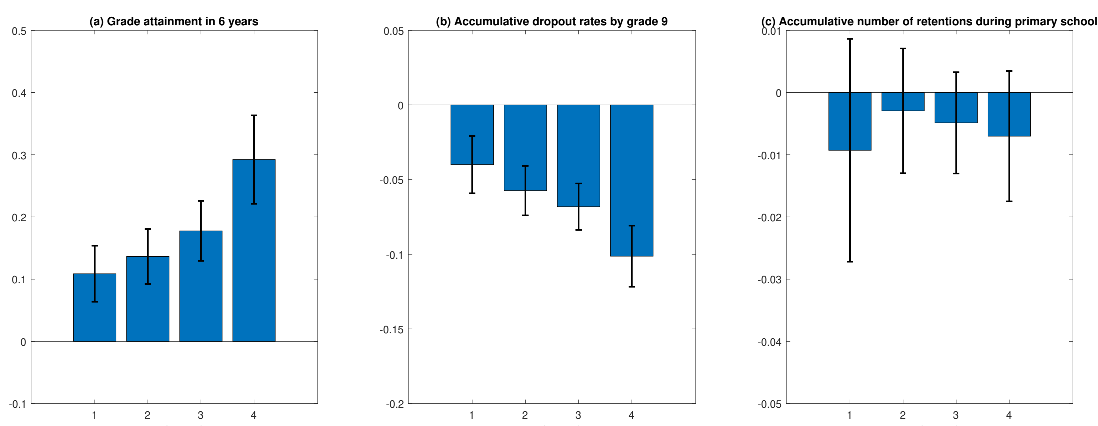
Comparing ATT with ATU. Next, we compare the average treatment effect on individuals who received treatment, commonly referred to as "ATT," with the average treatment effect on those

Figure 6: *Prospera* academic achievement effects by propensity-score quartiles



Note: 95% confidence intervals, depicted as bars, are derived using a parametric bootstrap method with 100 replications.

Figure 7: *Prospera* effects on schooling grade attainment, dropout and number of retentions by propensity-score quartiles



Note: 95% confidence intervals, depicted as bars, are derived using a parametric bootstrap method with 100 replications.

who did not receive treatment, commonly referred to as "ATU." In our analysis, the "untreated" group consists of children/youth who are not beneficiaries of the *Prospera* program ($P = 0$) but who had a positive probability of being a beneficiary, as determined by their household characteristics. (Recall that we imposed common support as described previously.)

The results in Table 13 indicate that the impact of the *Prospera* program is significantly greater for the treated group compared to the untreated group. For instance, the *Prospera* program leads to substantial improvements in mathematics and Spanish scores for the treated students, with increases of 20.8 and 4.0. For the untreated group, the program only results in a mathematics score improvement of 12.3 and has little effect on Spanish scores. At the extensive margin, *Prospera* reduces the dropout rate by 0.08 for the treated group but by only 0.05 for the untreated group. Lastly, *Prospera* also has a greater impact on reducing retention probabilities for the treated group compared to the untreated group.

These differences are in line with our earlier findings, shown in Figure 6. Those figures showed that the most substantial impacts are observed among the most disadvantaged groups. Now considering that these highly disadvantaged students are disproportionately more likely to be program beneficiaries, they predominantly fall into the treated group rather than the untreated group. Thus, it is expected that the ATT would be higher than the ATU.

6.2 The importance of the telesecondary-school option

As previously described, children/youth from *Prospera*-beneficiary households often live in rural areas where telesecondary schools are available and they more often attend this school type. We next evaluate the importance of telesecondary school as a determinant of *Prospera* impacts on school enrollment. In particular, we use our estimated model to simulate what educational outcomes would look like were the telesecondary-school option not available. The simulation takes into account that students might then have to travel further distances to get to schools or dropout if telesecondary schools had been their only option. Table 14 shows the distribution of local lower-secondary school choice sets in the data (baseline) and after removing the telesecondary option. For 6.9% of students, telesecondary schools are the only option.

The upper panel of Table 15 shows the simulated dropout proportion (at grade 9) for current *Prospera* telesecondary enrollees when the telesecondary schools are removed from their choice sets. The dropout proportion increases dramatically from 0.18 to 0.52. Average educational attainment over the six years of our observation period (up to grade 9) falls from 8.76 grades to 7.57 grades. Despite using different data sources and evaluation approaches, our results align with evidence on

Table 13: Comparing ATT, ATU and Overall treatment effect

	ATT		ATU		Overall	
	Mean	S.E.	Mean	S.E.	Mean	S.E.
Mathematics score						
Grade 5	-0.2	0.7	0.8	1.1	0.4	0.8
Grade 6	4.3	0.8	5.2	1.1	4.9	0.9
Grade 7	13.7	2.0	9.1	1.8	10.7	1.7
Grade 8	14.2	1.8	7.6	1.9	9.9	1.7
Grade 9	20.8	2.2	12.3	2.4	15.2	2.1
Spanish score						
Grade 5	-0.9	0.7	0.1	1.0	-0.3	0.8
Grade 6	-4.1	0.7	-2.5	1.0	-3.1	0.8
Grade 7	6.3	1.6	3.5	1.6	4.5	1.5
Grade 8	4.8	1.5	1.6	1.7	2.7	1.5
Grade 9	4.0	1.8	-0.2	1.8	1.2	1.7
Dropout rate						
Grade 7	0.07	0.01	0.03	0.007	0.05	0.01
Grade 8	0.08	0.01	0.04	0.007	0.06	0.01
Grade 9	0.08	0.01	0.05	0.007	0.06	0.01
Retention rate						
Grade 4	-0.004	0.002	-0.002	0.002	-0.001	0.001
Grade 5	-0.0025	0.0015	-0.0016	0.0011	-0.0009	0.0006
Grade 6	0.0001	0.0002	-0.0001	0.0002	0.0001	0.0001
Grade 7	-0.0009	0.0011	-0.0006	0.0008	-0.0003	0.0003
Grade 8	-0.0010	0.0008	-0.0005	0.0007	-0.0003	0.0002

telesecondary schools' importance reported in Navarro-Sola (2019).[49]

The lower panel of Table 15 shows simulated academic achievement for current *Prospera* telesecondary enrollees who continue their education even after telesecondary schools are no longer available. At grade 9, we observe a decrease in average mathematics test scores, from 602 to 535, and a decrease in average Spanish test scores from 492 to 473. This finding is consistent with the test score distributional differences seen in Figure 2, which suggested that telesecondary schools are relatively effective in enhancing students' test scores, particularly in mathematics,

Table 14: The school-choice distribution with and without the telesecondary option

	Baseline	No telesecondary
General, technical and telesecondary	0.696	N/A
General and telesecondary	0.072	N/A
General and technical	0.061	0.757
Telesecondary and technical	0.078	N/A
Only general	0.012	0.090
Only telesecondary	0.069	N/A
Only technical	0.008	0.080
No local schools	0.004	0.073

Table 15: Simulated dropout, educational attainment and achievement for *Prospera* telesecondary enrollees when the telesecondary option is removed

	With telesecondary	Without telesecondary
Dropout rate	0.18	0.52
Grades attained	8.76	7.57
Test scores at grade 9		
Math score	602	535
Spanish score	492	473

Note: The simulation is based on the *Prospera* beneficiaries who are currently enrolled in telesecondary school at grade 7. The outcomes are measured by grade 9.

6.3 Quantifying the importance of dynamic selection

The multi-equation modeling framework we implemented controlled for multiple sources of dynamic selection - due to dropout, school choice and grade retention - as well as for cheating and missing

[49]Using a difference-in-difference approach and Employment and Occupation National Survey (EONS) dataset, she showed that the construction of an additional telesecondary per 50 children would encourage 10 individuals to enroll in lower-secondary education, causing an average increase of one additional grade of education among individuals that could have attended it.

data. It incorporated unobserved types to control for potential selectivity on unobserved factors. Arguably, in Mexico, selection is an important consideration, given that school enrollment drops significantly in lower-secondary school grades and that parents can select from available schools.[50] In the US context, value-added models are often implemented without accounting for selection.

To explore the importance of controlling for multiple sources of selection, we compare our baseline results with results obtained from a simpler value-added model that we estimate grade-by-grade without distinguishing school types:

$$A_{ia}^{m} = \delta_0^{mg} + A_{i,a-1}\delta_1^{g} + \delta_2^{mg}P_i + Z_{ia}^{A}\delta_3^{mg} + \omega_{ia}^{mg}$$

Compared with equation 3.3, the contemporaneous *Prospera* effect δ_2^{mg} is homogeneous across school types and we do not model school choice. Also, this model does not include permanent unobserved heterogeneity (types). The cumulative program effect can be calculated as:

$$\Delta_g^m = \begin{cases} \delta_2^{m5} & \text{if } g = 5 \\ \delta_2^{mg} + \delta_1^{g}\Delta_{g-1} & \text{if } g > 5 \end{cases}$$

where Δ_g^m represents the cumulative effect for subject m in grade g, $\Delta_{g-1} = [\Delta_{g-1}^1, \Delta_{g-1}^2]$ is a 2×1 vector of cumulative effects for both mathematics and Spanish in grade $g-1$.

Table 16: Cumulative program impacts

	Math score		Spanish score	
	(1) Baseline	(2) Simple VA	(3) Baseline	(4) Simple VA
Grade 5	-0.2	-0.4	-0.9	-4.0
Grade 6	4.3	-1.8	-4.1	-6.7
Grade 7	13.7	6.6	6.3	-1.2
Grade 8	14.2	10.4	4.8	0.6
Grade 9	20.8	15.4	4.0	0.8

Note: Simple VA = simple value added model.

Table 16 compares our baseline results for mathematics and Spanish test scores (Columns (1) and (3)) with results generated from the simpler model (Columns (2) and (4)). The cumulative program impacts derived from the simpler model are noticeably smaller, especially in the lower-secondary grades. As previously noted, not controlling for dropping-out will tend to downward bias the impact estimates if the program causes students at the margin of dropping-out to stay in school

[50]Cameron and Heckman (2001) consider the problem of selection in modeling grade progression in US high schools, but they do not analyze test-score data.

longer. Also, our previous analysis showed that *Prospera* beneficiaries attend telesecondary schools in greater numbers and that these schools were particularly effective in teaching mathematics. These benefits are not captured in the simpler model. Lastly, the simpler model also ignores the negative selection on unobserved types, which also leads to under-estimation of program impacts. In summary, we find that the richer modeling framework is needed to capture heterogeneous program impacts and to control for selection at various stages (in dropout decisions, school-choice decisions and grade retention).

6.4 Quantifying the importance of unobserved types

Table 17 shows the distribution of the unobserved types, which is permitted to vary by *Prospera*-beneficiary status and by a measure of local poverty called the marginality index. Non-*Prospera* students are more likely to be type I or type II, whereas *Prospera* students are more likely to be type III or type IV. Table 18 explores how types are related to outcomes by simulating ninth-grade test scores and dropout outcomes when all *Prospera* beneficiaries are assumed to be of one type. Type I has the highest academic achievement on average (both in mathematics and Spanish), followed by types II and III. Type IV has the lowest academic achievement. Thus, we find the weaker types are disproportionately more likely to be enrolled in the *Prospera* program, indicating the presence of negative selection on unobserved factors in addition to the selection that is controlled by the observed characteristics.

If we simulate the average ninth-grade test-score impacts assuming that the type distribution for *Prospera* beneficiaries is the same as that of non-beneficiaries (as indicated in the column labeled "Avg P=0"), we find that the positive effects of *Prospera* participation are underestimated for both mathematics (by 8.7 points) and Spanish (by 2.8 points). The decrease in dropout rates is also mitigated. Thus, selection on unobserved factors is an important feature of the data.

Table 17: The unobserved type distributions

	Type proportions			
	Type I	Type II	Type III	Type IV
Non-Prospera, High Marginality	0.12	0.47	0.24	0.17
Prospera, High Marginality	0.24	0.23	0.24	0.30
Non-Prospera, Low Marginality	0.45	0.19	0.21	0.15
Prospera, Low Marginality	0.20	0.14	0.49	0.18

Table 18: Test-score and dropout outcomes by unobserved types

Outcomes	Type I	Type II	Type III	Type IV	Avg $P = 1$	Avg $P = 0$	Diff
Math score	601	582	553	522	561	570	-8.7
Spanish score	501	488	481	458	481	484	-2.8
Dropout	0.26	0.24	0.28	0.28	0.27	0.26	0.009

Note: Columns "Type I" to "Type IV" display the simulated test-score and dropout-rate outcomes if all *Prospera* students are assumed to be a particular type. The column "Avg P=1" reports the outcomes when *Prospera* students follow their own unobserved type distribution. The column "Avg $P = 0$" reports the alternative outcomes when *Prospera* students are assigned the unobserved type distribution for non-*Prospera* students. "Diff" reports the difference between "Avg $P = 0$" and "Avg $P = 1$".

6.5 Exploration of longer-term *Prospera* effects

Because our data tracks students for a maximum of six years, we cannot observe longer-term outcomes for our baseline cohort (4th graders in 2008). We can, however, use data for a slightly older cohort - 6th graders in 2007 - to draw inferences about how program impacts on 9th grade test scores relate to 12th grade test scores and graduation rates, subject to some caveats described below. We estimate the following regression model:

$$y_{i,12} = \alpha_0^y + \alpha_1^y \text{ENLACE}_{i,9}^{\text{math}} + \alpha_2^y \text{ENLACE}_{i,9}^{\text{Spanish}} + \alpha_3^y \text{female} + \epsilon_{i,9}^y$$

where $y_{i,12}$ consists of three different outcomes: (1) math test score at the end of grade 12; (2) Spanish test score at the end of grade 12; and (3) an indicator for whether completed secondary school (inferred from taking ENLACE test at end of grade 12). This regression is estimated using a sample of public-school students who took the 9th grade ENLACE exams in 2010 and also have a 12th grade ENLACE score.

Our previous findings showed that the *Prospera* program increases math scores by 0.21 standard deviations (SD) and Spanish scores by 0.04 SD by grade 9. We can obtain the predicted impact on grade 12 outcomes as follows:

$$\Delta_{12}^y = 0.21 * \alpha_1^{\text{math}} + 0.04 * \alpha_2^{\text{Spanish}}$$

Panel (a) of Table 19 reports the estimated *Prospera* effects derived in this way. A 1-SD increase in 9th grade math scores predicts a 0.34 SD increase in 12th grade math scores and a 0.12 increase in 12th grade Spanish scores. A 1-SD increase in the 9th grade math scores is also associated with a 6.8 percentage point increase in the graduation rate. Similarly, a 1-SD increase in 9th grade Spanish scores predicts a 0.30 SD increase in 12th grade math scores, a 0.53 increase in 12th grade

Table 19: Longer-term extrapolation of *Prospera* effects to grade 12

	Panel (a): full sample			Panel (b): twin subsample		
	(1) Math	(2) Spanish	(3) Grad.	(1) Math	(2) Spanish	(3) Grad.
+1 SD math (g9) (α_1^y)	0.34	0.12	0.068	0.25	0.16	0.034
	(0.002)	(0.002)	(0.0006)	(0.03)	(0.03)	(0.008)
+1 SD Spanish (g9) (α_2^y)	0.30	0.53	0.100	0.23	0.37	0.054
	(0.002)	(0.002)	(0.0006)	(0.03)	(0.03)	(0.008)
Twins FE	No	No	No	Yes	Yes	Yes
Prospera effect (Δ_{12}^y)	0.083	0.045	0.009	0.060	0.048	0.009

Data Source: The ENLACE dataset, as provided in De Hoyos et al. (2021). Our analysis focuses students in public schools who took the ENLACE exam in grade 9 in year 2010. Panel (a) uses all students while Panel (b) focuses on twins (students who enrolled in the same school in grade 6, with identical last names and birth dates). Robust standard errors are reported in parentheses.

Spanish scores, and a 10.0 percentage point increase in the graduation rate. Using the 9th grade test score gains implied by our estimated *Prospera* impacts, we can infer that being a *Prospera* beneficiary from grades 4 through 9 would increase 12th grade math scores by 0.083 SD, increase 12th grade Spanish scores by 0.045 SD, and increase the upper-secondary-school graduation rate by 0.9 percentage points. These extrapolated longer-term *Prospera* impacts are likely underestimates, because they do not account for the fact that students typically get additional conditional cash transfers in grades 9-12, which are the grades with the largest transfers.[51]

A potential concern with using the above OLS regression model as a means of extrapolating longer-term impacts is the omission of family-background covariates, which could bias the estimated lagged test score coefficients. To address this concern, Panel (b) performs a robustness check where we estimate the same model on a subsample of twins, adopting a twins-fixed-effects approach to control for inter-family differences in school, household, and neighborhood factors.[52] This estimation yields a slightly lower effect of the *Prospera* program on grade 12 math scores, but similar impacts on Spanish scores and graduation rates.

When interpreting our predicted *Prospera* effects at the end of grade 12, there are two important caveats. First, our estimated relationships between grade 9 and 12 test scores are based on individuals who participated in the ENLACE exams in both of those grades. This could lead to biased results due to selective attrition or selective exam-taking. Weaker students are more likely to drop out or repeat grades or otherwise not take the ENLACE exams, and therefore are not accounted for in our estimations. Second, our extrapolation based on regression only assesses *Prospera*'s impact

[51] De Hoyos et al. (2021) demonstrate that higher 12th grade ENLACE test scores predict better outcomes between ages 18-20. Specifically, they show that a one SD increase in these scores boosts the likelihood of university enrollment by 10% and, for employed individuals, leads to 6% higher wages within two years post-high school.

[52] Note we are not able to use the twins-sample approach to analyze the benefit of *Prospera*, because there is no variation in *Prospera* receipt for twins within the same household.

through its effect on 9th grade test scores, potentially overlooking other program benefits, such as enhanced non-cognitive skills or the advantages of continued exposure to *Prospera* during higher-secondary education. As a result, our predicted effects on outcomes in grade 12 probably represent lower bounds of the actual *Prospera* program impacts through grade 12.

7 Conclusions

Prior literature demonstrated substantial effects of the Mexican *PROGRESA/Oportunidades/Prospera* CCT program on educational attainment and schooling progression. However, little was known about how the program affected children's academic achievement, because comprehensive data to study that question were not available. Using newly available nationwide school-roster and test-score data, we develop and implement a model of school progression and academic achievements. Our modeling framework goes beyond the previous literature by integrating value-added test-score models, school-choice models that include the dropout option and account for local-labor-market-work opportunities, probabilistic grade retention, and measurement equations to allow for missing test-score data and to account for potential distortions arising from student cheating. Our analysis incorporates rich observed heterogeneity in family and child characteristics and unobserved heterogeneity in the form of discrete types, which enter multiple model equations. Our likelihood estimation approach explicitly controls for selective school enrollment/dropping-out and selection into different school types. Our estimation is based on multiple linked administrative and survey datasets as well as geocoded data on school locations, used to characterize individual-specific school-choice sets.

The data show that children from *Prospera*-beneficiary households live in less-urban areas and enroll in distance-learning schools (telesecondary) at much higher rates, so another goal of our analysis is to understand how academic achievement depends on the type of school attended. The question of whether a distance-learning modality is an effective way of teaching in comparison to fully in-person approaches is of considerable independent interest. Many countries face problems of how to provide access to high-quality schooling for students living in rural areas. There is scant evidence on the effectiveness of distance learning in such contexts.

Our key results include the following. First, the *Prospera* program did not substantially impact test scores in grades 5 and 6 in either general or indigenous schools. However, there are positive and statistically significant impacts on test scores in grades 7, 8, 9 with larger overall average impacts in mathematics (0.14-0.21 standard deviations) than in Spanish (0.04-0.06 standard deviations). The pattern of larger impacts at higher grade levels is perhaps to be expected given that the cash transfer amounts are significantly higher in lower-secondary than in primary grades. Second, we find that the *Prospera* program decreases the dropout rate by 7 percentage points, primarily at the sixth-to-seventh grade transition. Third, there is a consistent pattern of larger impacts for the most-disadvantaged children/youth, those in higher *Prospera*-participation propensity-score quartiles.

Fourth, the value-added parameter estimates indicate that lagged test scores are important determinants of current test scores, implying that *Prospera*-program effects on test scores accumulate over time. Our empirical findings show the importance of accounting for the dynamic nature of academic-achievement production to quantify the program gains accruing over multiple years. Fifth, we also find evidence of gender differences on test scores, with boys scoring higher on average on mathematics tests than girls and girls scoring higher on average on Spanish tests. The gender gaps in mathematics widen from primary to secondary grades. Participating in *Prospera* benefits leads to substantial test score increases for both girls and boys. The mathematics impacts are slightly greater for girls and the Spanish impacts are slightly greater for boys, which leads to a modest narrowing of the gender gaps in both subjects.

Sixth, our analysis takes into account that a small proportion of students in each grade were identified as having cheated (copied). Although cheating rates are low overall, they are somewhat higher in telesecondary schools and at higher grade levels. We develop an approach to account for possible test-score distortions (one-sided measurement error) arising from copying, which can affect both the dependent and right-side (lagged) variables in the value-added model.

Seventh, our analysis shows that telesecondary schools play an important role in the *Prospera*-program effect. Even with the copying adjustment, we find that telesecondary schools are at least similar in effectiveness and, in some cases, more effective than regular schools in teaching mathematics and Spanish. When we simulate the effects of removing the telesecondary school option for the children who attend these schools, their dropout rate prior to grade 9 increases from 18% to 52% and educational attainment decreases by 1.2 grades. Thus, telesecondary schools are important to *Prospera*'s success in improving educational outcomes for disadvantaged students.

Lastly, we compared results based on our preferred model specification, integrating value-added and school-choice models and allowing for unobserved heterogeneity, to those obtained from a simpler model. Our extended approach led to estimates that in some respects, including program impacts, are substantially different. Overall, our results indicate that *Prospera* was not only effective in increasing school enrollments but also that the program led to significant positive impacts on academic achievement in mathematics and Spanish, with the most disadvantaged children/youth experiencing the greatest benefits and with distant learning through telesecondary schools playing critical roles.

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A Data sources

This appendix contains some additional information about the sample sizes and about the data elements in the different surveys that we use. We also describe how we obtain the GIS location data used in estimating the school-choice model.

The student survey: Students answer questions related to their school and home life. They are asked about their own effort on school work in terms of how much attention they pay in class, whether they participate in class and how many hours they study each day. There are also questions about the home environment, for example, how many siblings they have.

The parent survey: For the parent survey, there are questions about the socioeconomic status and some questions about early childhood (such as whether the child attended preschool and whether parents read to the child when they were young). We use information on parents' education, work status, on the household size, housing characteristics and on household assets.

The geographic location data. : In the *ENLACE* data, each school has a unique identifier. Several years of data also contain geographic data for each school, including the state, municipality and locality where the school is located. Mexico has 31 states and a federal district, and within these districts there are 2,448 municipalities. In the *ENLACE* test score data, there are schools recorded in all states, close to 2,000 municipalities and over 20,000 localities. Given that the school IDs are constant over time, it is possible to use the years that contain geographic data, and create a database containing the vast majority of the schools and their respective locations. This database was merged with census data which allows us to link with information on the locality in which the school resides with longitude and latitude coordinates. We used R software to obtain a measure of the distances between primary schools and between primary and secondary schools to determine the choice sets available to families given their location.

A.1 Sample restrictions

Our initial sample consists of individuals who completed surveys in 2008, totaling 218,685 individuals. We then followed these steps:

1. We excluded individuals with abnormal test scores (scores below 100) and those whose ages fell outside the range of 7 to 17 or were missing. This resulted in a sample size of 215,709.

2. We removed individuals for whom *Prospera* status information was unavailable, leaving us with a sample of 192,372.

3. We excluded individuals whose lower-secondary school types were not categorized as general, telesecondary, or technical types, resulting in a sample size of 191,768.

4. Individuals who attended primary and secondary schools in different states were also removed from the sample, resulting in 189,604 individuals.

5. We eliminated students with missing grade information or test scores for more than one period. However, students missing test scores for only one period were retained, resulting in a remaining sample size of 187,425.

6. Observations lacking primary-school names or distance information were dropped, resulting in a remaining sample size of 186,692.

7. Individuals missing any of the key variables (age, gender, retention, urban status, cheating factor, region, internet access, computer access, first language, presence of father at home, presence of mother at home, and number of household members) were excluded. Some variables, such as parental education, household income, and parental working status, were allowed to have missing values. The final sample consisted of 177,103 individuals.

Our final sample consists of 177,103 unique individuals, and 997,019 individual-period observations, as presented in Table 1. To establish our baseline sample for estimation, we further excluded individuals whose propensity score fell within the bottom 1% ($pscore < 0.0353$, as described in the text). This resulted in a reduced sample size of 135,433 that was used in estimating our model. (We removed 458 Prospera students and 41,212 non-Prospera students based on this criterion.)

A.2 Local wage imputation method

Our administrative test-score database does not contain wage information for students. We impute the potential wages that students could earn using data from the 2010 Mexican Census obtained through the IPUMS site:

https://international.ipums.org/international-action/sample_details/country/mx#tab_mx2010a.

The census contains the age and gender, working status, school-enrollment status, and the wages earned for children across Mexico. It also includes other information such as the parents' schooling, family income and descriptive statistics about the home. Lastly, there is information on the municipality in which each household lives. The full list of variables that we use appears in Table A1. We exclude individuals whose age is $<$12 or $>$20 years old, as well as children/youth for whom the school-attendance status is undefined. We further exclude students who have already finished upper-secondary school (educational attainment levels ≥ 13).

We estimate a wage regression for the working children/youth sample. The dependent variable is hourly wage, which is calculated by monthly income (Inc) divided by 4 and divided by hours worked in the last week (Wkhs).[53] To account for selection into working in estimating the wage offer

[53] We trimmed the hourly wage distribution at the 99th quantile.

Table A1: Variables used from Census

Variable	Note	Name in database
Age	Age of subject	MX2010A AGE
Enroll	School enrollment dummy (1 = yes, 2 = no)	MX2010A_SCHOOL
Inc	Income of individual for the last month	MX2010A_INCOME
HInc	Household's income from work	MX2010A_INCHOME
Wkhs	Number of hours worked in the last week	MX2010A_HRSWORK
Edu	Educational attainment (in years)	MX2010A_EDATTAIN
Gender	Gender; Male = 1, Female = 2	MX2010A_SEX
Empl	Employment status	MX2010A_EMPSTAT
Pps	Position at work	MX2010A_CLASSWK
Edu_Mom	Mother's Educational attainment (in years)	MX2010A_EDATTAIN_MOM
Edu_Dad	Father's Educational attainment (in years)	MX2010A_EDATTAIN _POP
Electricity	Access to electricity	MX2010A_ELECTRIC
PipWater	Access to piped water	MX2010A_PIPEDWTR
Internet	Access to the internet	MX2010A_INTERNET
Comp	Access to computer	MX2010A_COMPUTR
State	State code	GEO1_MX2010
Mun	Municipality code	GEO2_MX2010
Urban	Urban-rural status; 1 = rural, 2 = urban	URBAN

parameters, a Heckman (1979) selection model is estimated. The wage regression and labor-force-participation equations include age, a school-attendance indicator, educational attainment, parents' education, missing indicators for parents' education, urban-rural dummies, north-south dummies, and municipality dummies. In addition, variables representing family socioeconomic levels, such as family income (household income) and home infrastructure (home electricity access, home piped-water access, home internet access, home computer access) are used as exclusion restrictions that affect selection into working but not the wage offers directly.

Using the estimated coefficients from the probit estimation of the labor force participation equation, we form control functions for each student (the inverse Mills ratio λ). The second-stage regression has hourly wages as the dependent variable, and regressions are done separately for girls and boys. The hourly wage specification includes municipality fixed effects, which allow for substantial regional variation. The results are reported in Table A2. We use the regression estimates to impute hourly wage offers to the children/youth in our analysis sample, based on their observed characteristics.

Table A2: Wage regression with Heckman-selection correction

	Boys		Girls	
Coefficient	Estimate	S.E.	Estimate	S.E.
Age	0.682***	(0.173)	0.593**	(0.239)
Enroll	5.109***	(1.346)	5.301***	(1.885)
Edu_mom_missing	−1.483	(1.517)	3.561	(2.438)
Edu_dad_missing	0.08	(1.317)	−0.051	(2.097)
Edu	0.394**	(0.175)	−0.459*	(0.279)
Edu_mom	−0.535***	(0.163)	0.389	(0.265)
Edu_dad	−0.190	(0.179)	−0.039	(0.291)
Urban	−0.096	(1.719)	5.753**	(2.442)
λ	0.023	(0.017)	−0.015	(0.029)
Age*Enroll	−0.252***	(0.081)	−0.292***	(0.111)
Age*Edu_mom_missing	0.08	(0.085)	−0.216	(0.135)
Age*Edu_dad_missing	0.006	(0.074)	0.001	(0.117)
Age*Edu	−0.014	(0.010)	0.042***	(0.015)
Age*Edu_mom	0.034***	(0.009)	−0.019	(0.015)
Age*Edu_dad	0.013	(0.010)	0.006	(0.016)
Age*Urban	0.038	(0.102)	−0.306**	(0.141)
Urban*Enroll	−2.634***	(0.786)	−3.666***	(1.104)
Urban*Edu_mom_missing	0.84	(0.943)	−2.197	(1.459)
Urban*Edu_dad_missing	0.081	(0.827)	0.105	(1.280)
Urban*Edu	−0.019	(0.110)	0.072	(0.169)
Urban*Edu_mom	0.229**	(0.097)	−0.203	(0.154)
Urban*Edu_dad	0.137	(0.106)	−0.021	(0.168)
North*Age	−0.585***	(0.156)	−0.302	(0.231)
North*Enroll	−4.866***	(1.170)	0.438	(1.743)
North*Edu_mom_missing	−0.161	(1.621)	−2.976	(2.625)
North*Edu_dad_missing	−0.092	(1.461)	−4.095*	(2.409)
North*Edu	−0.080	(0.187)	−0.301	(0.301)
North*Edu_mom	0.177	(0.159)	−0.504*	(0.259)
North*Edu_dad	0.113	(0.162)	−0.037	(0.260)
North*Urban	0.144	(0.124)	−0.341	(0.213)
Age*Enroll*Urban	0.128***	(0.047)	0.190***	(0.064)
Age*Edu_mom_missing*Urban	−0.012	(0.053)	0.162**	(0.081)
Age*Edu_dad_missing*Urban	−0.006	(0.047)	0.012	(0.072)
Age*Edu*Urban	0.0001	(0.006)	−0.007	(0.009)
Age*Edu_mom*Urban	−0.013**	(0.005)	0.013	(0.009)
Age*Edu_dad*Urban	−0.008	(0.006)	0.001	(0.009)
Age*Enroll*North	0.278***	(0.068)	−0.029	(0.099)
Age*Edu_mom_missing*North	0.021	(0.090)	0.159	(0.144)
Age*Edu_dad_missing*North	−0.002	(0.081)	0.21	(0.133)
Age*Edu*North	0.006	(0.010)	0.012	(0.016)
Age*Edu_mom*North	−0.007	(0.009)	0.028*	(0.014)
Age*Edu_dad*North	−0.007	(0.009)	0.001	(0.014)
Observation	174,905		76,978	
R^2	0.216		0.251	

Sample: Mexico 2010 Census. we exclude individuals whose age is < 20 or > 12, and whose school attendance status is undefined. We further exclude students who have already finished high school (educational attainment levels ≥ 13). We defined a dummy variable *Edu_mom_missing* = 1 if *Edu_mom* is missing or unknown, a dummy variable *Edu_dad_missing* = 1 if *Edu_dad* is missing or unknown. And the variable North is also a binary variable whether the municipality is in the North or South region of Mexico (1 = North, 0 = South). The dependent variable is hourly wage, which is defined as the monthly income (Inc) divided by 4 times hours worked in the last week. The hourly wage is trimmed at the upper 99th quantile. λ is the inverse Mills ratio calculated from the first-stage probit regression. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

A.3 *Prospera*-beneficiary propensity-score model

We estimate a probit model for the probability that a child/youth comes from a *Prospera*-beneficiary family. The precise eligibility criteria are not made public and they vary somewhat by geographic region. Eligibility is not income-based but is rather based on housing characteristics and demographics that are highly correlated with poverty. From the program administers, we ascertained that the following characteristics are often used in determining eligibility: measures of overcrowding in the household, a demographic dependency index, sex of the household head, whether the household has access to social security (IMSS), number of children age 0-11 years, education of the household head, whether the house has a bathroom with water, type of floor in the home, whether the home has a gas stove, whether the house has a refrigerator, whether the house has a washing machine, whether the family has a vehicle and an indicator of rural/urban/semi-urban status. To estimate a probit model for the probability of being a *Prospera* beneficiary, we use variables from the context surveys that are equal to or are close proxies for the above known eligibility determinants. The percentage correctly classified under the model is 90%.

Table A3 shows the estimated coefficients from the model. The child being female makes it more likely that the family participates in *Prospera*. Also, having more siblings or a larger household increases the probability of being a beneficiary. Having higher income (>1500 pesos) makes it less likely to be a beneficiary. Higher education categories for the mother or father make it less likely that a family participates (the omitted category is less than primary education). If the mom works, the family is less likely to participate; but if the dad works more than 8 hours per day, the family is more likely to participate. A higher ratio of number of persons to number of rooms in the home increases the probability of being a participant. Car ownership, having drainage connected to a public system, having a refrigerator, owning a clothes washer, having garbage-collection service, having internet, having a tv, and having sanitary facilities in the home makes all make it less likely that the family participates in *Prospera*. Owning the home, having a dirt floor, having electricity (which is nearly universal), cooking and sleeping in the same room, and speaking an indigenous language, ceteris paribus, increases the participation probability. We allowed for item non-response by including indicator variables in the probit regression, but we excluded any observations for which both mother's and father's education information was missing. Thus, individuals were required to have parental survey information, in which the parental-education information was gathered.

Table A3: *Prospera*-participant propensity-score model (probit)

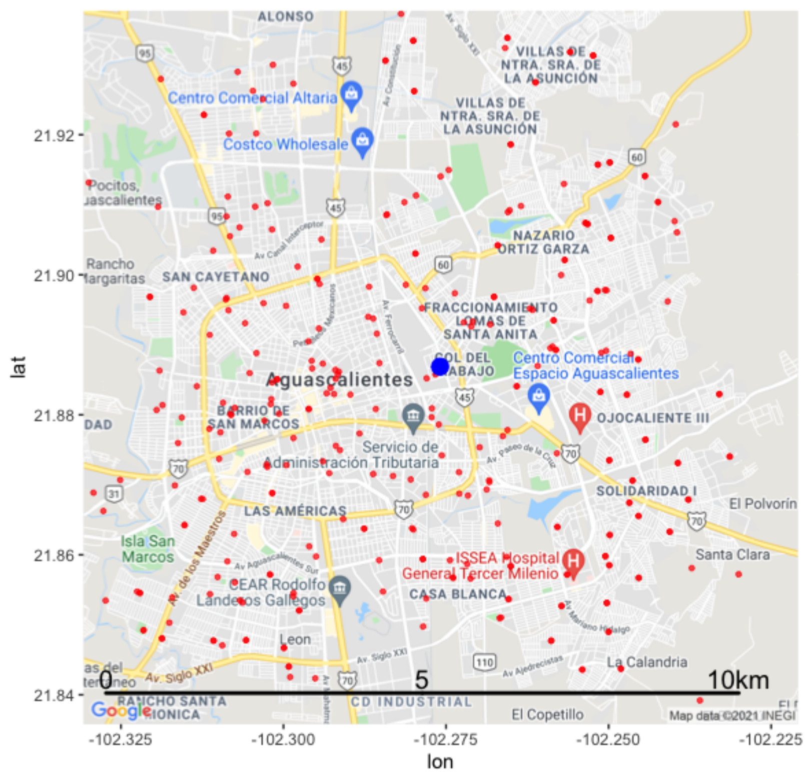
Coefficient	Estimate	Std. Error
intercept	0.642	0.037
female	-0.016	0.008
one sibling	0.086	0.021
2-3 siblings	0.324	0.020
4-5 siblings	0.515	0.021
6+ siblings	0.675	0.023
num siblings missing	0.503	0.034
father only present	0.029	0.024
mother only present	0.010	0.011
number total at home	0.013	0.002
num home missing.	0.108	0.036
family monthly income between 1500 and 29999	-0.165	0.009
family monthly income between 3000 and 7499	-0.385	0.011
family monthly income between 7500 and 14999	-0.517	0.021
family monthly income between 15000 and 30000	-0.470	0.037
family monthly income >30000	-0.103	0.037
famincmiss	-0.115	0.023
mom completed primary	-0.065	0.010
mom completed secondary	-0.072	0.011
mom bachalaureate or tech	-0.405	0.015
mom BA or more	-0.635	0.031
dad completed primary	-0.139	0.010
dad completed secondary	-0.282	0.011
dad bachalaureate or tech	-0.547	0.015
dad BA or more	-0.698	0.023
mom ed miss	-0.125	0.047
dad ed miss	-0.286	0.027
mom works 4+ hours daily	-0.258	0.009
mom works <4 hours or retired/looking for work	-0.199	0.017
dad works <8 hours daily	0.112	0.012
dad does not work or is retired	-0.005	0.013
mom work info missing	-0.178	0.036
dad work info missing	0.023	0.021
ratio of number in home/number of rooms	0.027	0.003
missing ratio	0.053	0.040
whether own home	0.177	0.010
missing home own info	0.173	0.030
whether have a car or truck	-0.264	0.009
cartruckmiss	-0.030	0.027
house has dirt floor	0.112	0.010
dirtfloormiss	-0.071	0.024
house drainage connected to public	-0.519	0.009
drainagemiss	-0.229	0.026
sanitary fac in home	-0.127	0.013
sanitarymiss	-0.113	0.028
electric power in the home	0.102	0.022
electric power miss	0.032	0.031
house has a refridgerator	-0.232	0.011
refridgerator miss	-0.142	0.027
cook and sleep in the same room	0.102	0.012
cooksleepmiss	0.086	0.035
house has a clothes washer	-0.147	0.010
clotheswashmiss	-0.037	0.029
speak indigenous language	0.343	0.014
indiglangmiss	0.045	0.028
covered by IMSS social security	-0.138	0.009
IMSS info missing	0.069	0.011
household has a stove	-0.283	0.015
stove info missing	-0.163	0.024
household has internet	-0.272	0.016
internet missing	0.055	0.026
household has tv	-0.042	0.020
tv missing	-0.208	0.029
household has garbage collection	-0.111	0.009
garbage collection missing	-0.028	0.028

The model also includes state fixed effects. The omitted category for dad working is works 8+ hours per day. The omitted category for mom working is engaged in housework. The percent correctly classified under the model is 89%.

B Additional descriptive statistics

In Mexico, multiple school sessions are often held in the same building, such as a morning and afternoon session. The different sessions may have different principals and teachers, so in the dataset they are considered to be different schools. Figure B1 shows one illustrative example of local primary-school sessions in Aguascalientes, a city in central Mexico. It has 316 school sessions distributed in 250 unique coordinates within 10 kilometers.

Figure B1: Local primary school sessions around Aguascalientes



Aguascalientes is a city with about 1.4 million inhabitants in 2020 in central Mexico. It contains 316 school sessions with 250 unique coordinates, indicating multiple school sessions share the same physical teaching place.

Table B1 illustrates the distribution of children across various grades and types of schools, categorized by age. As observed in Table B1, there is a notable variation in the types of schools attended and enrollment decisions for children of the same age.

Table B1: Distribution across grades and types of schools by age

	Grade 4		Grade 5		Grade 6		Grade 7			Grade 8			Grade 9				
Age	Gen	Ind	Gen	Ind	Gen	Ind	Gen	Tele	Tech	Gen	Tele	Tech	Gen	Tele	Tech	Drop	Obs.
9	0.97	0.03	-	-	-	-	-	-	-	-	-	-	-	-	-	-	35,346
10	0.76	0.03	0.21	0.01	-	-	-	-	-	-	-	-	-	-	-	-	157,928
11	0.09	0.01	0.68	0.02	0.19	0.01	-	-	-	-	-	-	-	-	-	-	172,691
12	0.02	-	0.09	0.01	0.66	0.02	0.10	0.03	0.06	-	-	-	-	-	-	0.01	176,552
13	-	-	0.03	-	0.09	0.01	0.31	0.13	0.21	0.09	0.03	0.06	-	-	-	0.05	175,975
14	-	-	-	-	0.03	-	0.03	0.03	0.02	0.30	0.12	0.19	0.09	0.03	0.05	0.10	176,234
15	-	-	-	-	-	-	0.01	0.01	0.01	0.04	0.03	0.03	0.34	0.14	0.22	0.17	141,657
16	-	-	-	-	-	-	-	0.01	-	0.05	0.07	0.03	0.17	0.15	0.11	0.41	19,026
17	-	-	-	-	-	-	-	-	-	0.01	0.02	0.01	0.13	0.21	0.09	0.54	3,962

Note: The table presents data on school enrollment, school types and grade distributions across different ages. The final column shows the number of observations for each age group. Excluding the last column, the sum of each row's values equals 1.

C Model estimates (key parameters)

C.1 Value-added models

Tables C1 and C2 show a subset of the parameter estimates for the value-added production functions for the mathematics and Spanish test-score outcomes. The tables show the coefficients associated with the lagged scores, the *Prospera* participation indicator and gender. The specifications also include other covariates, such as parents' education and numbers of siblings, to capture heterogeneous family inputs into the achievement-production process. Also, they include indicators for rural/urban residence and for region of residence to capture regional differences in school quality/infrastructure. The full set of estimated parameters is in Appendix E. Each model includes lagged scores in both subjects, assuming that knowledge of Spanish might facilitate mathematics learning and vice versa.[54] For example, children who are more proficient in one subject might be able to focus their efforts more on studying the other subject. The lagged parameters are highly statistically significant in all grades and in both subjects. As expected, the lagged own-subject coefficient estimates are larger than the other-subject coefficient estimates. Boys have significantly higher scores than girls in mathematics and lower scores in Spanish. The gender gaps in mathematics are larger in secondary school than in primary school.[55]

Tables C1 and C2 report the one-year effect of being a *Prospera* beneficiary, allowing the effects to vary by propensity-score quartile. Given that only a small portion of *Prospera* beneficiaries are in the low propensity-score quartiles (4% in quartile 1 and 13% in quartile 2), it is not surprising to see systematically larger standard errors for the estimates associated with these two quartiles, leading to their coefficient estimates being insignificant. We observe more-significant effects of *Prospera* participation in the top two quartiles, especially the one with the highest propensity score (quartile 4) that includes 53% of *Prospera* beneficiaries from the most-disadvantaged backgrounds in terms of SES. In particular, *Prospera* has statistically significant impacts on mathematics for children/youth in quartile 4 in lower-secondary school (grades 7, 8, 9), with a range from 0.07-0.19 standard deviations. In Spanish, the effect sizes are smaller and and less often statistically significantly different from zero. The significant effect sizes range from 0.06-0.16 standard deviations. For both mathematics and Spanish, the largest impacts tend to be observed in the 7th grade.

[54] Aucejo and James (2021) estimate production functions for mathematics and verbal test scores using UK data. They find cross-effects for verbal skills for learning mathematics but not vice versa. We find cross-effects for both subjects.

[55] Bharadwaj et al. (2016) also find that Chilean boys perform better than girls in mathematics and that the gaps widen between fourth and eighth grades. Aucejo and James (2021) find a large female advantage in verbal skills in the UK.

Table C1: Mathematics value-added model estimates

	General		*Indigenous*			*General*			*Telesecondary*			*Technical*	
	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
Math score													
Lag math	0.54	0.53	0.40	0.43	0.43	0.46	0.47	0.32	0.41	0.41	0.39	0.46	0.46
	(0.00)	(0.00)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Spanish	0.16	0.13	0.14	0.16	0.26	0.18	0.27	0.25	0.17	0.24	0.27	0.16	0.26
	(0.00)	(0.00)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Prospera effect by propensity-score quartile and gender													
P*Q1 (4%)													
Female	-3.30	5.50	5.00	10.01	3.20	-1.20	8.30	9.00	-2.01	6.79	7.30	4.60	1.70
	(3.70)	(3.71)	(47.25)	(54.46)	(4.97)	(4.53)	(5.38)	(12.63)	(12.90)	(12.39)	(7.20)	(5.99)	(7.41)
Male	5.20	0.30	-1.00	10.00	3.30	-2.70	3.31	9.00	-4.30	8.10	5.70	1.70	11.80
	(3.48)	(3.52)	(44.38)	(29.42)	(4.89)	(4.45)	(5.74)	(13.86)	(12.15)	(14.87)	(7.04)	(6.57)	(8.09)
P*Q2 (13%)													
Female	4.40	7.30	10.30	7.00	7.90	0.90	7.80	0.00	0.51	1.50	12.10	5.30	10.50
	(2.07)	(2.05)	(18.41)	(15.58)	(3.24)	(2.88)	(3.43)	(6.84)	(5.45)	(6.16)	(4.42)	(3.34)	(4.37)
Male	-0.90	4.60	-3.00	11.30	6.90	3.10	2.30	-1.00	-1.00	9.50	4.50	2.10	6.60
	(1.99)	(2.04)	(18.85)	(15.71)	(3.24)	(2.89)	(3.57)	(6.50)	(5.55)	(6.42)	(4.26)	(3.84)	(4.75)
P*Q3 (30%)													
Female	-0.10	6.10	-6.90	5.40	6.70	2.80	6.40	3.70	-0.90	7.80	8.80	5.10	13.30
	(1.41)	(1.42)	(9.03)	(8.29)	(2.56)	(2.23)	(2.54)	(4.05)	(3.29)	(3.85)	(3.31)	(2.76)	(3.35)
Male	-0.90	7.20	-7.60	-0.50	5.30	-2.20	5.20	3.90	-0.70	8.80	8.20	6.10	5.21
	(1.38)	(1.43)	(9.71)	(8.99)	(2.46)	(2.37)	(2.78)	(3.97)	(3.57)	(4.02)	(3.22)	(2.80)	(3.46)
P*Q4 (53%)													
Female	1.70	3.50	-3.20	-0.70	15.21	9.90	11.20	13.00	7.10	15.40	17.40	13.50	19.00
	(1.30)	(1.33)	(6.55)	(5.91)	(2.61)	(2.22)	(2.57)	(3.54)	(2.97)	(3.46)	(2.73)	(2.43)	(2.97)
Male	0.10	4.60	-7.90	-1.70	16.70	6.10	8.90	15.10	8.20	16.00	13.00	13.30	17.11
	(1.28)	(1.35)	(6.51)	(5.88)	(2.52)	(2.33)	(2.70)	(3.51)	(3.08)	(3.61)	(2.79)	(2.48)	(3.04)

Note: Standard errors are shown in parentheses. The first column gives the percentages of *Prospera* beneficiaries in each quartile. The model includes additional control variables for parents' schooling attainment, parents' working status, number of household members, child age and its square, gender, language spoken at home, internet access, computer access, number of years child attended preschool, urban-rural dummy, regional dummies (north, north-center, center, south) and unobserved types. The full set of estimated parameters can be found in Appendix E.

C.2 School-choice/dropout model

Table C3 reports estimated coefficients from the school-choice/dropout model after grade 6, where the omitted category is dropping-out and working.[56] The specification also includes other covariates as described in the table footnote. Family background and demographic variables capture heterogeneities in preferences for schooling and school types. Regional indicators capture regional differences in school quality and infrastructure that may affect school choices. As seen in the table, children with higher sixth-grade test scores are more likely to attend general or technical schools rather than telesecondary schools and are less likely to drop-out. Being in a higher propensity-score quartile increases the likelihood of attending a telesecondary school.

In grades 7 and 8, students decide only whether to drop out and Table C4 shows how being a *Prospera* beneficiary affects this decision. The estimates show a statistically significant negative effect on dropout for male and female youth from lower SES households (i.e. those with propensity

[56]In estimation, the choice set for individuals varies depending on the set of schools available to them.

Table C2: Spanish value-added model estimates

	General		*Indigenous*		*General*			*Telesecondary*			*Technical*		
	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
Spanish score													
Lag math	0.22	0.20	0.24	0.18	0.13	0.23	0.19	0.08	0.17	0.15	0.12	0.22	0.18
	(0.00)	(0.00)	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Spanish	0.42	0.39	0.28	0.34	0.52	0.47	0.48	0.43	0.40	0.40	0.53	0.47	0.48
	(0.00)	(0.00)	(0.02)	(0.02)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Prospera effect by propensity-score quartile and gender													
P*Q1 (4%)													
Female	-1.30	-0.60	6.00	4.00	2.80	-7.10	2.90	10.80	-2.10	5.00	4.01	-1.00	-4.70
	(3.33)	(3.10)	(32.30)	(68.93)	(4.75)	(4.32)	(4.43)	(9.85)	(9.48)	(11.13)	(5.99)	(5.60)	(6.38)
Male	6.20	-1.10	0.00	3.00	1.30	-8.80	-3.50	13.50	-6.20	-6.00	-1.80	3.60	-7.50
	(3.26)	(2.95)	(28.50)	(30.14)	(4.85)	(3.90)	(4.44)	(10.67)	(9.27)	(10.12)	(6.38)	(5.49)	(5.80)
P*Q2 (13%)													
Female	2.00	-2.40	0.59	-6.60	4.40	0.60	1.00	3.00	2.00	-5.00	5.10	4.50	-4.60
	(1.90)	(1.69)	(14.62)	(12.96)	(3.12)	(2.71)	(2.76)	(5.28)	(4.44)	(5.00)	(3.65)	(3.13)	(3.52)
Male	-2.60	-6.00	1.00	-7.70	4.30	-6.00	-5.00	1.30	0.10	-0.10	2.80	-6.50	-0.70
	(1.89)	(1.67)	(15.84)	(14.89)	(3.14)	(2.70)	(2.92)	(5.05)	(4.28)	(4.76)	(3.79)	(3.23)	(3.57)
P*Q3 (30%)													
Female	-3.40	-2.10	-1.90	-5.80	4.31	-0.60	-2.50	1.20	-4.00	-3.30	4.40	1.30	-2.90
	(1.34)	(1.20)	(8.16)	(7.14)	(2.52)	(2.13)	(2.21)	(3.23)	(2.70)	(3.08)	(2.93)	(2.61)	(2.73)
Male	-2.50	-3.29	0.00	-6.30	3.20	-5.40	-7.10	1.60	-3.30	-2.40	3.90	4.70	-4.60
	(1.32)	(1.17)	(8.44)	(7.28)	(2.53)	(2.17)	(2.27)	(3.21)	(2.72)	(3.05)	(2.88)	(2.45)	(2.68)
P*Q4 (53%)													
Female	-1.00	-6.70	0.00	-1.99	8.30	1.30	-5.50	4.70	-2.99	0.90	10.50	1.00	-0.70
	(1.25)	(1.10)	(6.00)	(5.00)	(2.61)	(2.27)	(2.27)	(2.86)	(2.44)	(2.74)	(2.54)	(2.40)	(2.42)
Male	0.60	-2.70	0.00	-2.00	15.80	0.10	-2.00	15.20	0.80	7.30	10.90	6.11	3.50
	(1.24)	(1.09)	(5.97)	(4.84)	(2.55)	(2.31)	(2.38)	(2.83)	(2.42)	(2.74)	(2.55)	(2.34)	(2.41)

Note: See note to Table C1.

scores in higher quartiles), with larger impacts for seventh than for eighth grade.

Table C3: Estimated parameters for the lower-secondary school-choice/dropout model (after grade 6)

	General	Tele	Technical
mathematics (6th grade)	0.0018	0.0004	0.002
	(0.0001)	(0.0001)	(0.0001)
Spanish (6th grade)	0.0027	0.0016	0.0029
	(0.0001)	(0.0001)	(0.0001)
Prospera effect by propensity-score quartile and gender			
P*Q1 (4%)			
Female	0.61	1.46	0.78
	(0.19)	(0.21)	(0.19)
Male	0.19	0.66	0.24
	(0.16)	(0.18)	(0.16)
P*Q2 (13%)			
Female	0.38	1.06	0.60
	(0.10)	(0.10)	(0.10)
Male	0.29	0.98	0.54
	(0.09)	(0.09)	(0.09)
P*Q3 (30%)			
Female	0.28	1.12	0.37
	(0.07)	(0.06)	(0.07)
Male	0.35	1.05	0.45
	(0.07)	(0.06)	(0.07)
P*Q4 (53%)			
Female	0.16	1.11	0.35
	(0.06)	(0.05)	(0.06)
Male	0.41	1.23	0.63
	(0.06)	(0.05)	(0.06)

Note: See note to Table C1.

C.3 Grade-retention model

Table C5 shows estimated coefficients for the probability of being retained (second and fifth columns) and also for the value-added model specifications that were estimated for retained children (taking the grade for the second time). The achievement test scores are not used in retention decisions, but one would expect them to be correlated with school performance. As seen in the second and fifth columns, students with higher test scores are less likely to be retained, in both primary and lower-secondary schools. Also, the retention probability does not appear to depend on *Prospera*-participation status or to vary by quartile. Gender is a significant determinant of retention with boys more likely to be retained than girls, a pattern widely found in developing countries (e.g., Grant and Behrman (2010)).

Table C4: Estimated parameters for the probability of dropping-out after grades 7 and 8

	Grade 7	Grade 8
Lag mathematics	-0.0015	-0.0007
	(0.0001)	(0.0001)
Lag Spanish	-0.0029	-0.0032
	(0.0001)	(0.0001)
Prospera effect by propensity-score quartile and gender		
P*Q1 (4%)		
Female	-0.16	-0.14
	(0.15)	(0.13)
Male	-0.47	-0.09
	(0.16)	(0.11)
P*Q2 (13%)		
Female	-0.47	-0.34
	(0.10)	(0.07)
Male	-0.38	-0.20
	(0.08)	(0.07)
P*Q3 (30%)		
Female	-0.32	-0.14
	(0.06)	(0.05)
Male	-0.42	-0.32
	(0.06)	(0.05)
P*Q4 (53%)		
Female	-0.23	-0.18
	(0.05)	(0.05)
Male	-0.58	-0.37
	(0.05)	(0.04)

Note: See note to Table C1

Table C5: Estimated model parameters for retained students

	Primary			Secondary		
	Prob Ret.	VA Math	VA Spanish	Prob Ret.	VA Math	VA Spanish
Lag mathematics	-0.0039	0.37	0.16	-0.0012	0.38	0.16
	(0.0001)	(0.03)	(0.02)	(0.0001)	(0.05)	(0.05)
Lag Spanish	0.0002	0.13	0.25	-0.0107	0.12	0.31
	(0.0001)	(0.03)	(0.03)	(0.0002)	(0.05)	(0.05)
Prospera effect by propensity-score quartile and gender						
P*Q1 (4%)						
	-0.05	30.59	6.13	-0.26	16.33	29.94
	(0.30)	(42.02)	(31.21)	(0.46)	(44.06)	(112.18)
	-0.22	-10.57	4.72	-0.01	-27.62	-11.72
	(0.21)	(21.29)	(17.86)	(0.27)	(51.30)	(40.62)
P*Q2 (13%)						
	-0.04	14.07	-4.78	-0.52	-16.33	-57.39
	(0.16)	(18.28)	(14.90)	(0.36)	(70.21)	(52.04)
	-0.11	-1.65	-2.85	-0.11	10.90	-22.15
	(0.12)	(10.13)	(9.73)	(0.18)	(20.68)	(20.88)
P*Q3 (30%)						
	-0.14	-7.17	-10.50	-0.08	20.50	-2.89
	(0.11)	(11.36)	(9.36)	(0.22)	(27.76)	(24.39)
	-0.10	-3.06	-2.59	-0.44	5.83	-0.96
	(0.08)	(7.58)	(6.40)	(0.14)	(18.50)	(17.22)
P*Q4 (53%)						
	-0.14	-1.55	-2.83	-0.65	58.52	6.46
	(0.08)	(8.14)	(7.05)	(0.22)	(24.08)	(25.54)
	-0.08	-2.05	-1.11	-0.56	0.03	-1.02
	(0.07)	(6.37)	(5.64)	(0.14)	(17.55)	(16.56)

Note: VA = value added. The probability of retention is estimated by a probit model. The notes from Table C1 also apply here.

D The selection of non-linear terms in the school-choice model

As described in section 3.5, our school-choice model can be viewed as an approximation to the difference in value functions from a dynamic schooling model (see, e.g. Heckman et al. (2016)). Such an approximation can be obtained, for example, from a discrete-choice dynamic-programming model with i.i.d. type 1 extreme-value preference shocks that are additive to utility. We are modeleing the decision to attend school or dropout and what type of school to attend, from grades 4 to grade 9. The last-period decision depends on the last-period test score achievements, as well as the expected value after leaving school, which is the continuation value. We now describe how we used the Bayesian Information Criterion (BIC) to determine the interaction terms used in specifying this approximation, given by equation 3 in the text.

We start with a general specification of the value function of individual i choosing option j at age a that allows for all possible quadratic terms and interactions among variables $\{Z_{ia}, S_{ija}, w_{ia}\}$

$$\begin{gathered}V^*_{ija} = \mu^g_{0jl} + A_{ia-1}\phi^g_{Aj} + P_i\phi^g_{pj} + Z^D_{ia}\phi^{1g}_{Zj} + w_{ia}\phi^{1g}_{wj} + S_{ija}\phi^{1g}_{Sj} \\ + \left(Z^D_{ia}\right)^2\phi^{2g}_{Zj} + w^2_{ia}\phi^{2g}_{wj} + S^2_{ija}\phi^{2g}_{Sj} \\ +\phi^{3g}_{ZSj}Int(Z_{ia}, S_{ija}) + \phi^{3g}_{Zwj}Int(Z_{ia}, w_{ia}) + \phi^{3g}_{Swj}Int(S_{ija}, w_{ia}) + \epsilon_{ija}\end{gathered}$$

The second line includes all potential quadratic terms, while the third line includes all potential interaction terms between the elements of Z_{ia}, w_{ia}, S_{ija}. First, we note that the quadratic terms for family characteristics Z^D_{ia} are not included, as Z^D_{ia} are categorical variables. Hence, any high-order non-linearity from Z^D_{ia} has already been accounted for by the distinct coefficients corresponding to each categorical level. However, quadratic terms $(w_{ia})^2\,\phi^{2g}_{wj}$ and $(S_{ija})^2\,\phi^{2g}_{Sj}$ are included.

The third line shows the potential interaction terms among the state variables $\{Z_{ia}, S_{ija}, w_{ia}\}$ that could be included in an approximation, which are more than 200. We therefore use model-selection criteria to select the the final model specification. It is not possible to consider all possible subsets of the potential variables, so we follow instead the following procedure: (i). We incorporate one interaction term at a time, ranking all interaction terms based on their contribution to the likelihood value when estimating equation 3. (ii). Then, we add the interaction terms in a sequential manner, according to the ranking of their individual likelihood contribution. The optimal cut-off of how many terms to include is then decided based on the Bayesian Information Criterion (BIC).

Following the procedure previously described and using the ordering obtained in D1 and examininig the BIC criterion, we arrive at the following specification that includes the top 10 interaction terms:

$$\begin{gathered}V^*_{ija} = \mu^g_{0jl} + A_{ia-1}\phi^g_{Aj} + P_i\phi^g_{pj} + Z^D_{ia}\phi^{1g}_{Zj} + w_{ia}\phi^{1g}_{wj} + S_{ija}\phi^{1g}_{Sj} + \left(Z^D_{ia}\right)^2\phi^{2g}_{Zj} + w^2_{ia}\phi^{2g}_{wj} + S^2_{ija}\phi^{2g}_{Sj} \\ +\phi^{1g}_{Ij} \times \text{Region}_i \times \text{Urban}_i + \phi^{2g}_{Ij} \times \text{Region}_i \times S^{distance}_{ija} + \phi^{3g}_{Ij} \times \text{Region}_i \times S^{number}_{ija} \\ +\phi^{4g}_{Ij} \times \text{Region}_i \times w_{ia} + \phi^{5g}_{Ij} \times \text{Urban}_i \times w_{ia} + \epsilon_{ija}.\end{gathered}$$

Table D1: The changes in BIC when adding interaction terms sequentially

Rank	Variable name	Likelihood	Improvement	BIC
	baseline	-67591	-	137039
1	urban:region	-67436	155.0	136830
2	region:distance technical	-67374	126.6	136807
3	region:number general	-67286	106.0	136732
4	region:distance general	-67251	98.2	136764
5	region:number technical	-67206	88.3	136774
6	region:distance tele	-67137	78.6	136738
7	region:imputed wage	-67079	77.7	136723
8	region:num telesecondary	-67018	69.7	136702
9	urban:imputed wage	-66943	68.5	136585
10	distance general:num general	-66895	68.5	136524
11	years of preschool:region	-66832	65.1	136802
12	dad edu degree:distance technical	-66776	61.3	136827
13	rank:region	-66751	58.9	137080
14	distance tele:distance technical	-66697	52.8	137005
15	dad edu degree:distance general	-66650	49.5	137047
16	dad edu degree:region	-66615	47.9	137381
17	mom edu degree:distance general	-66592	47.1	137471
18	dad edu degree:distance tele	-66559	43.7	137539
19	urban dummy:num general	-66550	43.3	137555
20	mom edu degree:region	-66526	42.8	137912

Note: The column titled "Likelihood" reports the log likelihood value obtained when estimating equation 3, with each interaction term included in isolation. The "Improvement" column shows the enhancement in the log-likelihood value compared to the baseline scenario, which does not incorporate any interaction terms. The interaction terms are subsequently ranked from the highest to the lowest improvement. Out of a total of 253 interaction terms, only the top 20, exhibiting the most-significant improvements, are reported in the table. The column labeled "BIC" displays the BIC values as interaction terms are sequentially added, with the lowest value being reached when having the first 10 interaction terms. Under "Variable Name" column, "baseline" refers to the baseline specification without any interaction terms. "Urban" denotes a dummy variable indicating urban or rural areas, while "region" signifies the geographical location of individuals—whether they live in the north, north-center, center, or south areas. "Number general" represents the log value of the total number of general schools, "number tele" represents the log value of the total number of telesecondary schools, and "number technical" represents the log value of the total number of technical schools. Furthermore, "distance general" indicates the distance to the nearest general school, "distance tele" indicates the distance to the nearest telesecondary school, and "distance technical" indicates the distance to the nearest technical school.

E Model estimates (full parameters)

This appendix shows the full set of model parameter estimates. Tables E1 and E2 show the estimated parameters of the value-added production functions for mathematics and Spanish. Table E3 reports estimated coefficients from primary and lower-secondary school choices. Table E4 shows estimated coefficients for the probability of being retained and also for the value-added model specifications that were estimated for retained children (who were taking the grade for the second time). Table E5 reports estimated coefficients from the dropout decision during lower-secondary school choices, where the omitted category is dropping out and working. Lastly, Table E6 shows the coefficients associated with the cheating equation. See further discussion in the text.

Table E1: Mathematics value-added estimates

	General		Indigenous		General			Telesecondary			Technical		
	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
Lag Mathematics	0.54	0.53	0.40	0.43	0.43	0.46	0.47	0.32	0.41	0.41	0.40	0.46	0.46
	(0.00)	(0.00)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Spanish	0.16	0.13	0.14	0.16	0.26	0.18	0.28	0.25	0.17	0.24	0.27	0.16	0.26
	(0.01)	(0.00)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Prospera by score quartile and gender													
P*Q1 (4%)	-3.30	5.50	5.00	10.01	3.20	-1.20	8.30	9.00	-2.01	6.79	7.30	4.60	1.70
Female	(3.70)	(3.71)	(47.25)	(54.46)	(4.97)	(4.53)	(5.39)	(12.63)	(12.90)	(12.39)	(7.20)	(5.99)	(7.41)
	5.20	0.30	-1.00	10.00	3.30	-2.70	3.31	9.00	-4.30	8.10	5.70	1.70	11.80
Male	(3.48)	(3.52)	(44.38)	(29.42)	(4.89)	(4.45)	(5.74)	(13.86)	(12.15)	(14.87)	(7.04)	(6.57)	(8.09)
P*Q2 (13%)													
Female	4.40	7.30	10.30	7.00	7.90	0.90	7.80	0.00	0.51	1.50	12.10	5.30	10.50
	(2.07)	(2.05)	(18.41)	(15.58)	(3.24)	(2.88)	(3.43)	(6.84)	(5.45)	(6.16)	(4.42)	(3.34)	(4.37)
Male	-0.90	4.60	-3.00	11.30	6.90	3.10	2.30	-1.00	-1.00	9.50	4.50	2.10	6.60
	(1.99)	(2.04)	(18.85)	(15.71)	(3.24)	(2.89)	(3.57)	(6.50)	(5.55)	(6.42)	(4.26)	(3.84)	(4.75)
P*Q3 (30%)													
Female	-0.10	6.10	-6.90	5.40	6.70	2.80	6.40	3.70	-0.90	7.80	8.80	5.10	13.30
	(1.41)	(1.42)	(9.03)	(8.29)	(2.56)	(2.23)	(2.54)	(4.05)	(3.29)	(3.86)	(3.31)	(2.76)	(3.35)
Male	-0.90	7.20	-7.60	-0.50	5.30	-2.21	5.20	3.90	-0.70	8.80	8.20	6.10	5.21
	(1.38)	(1.43)	(9.71)	(8.99)	(2.46)	(2.37)	(2.78)	(3.97)	(3.57)	(4.02)	(3.22)	(2.80)	(3.46)
P*Q4 (53%)													
Female	1.70	3.50	-3.20	-0.70	15.21	9.90	11.20	13.00	7.10	15.40	17.40	13.50	19.00
	(1.30)	(1.33)	(6.55)	(5.91)	(2.61)	(2.22)	(2.57)	(3.54)	(2.97)	(3.46)	(2.73)	(2.43)	(2.97)
Male	0.10	4.60	-7.90	-1.70	16.70	6.10	8.90	15.10	8.20	16.00	13.00	13.30	17.11
	(1.28)	(1.35)	(6.51)	(5.88)	(2.53)	(2.33)	(2.70)	(3.51)	(3.08)	(3.61)	(2.79)	(2.48)	(3.04)

Table E1: Mathematics value-added estimates

	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
Education cat. (dad)													
Below primary school	-3.67	-0.83	-2.99	-5.66	-7.23	2.03	-2.83	-13.47	6.94	3.03	0.25	-1.98	-2.60
	(2.18)	(2.21)	(11.17)	(10.80)	(3.59)	(3.27)	(3.89)	(6.87)	(6.11)	(7.12)	(4.62)	(3.97)	(4.89)
Primary school completed	-2.73	0.90	-1.49	-6.57	-6.24	2.05	-1.23	-8.04	6.68	3.22	2.39	0.83	-0.11
	(2.20)	(2.22)	(11.33)	(10.96)	(3.59)	(3.27)	(3.87)	(6.96)	(6.18)	(7.21)	(4.64)	(3.99)	(4.89)
Secondary or below	-1.90	1.58	-4.48	1.24	-1.78	3.68	-0.27	-6.12	9.82	6.91	3.16	3.20	1.17
	(2.15)	(2.18)	(11.58)	(11.05)	(3.50)	(3.19)	(3.78)	(6.95)	(6.18)	(7.19)	(4.54)	(3.89)	(4.74)
College or above	4.66	7.51	2.72	-3.18	3.03	8.44	3.87	-2.82	14.80	9.01	8.23	6.74	6.49
	(2.25)	(2.28)	(12.87)	(12.17)	(3.62)	(3.27)	(3.89)	(7.72)	(6.78)	(7.87)	(4.71)	(4.03)	(4.91)
Working status (dad)													
Full time	3.87	2.81	-9.98	1.32	-0.15	-2.21	1.07	11.29	0.82	9.84	-0.82	-0.40	9.03
	(1.75)	(1.77)	(8.97)	(8.22)	(2.88)	(2.58)	(3.12)	(5.34)	(4.53)	(5.49)	(3.80)	(3.31)	(3.96)
Not full time	2.90	1.56	-4.22	0.48	-0.04	-3.10	-0.47	9.30	-2.81	9.98	-1.14	-0.62	4.20
	(1.67)	(1.69)	(8.65)	(7.98)	(2.76)	(2.45)	(2.98)	(5.17)	(4.38)	(5.31)	(3.66)	(3.19)	(3.77)
Father present													
At home	5.16	5.48	3.32	-2.95	5.37	4.96	2.23	8.28	2.18	2.73	9.95	7.89	7.17
	(1.24)	(1.21)	(5.80)	(5.29)	(2.02)	(1.81)	(2.14)	(3.59)	(3.15)	(3.82)	(2.60)	(2.29)	(2.87)
Not at home	5.97	4.75	9.08	0.34	9.79	5.43	2.93	7.75	3.76	2.63	10.22	6.40	6.04
	(0.96)	(0.94)	(4.26)	(3.81)	(1.58)	(1.44)	(1.67)	(2.71)	(2.33)	(2.86)	(1.99)	(1.77)	(2.20)
Education cat. (mom)													
Primary school	1.98	3.56	-16.84	8.70	0.50	10.65	-2.48	16.35	-18.39	17.78	6.95	-3.11	8.73
	(4.06)	(3.93)	(17.77)	(15.95)	(7.29)	(6.15)	(7.66)	(12.76)	(9.12)	(12.24)	(8.93)	(6.28)	(8.75)
Primary school completed	3.92	6.30	-11.25	16.06	1.08	12.17	-3.91	18.82	-15.68	17.30	8.89	-6.34	9.17
	(4.07)	(3.94)	(17.96)	(16.13)	(7.29)	(6.16)	(7.66)	(12.80)	(9.18)	(12.27)	(8.93)	(6.29)	(8.78)
Secondary or below	4.78	8.06	-10.89	19.49	3.02	13.15	-4.60	20.17	-13.48	20.01	9.64	-5.92	8.09
	(4.06)	(3.93)	(18.25)	(16.37)	(7.27)	(6.13)	(7.63)	(12.83)	(9.21)	(12.29)	(8.90)	(6.27)	(8.74)

Table E1: Mathematics value-added estimates

	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
College or above	10.51	10.74	-13.60	22.74	8.57	15.85	-1.26	25.89	-8.54	24.79	14.51	-2.53	12.81
	(4.14)	(4.01)	(19.75)	(17.82)	(7.35)	(6.21)	(7.71)	(13.45)	(9.86)	(12.93)	(9.03)	(6.42)	(8.87)
Working status (mom)													
Housework	2.07	0.02	8.84	-1.80	2.46	4.96	2.07	-2.34	-3.78	2.14	9.01	8.28	3.13
	(2.94)	(2.98)	(12.38)	(9.92)	(4.98)	(4.60)	(5.37)	(8.67)	(7.26)	(8.68)	(6.37)	(5.50)	(7.15)
Part time	0.74	-0.85	8.65	-7.16	-0.64	5.40	0.03	-3.92	-4.88	-0.49	7.26	4.97	0.10
	(3.00)	(3.03)	(12.77)	(10.37)	(5.05)	(4.65)	(5.44)	(8.97)	(7.52)	(8.99)	(6.46)	(5.57)	(7.24)
Full time	2.80	0.28	1.74	-12.52	1.77	4.45	2.34	-2.00	-6.21	5.49	6.70	7.08	0.40
	(3.05)	(3.08)	(13.24)	(10.82)	(5.11)	(4.71)	(5.50)	(9.14)	(7.69)	(9.18)	(6.56)	(5.66)	(7.35)
Mother present													
At home	2.38	-0.78	6.12	-2.19	-5.36	2.81	3.89	-0.95	4.85	7.33	-9.09	-5.45	5.03
	(2.29)	(2.21)	(8.33)	(7.41)	(3.85)	(3.47)	(4.17)	(5.90)	(4.99)	(6.09)	(4.91)	(4.33)	(5.22)
Not at home	2.44	0.17	-1.12	3.74	0.23	2.80	-1.46	1.70	-0.98	3.07	-1.86	-2.21	-0.06
	(1.36)	(1.33)	(5.92)	(5.29)	(2.26)	(2.01)	(2.41)	(3.89)	(3.29)	(4.07)	(2.82)	(2.45)	(3.10)
Number of people at home													
4 people	1.98	1.16	2.37	14.30	1.85	0.93	2.90	3.03	-0.62	2.01	1.66	1.11	2.42
	(0.84)	(0.82)	(4.97)	(4.46)	(1.29)	(1.14)	(1.37)	(2.60)	(2.23)	(2.62)	(1.69)	(1.47)	(1.78)
5 people	1.52	0.87	0.21	6.78	1.13	1.63	3.38	3.75	-1.67	3.57	0.27	-0.76	5.07
	(0.92)	(0.90)	(4.73)	(4.28)	(1.45)	(1.29)	(1.55)	(2.74)	(2.33)	(2.73)	(1.89)	(1.64)	(2.00)
$\geq$ 6 people	0.41	-0.04	-2.48	5.38	-1.03	-2.43	1.62	0.91	-2.87	4.83	-1.48	-0.09	1.43
	(0.80)	(0.78)	(3.89)	(3.44)	(1.27)	(1.14)	(1.37)	(2.30)	(1.98)	(2.34)	(1.67)	(1.46)	(1.79)
Age	2.66	0.94	2.78	-2.57	1.07	4.28	-2.09	-1.05	-3.00	2.36	1.84	4.74	1.40
	(1.04)	(1.02)	(5.13)	(4.39)	(1.70)	(1.51)	(1.84)	(3.13)	(2.71)	(3.27)	(2.14)	(1.91)	(2.41)
Age^2	-2.97	-2.72	-2.46	-0.18	-2.96	-3.86	-2.23	-2.06	-1.46	-3.42	-2.31	-3.39	-3.90
	(0.37)	(0.34)	(1.60)	(1.28)	(0.65)	(0.62)	(0.82)	(1.02)	(0.90)	(1.20)	(0.77)	(0.73)	(1.06)
Male	3.41	2.39	5.60	0.50	7.08	17.74	11.32	-0.22	9.89	0.77	5.67	18.27	9.54

Table E1: Mathematics value-added estimates

	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
	(0.75)	(0.74)	(7.33)	(6.79)	(1.12)	(1.00)	(1.19)	(3.11)	(2.65)	(3.07)	(1.53)	(1.33)	(1.60)
First language spoken at home													
Indigenous	-6.19	-5.07	-6.49	-6.78	-9.68	-2.19	-11.79	3.25	2.28	19.16	8.00	13.29	14.13
	(2.35)	(2.23)	(3.66)	(3.25)	(4.64)	(3.94)	(5.07)	(3.45)	(3.08)	(4.02)	(3.47)	(2.98)	(3.84)
Both Spanish and indigenous	-0.85	-4.03	4.08	5.99	-6.04	4.21	2.14	3.45	9.99	7.89	5.50	0.01	14.24
	(2.25)	(2.22)	(4.64)	(4.12)	(4.32)	(3.78)	(4.43)	(4.82)	(3.95)	(4.38)	(4.12)	(3.67)	(4.28)
Internet access	-6.58	-7.75	-6.87	-7.13	-9.15	-6.46	-8.42	-16.64	-3.75	-14.87	-8.08	-6.44	-5.00
	(0.93)	(0.92)	(5.24)	(4.65)	(1.44)	(1.28)	(1.54)	(3.20)	(2.70)	(3.30)	(1.86)	(1.66)	(1.97)
Computer access	1.48	4.16	-5.31	0.84	3.43	4.08	3.92	-4.41	0.48	-1.98	1.21	3.73	2.83
	(0.83)	(0.81)	(4.75)	(4.09)	(1.26)	(1.12)	(1.34)	(2.71)	(2.31)	(2.75)	(1.66)	(1.46)	(1.73)
Number of pre-school years													
1 year	5.27	-1.18	-2.73	9.65	-3.09	-5.06	3.48	3.45	3.82	-7.74	1.34	-6.12	0.81
	(2.22)	(2.18)	(9.44)	(8.41)	(3.86)	(3.31)	(4.24)	(6.02)	(5.03)	(6.34)	(4.52)	(4.10)	(5.36)
2 years	4.50	1.24	9.89	12.26	0.65	-4.62	2.70	1.50	3.70	-3.99	2.61	-8.49	2.26
	(2.04)	(2.00)	(9.15)	(8.24)	(3.51)	(2.91)	(3.78)	(5.65)	(4.69)	(5.87)	(4.09)	(3.68)	(4.95)
3 years	5.40	1.32	0.76	11.54	2.27	-1.85	2.18	7.20	4.23	-1.40	6.46	-4.24	5.10
	(2.02)	(1.98)	(9.04)	(8.07)	(3.47)	(2.86)	(3.73)	(5.60)	(4.63)	(5.82)	(4.04)	(3.63)	(4.90)
4 years	6.52	0.59	-5.14	3.90	-0.99	-0.73	4.75	8.21	6.97	-0.56	3.07	-0.18	2.11
	(2.05)	(2.00)	(8.71)	(7.85)	(3.52)	(2.91)	(3.78)	(5.58)	(4.61)	(5.81)	(4.09)	(3.67)	(4.94)
Urban dummy	-1.02	3.24	12.51	26.04	-9.49	-6.41	-2.96	-15.72	-6.29	-4.02	-10.04	-4.41	-6.07
	(0.76)	(0.77)	(3.96)	(3.52)	(1.46)	(1.30)	(1.54)	(2.19)	(1.91)	(2.20)	(1.69)	(1.48)	(1.77)
Regions													
North-center	-3.18	-2.03	1.23	12.27	-1.89	-2.92	-6.11	-42.13	-30.33	-16.80	2.52	0.50	3.19
	(0.85)	(0.84)	(4.53)	(4.31)	(1.32)	(1.16)	(1.43)	(2.74)	(2.36)	(2.92)	(1.78)	(1.52)	(1.95)
Center	-1.16	-4.20	-7.80	-9.27	-7.30	7.70	7.33	-51.22	-25.96	-4.59	-0.92	0.88	14.91
	(1.06)	(1.05)	(4.83)	(4.49)	(1.71)	(1.44)	(1.72)	(3.14)	(2.78)	(3.28)	(2.36)	(1.99)	(2.37)

Table E1: Mathematics value-added estimates

	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
South	2.42	1.45	3.93	10.08	-0.45	-2.07	-4.22	-32.66	-20.97	-11.53	11.23	4.98	7.29
	(0.90)	(0.88)	(3.92)	(3.41)	(1.44)	(1.30)	(1.52)	(2.70)	(2.32)	(2.88)	(1.75)	(1.56)	(1.89)
Copying dummy	43.15	46.21	68.50	53.89	59.08	79.95	73.93	89.58	101.06	49.10	74.48	79.01	60.61
	(1.29)	(1.42)	(6.12)	(6.21)	(3.16)	(1.90)	(2.91)	(4.61)	(2.71)	(3.49)	(3.60)	(2.09)	(2.75)
Unobserved types													
Type I	3.04	10.58	-3.03	-6.22	18.73	0.98	2.05	10.13	8.15	9.11	28.78	21.26	34.43
	(1.23)	(1.22)	(5.97)	(5.32)	(2.25)	(2.04)	(2.22)	(3.69)	(3.14)	(3.60)	(2.34)	(2.06)	(2.41)
Type II	3.41	8.26	-2.01	-1.52	5.66	5.44	2.34	4.55	4.55	3.84	13.47	7.37	3.21
	(1.69)	(1.65)	(8.66)	(7.50)	(2.95)	(2.69)	(2.99)	(5.43)	(4.69)	(5.23)	(3.25)	(2.97)	(3.32)
Type III	-3.99	5.18	-1.83	34.23	-5.05	-6.38	-3.55	-24.46	-14.54	-7.72	-0.68	-3.84	-4.576
	(2.06)	(2.00)	(9.22)	(7.80)	(3.36)	(3.21)	(3.66)	(6.91)	(5.59)	(6.62)	(3.89)	(3.61)	(4.16)
Intercept term	141.07	189.81	240.04	206.84	100.76	176.16	159.73	218.54	295.57	213.37	84.88	193.30	131.38
	(5.66)	(5.55)	(24.98)	(23.13)	(9.93)	(8.41)	(10.22)	(17.37)	(12.97)	(17.14)	(12.44)	(8.99)	(12.88)
Standard error (σ)	82.05	83.05	84.85	83.68	89.47	85.17	97.18	114.15	106.93	116.40	91.64	85.08	98.56
	(0.22)	(0.23)	(1.10)	(1.44)	(0.46)	(0.31)	(0.36)	(0.78)	(0.62)	(0.71)	(0.59)	(0.41)	(0.58)

Table E2: Spanish value-added estimates

	General		Indigenous		General			Telesecondary			Technical		
	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
Lag Mathematics	0.22	0.20	0.24	0.18	0.13	0.23	0.19	0.08	0.17	0.15	0.12	0.22	0.18
	(0.00)	(0.00)	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Spanish	0.42	0.39	0.28	0.34	0.52	0.47	0.48	0.43	0.40	0.40	0.53	0.47	0.48
	(0.00)	(0.00)	(0.02)	(0.02)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Prospera by score quartile and gender													
P*Q1 (4%)													
Female	-1.30	-0.60	6.00	4.00	2.80	-7.10	2.90	10.80	-2.10	5.00	4.01	-1.00	-4.70
	(3.33)	(3.10)	(32.30)	(68.93)	(4.75)	(4.32)	(4.43)	(9.85)	(9.48)	(11.13)	(5.99)	(5.60)	(6.38)
Male	6.20	-1.10	0.00	3.00	1.30	-8.80	-3.50	13.50	-6.20	-6.00	-1.80	3.60	-7.50
	(3.26)	(2.95)	(28.50)	(30.14)	(4.85)	(3.90)	(4.44)	(10.67)	(9.27)	(10.12)	(6.38)	(5.49)	(5.80)
P*Q2 (13%)													
Female	2.00	-2.40	0.59	-6.60	4.40	0.60	1.00	3.00	2.00	-5.00	5.10	4.50	-4.60
	(1.90)	(1.69)	(14.62)	(12.96)	(3.12)	(2.71)	(2.76)	(5.28)	(4.44)	(5.00)	(3.65)	(3.13)	(3.52)
Male	-2.60	-6.00	1.00	-7.70	4.30	-6.00	-5.00	1.30	0.10	-0.10	2.80	-6.50	-0.70
	(1.89)	(1.67)	(15.84)	(14.89)	(3.14)	(2.70)	(2.92)	(5.05)	(4.28)	(4.76)	(3.79)	(3.23)	(3.57)
P*Q3 (30%)													
Female	-3.40	-2.10	-1.90	-5.80	4.31	-0.60	-2.50	1.20	-4.00	-3.30	4.40	1.30	-2.90
	(1.34)	(1.20)	(8.16)	(7.14)	(2.52)	(2.13)	(2.21)	(3.23)	(2.70)	(3.08)	(2.93)	(2.61)	(2.73)
Male	-2.50	-3.29	0.00	-6.30	3.20	-5.40	-7.10	1.60	-3.30	-2.40	3.90	4.70	-4.60
	(1.32)	(1.17)	(8.44)	(7.28)	(2.53)	(2.17)	(2.27)	(3.21)	(2.72)	(3.05)	(2.88)	(2.45)	(2.68)
P*Q4 (53%)													
Female	-1.00	-6.70	0.00	-1.99	8.30	1.30	-5.50	4.70	-2.99	0.90	10.50	1.00	-0.70
	(1.25)	(1.10)	(6.00)	(5.00)	(2.61)	(2.27)	(2.27)	(2.86)	(2.44)	(2.74)	(2.54)	(2.40)	(2.42)
Male	0.60	-2.70	0.00	-2.00	15.80	0.10	-2.00	15.20	0.80	7.30	10.90	6.11	3.50
	(1.24)	(1.09)	(5.97)	(4.84)	(2.55)	(2.31)	(2.38)	(2.83)	(2.42)	(2.74)	(2.55)	(2.34)	(2.41)

Table E2: Spanish value-added estimates

	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
Education cat. (dad)													
Below primary school	-3.91	-0.41	-7.44	8.23	-7.38	1.07	-2.01	-14.04	-5.46	-2.60	-2.19	-0.25	-1.01
	(2.06)	(1.83)	(10.23)	(9.84)	(3.34)	(2.97)	(3.23)	(5.54)	(4.83)	(5.62)	(4.05)	(3.65)	(3.90)
Primary school completed	-2.92	1.29	-4.25	6.86	-6.46	2.13	-0.81	-9.76	-6.10	-0.89	-0.49	-0.35	2.04
	(2.07)	(1.85)	(10.39)	(9.95)	(3.35)	(2.96)	(3.22)	(5.61)	(4.89)	(5.68)	(4.07)	(3.66)	(3.90)
Secondary or below	-0.69	2.69	-6.04	12.24	-3.35	2.94	1.63	-8.06	-3.38	3.91	0.40	2.84	1.03
	(2.03)	(1.81)	(10.55)	(10.03)	(3.25)	(2.88)	(3.13)	(5.60)	(4.89)	(5.65)	(3.96)	(3.55)	(3.79)
College or above	5.52	7.44	-4.67	11.06	4.83	7.00	4.14	-1.17	0.37	6.48	7.23	8.20	5.21
	(2.12)	(1.90)	(11.59)	(11.00)	(3.35)	(2.97)	(3.22)	(6.26)	(5.34)	(6.17)	(4.10)	(3.68)	(3.90)
Working status (dad)													
Full time	2.42	-1.69	-8.55	-11.33	0.19	-2.92	-2.00	6.71	0.57	1.78	1.94	-3.93	1.22
	(1.64)	(1.48)	(7.50)	(7.44)	(2.76)	(2.43)	(2.66)	(4.41)	(3.61)	(4.25)	(3.25)	(2.98)	(3.17)
Not full time	1.73	-1.85	-4.64	-10.98	1.37	-2.12	-2.23	6.15	0.56	3.39	0.35	-1.11	1.59
	(1.57)	(1.42)	(7.23)	(7.25)	(2.63)	(2.31)	(2.55)	(4.26)	(3.48)	(4.11)	(3.12)	(2.86)	(3.04)
Father present													
At home	2.50	4.98	3.07	-2.79	6.30	2.46	4.38	5.58	1.91	4.38	8.77	5.70	5.68
	(1.18)	(1.03)	(5.07)	(4.43)	(1.92)	(1.69)	(1.81)	(2.98)	(2.56)	(2.90)	(2.31)	(2.07)	(2.24)
Not at home	3.28	3.61	5.23	0.58	7.51	2.16	4.06	5.30	2.87	1.47	8.81	5.44	4.67
	(0.92)	(0.80)	(3.80)	(3.27)	(1.53)	(1.34)	(1.43)	(2.26)	(1.90)	(2.19)	(1.80)	(1.60)	(1.72)
Education cat. (mom)													
Primary school	-3.68	0.51	-8.38	0.96	3.38	-0.95	1.72	7.30	-7.82	4.28	9.67	-1.40	7.10
	(3.67)	(3.23)	(15.91)	(12.76)	(6.42)	(5.56)	(6.35)	(9.77)	(7.41)	(8.99)	(7.32)	(6.45)	(7.08)
Primary school completed	-1.33	1.82	-6.39	8.17	2.61	2.76	1.01	8.53	-5.46	3.93	10.11	-2.22	8.38
	(3.67)	(3.24)	(16.05)	(12.90)	(6.41)	(5.55)	(6.35)	(9.80)	(7.44)	(9.02)	(7.31)	(6.45)	(7.07)
Secondary or below	-0.29	4.70	-2.90	10.24	4.38	2.55	0.99	10.47	-7.01	6.34	10.61	-1.41	10.10
	(3.66)	(3.23)	(16.28)	(13.09)	(6.38)	(5.53)	(6.32)	(9.82)	(7.47)	(9.04)	(7.28)	(6.42)	(7.04)

Table E2: Spanish value-added estimates

	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
College or above	4.96	9.54	-9.57	13.74	10.72	8.03	6.25	17.93	5.74	10.07	15.06	3.60	12.60
	(3.74)	(3.30)	(17.45)	(14.44)	(6.46)	(5.60)	(6.38)	(10.37)	(7.90)	(9.56)	(7.39)	(6.52)	(7.13)
Working status (mom)													
Housework	2.39	-1.17	10.47	2.20	5.87	10.05	0.33	0.56	-3.98	7.09	6.80	-0.41	0.62
	(2.78)	(2.51)	(10.98)	(9.72)	(4.82)	(4.13)	(4.38)	(7.08)	(5.88)	(6.96)	(5.65)	(4.64)	(5.27)
Part time	0.85	-1.82	8.35	-4.74	5.47	10.13	0.09	2.72	-5.70	4.75	4.73	-1.81	2.07
	(2.83)	(2.55)	(11.34)	(10.07)	(4.88)	(4.17)	(4.44)	(7.33)	(6.07)	(7.19)	(5.73)	(4.71)	(5.36)
Full time	2.74	-1.43	-0.71	-2.48	4.03	9.25	1.04	1.18	-9.14	7.19	3.70	-1.22	1.60
	(2.88)	(2.59)	(11.75)	(10.28)	(4.94)	(4.23)	(4.49)	(7.46)	(6.19)	(7.31)	(5.80)	(4.80)	(5.43)
Mother present													
At home	0.16	-4.37	8.54	-9.27	-10.28	1.53	-1.39	-1.76	0.21	-3.86	-5.46	-5.18	-3.88
	(2.20)	(1.88)	(7.60)	(6.55)	(3.78)	(3.29)	(3.61)	(4.82)	(4.20)	(4.72)	(4.35)	(3.85)	(4.18)
Not at home	1.31	-1.86	1.04	-3.07	-1.38	1.43	0.11	-0.16	-0.64	0.35	-1.61	-0.51	0.72
	(1.30)	(1.11)	(5.54)	(4.71)	(2.18)	(1.87)	(2.04)	(3.24)	(2.69)	(3.11)	(2.59)	(2.23)	(2.47)
Number of people at home													
4 people	0.48	-0.01	1.28	10.00	-0.31	-1.29	2.43	3.06	2.72	3.28	-0.21	2.56	-0.29
	(0.78)	(0.69)	(4.51)	(3.85)	(1.23)	(1.06)	(1.14)	(2.11)	(1.78)	(2.03)	(1.49)	(1.33)	(1.42)
5 people	0.14	-1.63	3.74	2.80	-0.39	-1.42	0.40	3.17	0.12	2.54	-0.17	-1.46	2.42
	(0.86)	(0.76)	(4.18)	(3.63)	(1.39)	(1.20)	(1.29)	(2.24)	(1.89)	(2.11)	(1.69)	(1.50)	(1.61)
$\geq$ 6 people	-1.76	-2.79	-1.70	3.76	-3.86	-3.54	0.19	1.68	0.75	0.62	-4.22	-1.51	-2.18
	(0.75)	(0.66)	(3.49)	(2.96)	(1.21)	(1.05)	(1.13)	(1.88)	(1.60)	(1.82)	(1.48)	(1.32)	(1.42)
Age	4.25	2.17	-3.08	-2.94	4.00	4.16	2.51	-1.17	0.57	0.92	2.12	5.74	3.80
	(0.99)	(0.85)	(4.55)	(3.69)	(1.61)	(1.38)	(1.53)	(2.51)	(2.15)	(2.52)	(1.92)	(1.71)	(1.93)
Age^2	-3.24	-2.82	-0.20	-0.46	-3.81	-4.03	-5.30	-1.44	-2.15	-3.43	-2.28	-4.22	-5.34
	(0.36)	(0.29)	(1.45)	(1.07)	(0.64)	(0.56)	(0.69)	(0.82)	(0.72)	(0.93)	(0.72)	(0.64)	(0.85)
Male	-16.81	-16.16	-3.99	-14.24	-26.70	-15.36	-12.28	-30.31	-20.63	-17.24	-26.21	-16.84	-14.09

Table E2: Spanish value-added estimates

	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
	(0.69)	(0.62)	(6.64)	(5.56)	(1.05)	(0.91)	(0.98)	(2.49)	(2.08)	(2.39)	(1.32)	(1.18)	(1.27)
First language spoken at home													
Indigenous	-11.78	-9.19	-8.96	-11.27	1.76	-8.02	-13.07	-6.13	-7.85	-0.94	6.51	2.91	-18.12
	(2.33)	(1.93)	(3.27)	(2.79)	(4.66)	(3.99)	(4.71)	(2.91)	(2.55)	(2.98)	(3.52)	(3.08)	(3.30)
Both Spanish and indigenous	-3.00	-6.84	0.47	1.50	-6.11	0.68	-4.15	4.18	-0.22	-3.31	6.11	-0.58	-0.04
	(2.12)	(1.87)	(4.10)	(3.52)	(4.26)	(3.57)	(3.86)	(3.90)	(3.16)	(3.47)	(4.14)	(3.55)	(3.52)
Internet access	-5.95	-6.75	-4.52	-8.32	-6.68	-5.77	-5.78	-13.68	-9.43	-12.28	-5.03	-7.35	-5.00
	(0.87)	(0.77)	(4.76)	(4.13)	(1.36)	(1.16)	(1.26)	(2.59)	(2.18)	(2.52)	(1.65)	(1.48)	(1.60)
Computer access	1.83	3.85	-2.71	-4.99	2.27	2.20	3.33	-6.03	-0.88	-2.36	1.67	3.55	2.55
	(0.77)	(0.68)	(4.29)	(3.64)	(1.20)	(1.03)	(1.11)	(2.21)	(1.84)	(2.11)	(1.46)	(1.31)	(1.41)
Number of pre-school years													
1 year	4.64	-2.61	0.34	4.32	-1.10	-2.78	-5.13	-1.21	5.33	-2.81	3.43	0.89	-2.08
	(2.08)	(1.82)	(9.05)	(7.43)	(3.74)	(3.18)	(3.43)	(4.86)	(4.06)	(4.90)	(4.25)	(3.85)	(4.12)
2 years	5.25	1.05	5.41	12.49	0.05	-0.15	-0.59	-0.42	6.52	1.18	8.49	-0.14	3.40
	(1.90)	(1.66)	(8.80)	(7.25)	(3.36)	(2.80)	(3.03)	(4.53)	(3.76)	(4.56)	(3.83)	(3.43)	(3.69)
3 years	5.91	1.62	2.36	10.26	2.82	1.63	-0.29	2.89	7.20	1.49	10.87	1.84	5.32
	(1.88)	(1.64)	(8.74)	(7.15)	(3.32)	(2.76)	(2.98)	(4.49)	(3.71)	(4.51)	(3.79)	(3.38)	(3.65)
4 years	6.67	0.12	-3.77	4.42	1.62	3.73	0.60	4.89	5.58	2.10	7.97	2.42	5.44
	(1.91)	(1.66)	(8.47)	(6.94)	(3.37)	(2.81)	(3.03)	(4.47)	(3.70)	(4.51)	(3.83)	(3.44)	(3.69)
Urban dummy	0.85	4.77	11.81	19.93	-5.81	-2.41	1.45	-5.96	-2.16	6.73	-7.08	-3.42	0.86
	(0.73)	(0.65)	(3.47)	(2.96)	(1.44)	(1.29)	(1.30)	(1.78)	(1.52)	(1.71)	(1.54)	(1.39)	(1.46)
Regions													
North-center	-2.15	-3.17	9.13	22.11	-2.39	-3.74	-5.08	-26.94	-9.83	-14.15	-3.20	-3.12	-3.08
	(0.79)	(0.71)	(4.17)	(3.67)	(1.26)	(1.07)	(1.18)	(2.23)	(1.93)	(2.18)	(1.55)	(1.36)	(1.49)
Center	-1.97	-5.67	7.55	10.31	-7.44	-0.78	1.04	-37.76	-13.98	-8.90	-2.92	-6.43	5.11
	(0.99)	(0.88)	(4.17)	(3.66)	(1.63)	(1.37)	(1.48)	(2.58)	(2.22)	(2.50)	(1.97)	(1.72)	(1.84)

Table E2: Spanish value-added estimates

	G5	G6	G5	G6	G7	G8	G9	G7	G8	G9	G7	G8	G9
South	-0.87	-0.56	16.69	18.84	-7.07	-2.37	-6.24	-20.49	-4.55	-8.16	2.11	2.46	-2.90
	(0.85)	(0.75)	(3.45)	(2.95)	(1.39)	(1.21)	(1.28)	(2.21)	(1.91)	(2.15)	(1.58)	(1.44)	(1.49)
Copying dummy	26.65	29.35	49.75	37.14	35.03	45.95	26.13	55.28	65.58	19.68	47.84	42.20	25.80
	(1.28)	(1.14)	(5.25)	(4.72)	(2.92)	(1.79)	(2.53)	(3.70)	(2.11)	(2.82)	(3.53)	(2.06)	(2.41)
Unobserved types													
Type I	1.36	-3.45	-4.88	-10.77	22.76	-3.01	3.03	9.37	8.92	3.47	16.02	3.81	8.77
	(1.14)	(1.04)	(5.57)	(4.59)	(2.28)	(1.94)	(1.89)	(2.94)	(2.47)	(2.75)	(2.17)	(2.00)	(2.06)
Type II	1.53	0.50	-5.10	-6.08	3.09	-0.36	-2.38	5.73	7.40	-2.02	9.79	-4.68	-3.63
	(1.60)	(1.39)	(8.22)	(6.55)	(2.94)	(2.56)	(2.52)	(4.31)	(3.67)	(4.01)	(3.10)	(2.87)	(2.85)
Type III	0.56	-5.13	2.34	32.53	1.31	-3.35	1.80	-6.67	-1.53	1.78	0.03	-3.06	3.92
	(1.97)	(1.67)	(8.43)	(7.09)	(3.35)	(2.87)	(3.05)	(5.37)	(4.32)	(5.06)	(3.88)	(3.44)	(3.54)
Intercept term	188.69	241.27	242.83	245.48	127.08	141.92	158.49	231.93	242.31	212.21	112.37	157.36	143.27
	(4.85)	(4.31)	(21.57)	(17.12)	(8.91)	(7.74)	(8.42)	(12.94)	(10.52)	(12.25)	(10.37)	(8.98)	(9.85)
Standard error (\sigma)	76.61	71.37	76.05	70.33	83.40	79.14	80.41	93.50	86.32	90.25	85.05	80.74	82.59
	(0.20)	(0.19)	(0.96)	(1.28)	(0.52)	(0.32)	(0.31)	(0.57)	(0.47)	(0.49)	(0.54)	(0.37)	(0.43)

Table E3: Primary and lower-secondary school choices

	Indigenous	General	Telesecondary	Technical
Lag mathematics		0.0018	0.0004	0.002
		(0.0001)	(0.0001)	(0.0001)
Lag Spanish		0.0027	0.0016	0.0029
		(0.0001)	(0.0001)	(0.0001)
Prospera by score quartile and gender				
P*Q1 (4%)				
Female	-0.47	0.61	1.46	0.78
	(0.72)	(0.19)	(0.21)	(0.19)
Male	0.37	0.19	0.66	0.24
	(0.52)	(0.16)	(0.18)	(0.16)
P*Q2 (13%)				
Female	0.05	0.38	1.06	0.60
	(0.28)	(0.10)	(0.10)	(0.10)
Male	-0.10	0.29	0.98	0.54
	(0.28)	(0.09)	(0.09)	(0.09)
P*Q3 (30%)				
Female	0.19	0.28	1.12	0.37
	(0.16)	(0.07)	(0.06)	(0.07)
Male	0.08	0.35	1.05	0.45
	(0.17)	(0.07)	(0.06)	(0.07)
P*Q4 (53%)				
Female	0.41	0.16	1.11	0.35
	(0.13)	(0.06)	(0.05)	(0.06)
Male	0.50	0.41	1.23	0.63
	(0.13)	(0.06)	(0.05)	(0.06)
Education cat. (dad)				
Below primary school	0.13	-0.25	-0.08	-0.32
	(0.24)	(0.08)	(0.09)	(0.09)
Primary school completed	0.07	-0.10	-0.03	-0.14
	(0.24)	(0.08)	(0.09)	(0.09)
Secondary or below	-0.18	0.22	0.12	0.19
	(0.24)	(0.08)	(0.09)	(0.09)
College or above	-0.11	0.36	-0.03	0.32
	(0.26)	(0.09)	(0.10)	(0.09)
Working status (dad)				
Full time	0.15	0.08	0.00	0.03
	(0.19)	(0.07)	(0.07)	(0.07)

Table E3: Primary and secondary school choices

	Indigenous	General	Telesecondary	Technical
Not full time	0.03	0.27	0.13	0.23
	(0.19)	(0.06)	(0.07)	(0.07)
Father present				
At home	-0.28	0.03	-0.02	-0.02
	(0.12)	(0.05)	(0.05)	(0.05)
Not at home	-0.27	0.11	0.17	0.17
	(0.09)	(0.04)	(0.04)	(0.04)
Education cat. (mom)				
Primary school	0.01	-0.16	-0.16	-0.32
	(0.40)	(0.15)	(0.16)	(0.16)
Primary school completed	-0.12	0.11	0.00	-0.04
	(0.40)	(0.15)	(0.16)	(0.16)
Secondary or below	-0.37	0.36	0.13	0.18
	(0.40)	(0.15)	(0.16)	(0.16)
College or above	-0.31	0.58	0.02	0.36
	(0.42)	(0.16)	(0.17)	(0.17)
Working status (mom)				
Housework	-0.15	-0.28	-0.25	-0.13
	(0.28)	(0.12)	(0.12)	(0.12)
Part time	-0.02	-0.30	-0.32	-0.18
	(0.29)	(0.12)	(0.12)	(0.12)
Full time	0.08	-0.26	-0.24	-0.17
	(0.30)	(0.12)	(0.12)	(0.12)
Mother present				
At home	0.29	-0.13	-0.18	-0.16
	(0.20)	(0.08)	(0.08)	(0.09)
Not at home	0.06	0.03	0.03	0.07
	(0.14)	(0.05)	(0.05)	(0.05)
Number of people at home				
4 people	-0.11	0.14	0.09	0.18
	(0.10)	(0.04)	(0.04)	(0.04)
5 people	0.05	-0.03	0.01	-0.03
	(0.10)	(0.04)	(0.04)	(0.04)
$\geq$ 6 people	0.10	-0.21	-0.11	-0.18
	(0.08)	(0.03)	(0.03)	(0.03)
Age	-0.07	-0.49	-0.47	-0.42
	(0.11)	(0.05)	(0.05)	(0.05)

Table E3: Primary and secondary school choices

	Indigenous	General	Telesecondary	Technical
Age^2	-0.02	-0.10	-0.06	-0.14
	(0.04)	(0.01)	(0.01)	(0.02)
Male	-0.02	-0.05	0.07	-0.04
	(0.10)	(0.03)	(0.04)	(0.03)
First language spoken at home				
Indigenous	1.47	-0.56	-0.14	-0.37
	(0.13)	(0.08)	(0.06)	(0.07)
Both Spanish and indigenous	1.10	-0.23	-0.14	-0.27
	(0.13)	(0.08)	(0.07)	(0.08)
Internet access	0.06	-0.13	-0.12	-0.13
	(0.11)	(0.04)	(0.04)	(0.04)
Computer access	-0.15	0.31	0.06	0.34
	(0.10)	(0.03)	(0.04)	(0.04)
Number of pre-school years				
1 year	-0.12	-0.27	-0.04	-0.31
	(0.21)	(0.08)	(0.08)	(0.08)
2 years	-0.14	0.12	0.16	0.07
	(0.20)	(0.07)	(0.08)	(0.08)
3 years	-0.13	0.37	0.34	0.40
	(0.20)	(0.07)	(0.07)	(0.08)
4 years	0.03	0.36	0.33	0.36
	(0.20)	(0.08)	(0.08)	(0.08)
Urban dummy	-0.92	0.72	0.25	1.79
	(0.08)	(0.16)	(0.15)	(0.15)
Regions				
North-center	-0.10	2.00	1.56	1.54
	(0.10)	(0.22)	(0.25)	(0.23)
Center	1.04	2.38	2.64	2.42
	(0.12)	(0.29)	(0.32)	(0.31)
South	0.34	0.02	0.09	-0.01
	(0.09)	(0.21)	(0.24)	(0.21)
Distance to general school		-0.64	0.31	0.29
		(0.02)	(0.02)	(0.02)
Distance to general school (squared)		0.04	-0.02	-0.03
		(0.00)	(0.00)	(0.00)
Distance to telesecondary school		0.14	-0.89	0.22
		(0.02)	(0.02)	(0.02)

Table E3: Primary and secondary school choices

	Indigenous	General	Telesecondary	Technical
Distance to telesecondary school (squared)		-0.01	0.05	-0.02
		(0.00)	(0.00)	(0.00)
Distance to technical school		0.34	0.13	-0.68
		(0.02)	(0.02)	(0.02)
Distance to technical school (squared)		-0.03	-0.01	0.04
		(0.00)	(0.00)	(0.00)
Imputed wages		0.08	0.09	0.25
		(0.02)	(0.01)	(0.01)
Imputed wages (squared)		0.00	0.00	0.00
		(0.00)	(0.00)	(0.00)
(Log) number of general schools	-0.50	0.34	0.20	0.06
	(0.04)	(0.05)	(0.06)	(0.05)
(Log) number of general schools (squared)		0.00	0.01	0.02
		(0.01)	(0.01)	(0.01)
(Log) number of indigenous schools	0.75			
	(0.05)			
(Log) number of telesecondary schools		-0.08	-0.12	0.51
		(0.05)	(0.07)	(0.05)
(Log) number of telesecondary schools (squared)		0.02	0.11	-0.02
		(0.01)	(0.01)	(0.01)
(Log) number of technical schools		-0.44	-0.37	-0.43
		(0.07)	(0.08)	(0.07)
(Log) number of technical schools (squared)		0.03	0.01	0.02
		(0.02)	(0.02)	(0.02)
North-center × urbanity dummy		-0.03	0.44	0.59
		(0.10)	(0.11)	(0.10)
Center × urbanity dummy		-0.60	0.49	0.35
		(0.13)	(0.12)	(0.13)
South × urbanity dummy		-0.24	0.80	0.48
		(0.11)	(0.11)	(0.10)
North-center × distance to general school		-0.04	-0.03	0.02
		(0.02)	(0.02)	(0.02)
Center × distance to general school		-0.04	-0.04	0.00
		(0.02)	(0.02)	(0.02)
South × distance to general school		0.03	-0.03	0.05
		(0.02)	(0.02)	(0.02)
North-center × (log) number of general schools		-0.15	-0.23	-0.43

Table E3: Primary and secondary school choices

	Indigenous	General	Telesecondary	Technical
		(0.05)	(0.06)	(0.05)
Center × (log) number of general schools		-0.29	-0.36	-0.49
		(0.06)	(0.07)	(0.06)
South × (log) number of general schools		-0.35	-0.19	-0.25
		(0.05)	(0.06)	(0.05)
North-center × distance to technical school		0.00	0.09	-0.05
		(0.02)	(0.01)	(0.02)
Center × distance to technical school		0.10	0.04	-0.07
		(0.02)	(0.02)	(0.02)
South × distance to technical school		-0.04	0.06	-0.07
		(0.02)	(0.02)	(0.02)
North-center × (log) number of technical schools		0.25	0.18	0.70
		(0.06)	(0.07)	(0.06)
Center × (log) number of technical schools		0.67	0.16	0.74
		(0.08)	(0.09)	(0.08)
South × (log) number of technical schools		0.32	-0.07	0.52
		(0.07)	(0.08)	(0.07)
North-center × distance to telesecondary school		-0.01	0.03	-0.08
		(0.01)	(0.02)	(0.01)
Center × distance to telesecondary school		-0.07	-0.02	-0.06
		(0.02)	(0.03)	(0.02)
South × distance to telesecondary school		0.03	-0.02	0.01
		(0.01)	(0.02)	(0.01)
North-center × (log) number of telesecondary schools		-0.23	-0.42	-0.65
		(0.04)	(0.06)	(0.04)
Center × (log) number of telesecondary schools		-0.28	-0.15	-0.46
		(0.07)	(0.08)	(0.07)
South × (log) number of telesecondary schools		0.00	-0.04	-0.43
		(0.04)	(0.06)	(0.04)
North-center × imputed wages		-0.11	-0.08	-0.05
		(0.01)	(0.02)	(0.01)
Center × imputed wages		-0.14	-0.16	-0.14
		(0.02)	(0.02)	(0.02)
South × imputed wages		0.03	-0.02	0.00
		(0.01)	(0.01)	(0.01)
Urbanity dummy × imputed wages		-0.04	-0.06	-0.16
		(0.01)	(0.01)	(0.01)

Table E3: Primary and secondary school choices

	Indigenous	General	Telesecondary	Technical
Type I	3.08	-1.29	-0.02	-1.10
	(0.17)	(0.06)	(0.05)	(0.05)
Type II	1.04	-0.27	0.25	-0.32
	(0.14)	(0.08)	(0.08)	(0.08)
Type III	-1.37	0.26	-0.52	0.06
	(0.19)	(0.10)	(0.09)	(0.10)
Intercept	-1.46	-0.94	-0.37	-2.82
	(0.55)	(0.28)	(0.29)	(0.28)

Table E4: Retention estimates (choice and value-added)

	Choice	Mathematics	Spanish	Choice	mathematics	Spanish
Lag mathematics	-0.0039	0.37	0.16	-0.0012	0.38	0.16
	(0.0001)	(0.03)	(0.02)	(0.0001)	(0.05)	(0.05)
Lag Spanish	0.0002	0.13	0.25	-0.0107	0.12	0.31
	(0.0001)	(0.03)	(0.03)	(0.0002)	(0.05)	(0.05)
Prospera by score quartile and gender						
P*Q1 (4%)						
Female	-0.05	30.59	6.13	-0.26	16.33	29.94
	(0.30)	(42.02)	(31.21)	(0.46)	(44.06)	(112.18)
Male	-0.22	-10.57	4.72	-0.01	-27.62	-11.72
	(0.21)	(21.29)	(17.86)	(0.27)	(51.30)	(40.62)
P*Q2 (13%)						
Female	-0.04	14.07	-4.78	-0.52	-16.33	-57.39
	(0.16)	(18.28)	(14.90)	(0.36)	(70.21)	(52.04)
Male	-0.11	-1.65	-2.85	-0.11	10.90	-22.15
	(0.12)	(10.13)	(9.73)	(0.18)	(20.68)	(20.88)
P*Q3 (30%)						
Female	-0.14	-7.17	-10.50	-0.08	20.50	-2.89
	(0.11)	(11.36)	(9.36)	(0.22)	(27.76)	(24.39)
Male	-0.10	-3.06	-2.59	-0.44	5.83	-0.96
	(0.08)	(7.58)	(6.40)	(0.14)	(18.50)	(17.22)
P*Q4 (53%)						
Female	-0.14	-1.55	-2.83	-0.65	58.52	6.46
	(0.08)	(8.14)	(7.05)	(0.22)	(24.08)	(25.54)
Male	-0.08	-2.05	-1.11	-0.56	0.03	-1.02
	(0.07)	(6.37)	(5.64)	(0.14)	(17.55)	(16.56)
Education cat. (dad)						
Below primary school	-0.48	-1.10	3.47	-0.53	-8.72	-14.60
	(0.13)	(13.39)	(10.83)	(0.20)	(28.02)	(21.87)
Primary school completed	-0.32	2.47	5.30	-0.54	-21.56	-23.40
	(0.13)	(13.62)	(11.09)	(0.20)	(28.55)	(22.25)
Secondary or below	-0.20	5.79	10.44	-0.25	-2.84	-16.73
	(0.13)	(13.45)	(10.91)	(0.19)	(27.58)	(20.82)
College or above	-0.39	5.47	11.18	-0.26	1.23	-5.36
	(0.14)	(14.83)	(11.91)	(0.21)	(28.97)	(22.29)
Working status (dad)						
Full time	-0.08	-6.15	-13.76	-0.02	15.23	11.30
	(0.11)	(10.65)	(9.27)	(0.17)	(22.84)	(19.44)

Table E4: Retention estimates (choice and value-added)

	Choice	Mathematics	Spanish	Choice	mathematics	Spanish
Not full time	0.02	-5.44	-10.29	-0.08	19.50	4.97
	(0.10)	(10.32)	(8.92)	(0.17)	(21.47)	(18.03)
Father present						
At home	-0.03	8.39	6.93	0.18	31.91	22.00
	(0.07)	(7.17)	(6.13)	(0.12)	(14.83)	(14.01)
Not at home	0.05	7.02	3.10	0.09	13.36	5.67
	(0.06)	(5.59)	(4.82)	(0.10)	(12.47)	(11.93)
Education cat. (mom)						
Primary school	-0.52	-8.30	-3.47	-0.46	-19.49	-14.06
	(0.21)	(24.39)	(19.41)	(0.38)	(50.49)	(40.96)
Primary school completed	-0.34	-7.76	-0.76	-0.33	-28.04	-9.81
	(0.21)	(24.49)	(19.52)	(0.38)	(50.32)	(41.06)
Secondary or below	-0.33	-2.08	-3.87	0.00	-19.77	-7.99
	(0.21)	(24.48)	(19.43)	(0.38)	(49.94)	(40.29)
College or above	-0.15	-0.58	5.47	0.12	-5.39	0.56
	(0.23)	(25.22)	(20.29)	(0.39)	(50.99)	(41.43)
Working status (mom)						
Housework	0.17	-4.94	1.37	0.17	17.71	12.86
	(0.16)	(15.33)	(14.98)	(0.31)	(48.23)	(45.92)
Part time	0.17	-4.65	5.40	0.27	13.49	6.46
	(0.16)	(15.75)	(15.31)	(0.31)	(48.44)	(45.91)
Full time	0.09	-0.72	5.91	0.17	18.52	4.48
	(0.16)	(16.10)	(15.62)	(0.32)	(49.16)	(46.64)
Mother present						
At home	-0.04	-9.38	-4.22	-0.21	-22.13	-3.46
	(0.13)	(11.88)	(11.27)	(0.23)	(30.01)	(26.36)
Not at home	0.09	-4.81	-1.60	-0.14	-14.08	-4.73
	(0.08)	(7.67)	(6.82)	(0.13)	(17.70)	(17.88)
Number of people at home						
4 people	-0.05	6.32	4.38	-0.03	15.94	3.58
	(0.06)	(5.41)	(4.66)	(0.08)	(10.89)	(9.99)
5 people	-0.14	12.77	5.63	-0.19	0.38	-4.91
	(0.06)	(5.72)	(5.04)	(0.10)	(12.35)	(11.80)
$\geq$ 6 people	-0.14	13.13	3.57	-0.16	3.78	5.61
	(0.05)	(4.68)	(4.07)	(0.08)	(10.27)	(9.50)
Age	4.38	8.02	7.58	6.09	16.11	-4.59
	(0.05)	(9.89)	(8.73)	(0.08)	(26.18)	(23.14)

Table E4: Retention estimates (choice and value-added)

	Choice	Mathematics	Spanish	Choice	mathematics	Spanish
Age^2	-0.66	-1.64	-1.69	-1.15	-4.76	0.51
	(0.01)	(1.99)	(1.77)	(0.02)	(6.08)	(5.26)
Male	0.40	3.19	-14.40	0.78	26.51	-6.36
	(0.06)	(5.75)	(4.74)	(0.09)	(11.81)	(10.44)
First language spoken at home						
Indigenous	-0.02	-22.02	-10.40	-0.63	13.64	33.88
	(0.08)	(8.02)	(7.09)	(0.22)	(24.83)	(26.93)
Both Spanish and indigenous	-0.15	-20.58	-11.51	-0.13	-12.44	-9.75
	(0.11)	(9.92)	(8.64)	(0.20)	(30.25)	(35.29)
Internet access	0.17	-11.46	-10.30	-0.01	-16.94	-10.39
	(0.06)	(5.38)	(4.78)	(0.09)	(12.22)	(10.72)
Computer access	0.01	-3.72	2.05	0.09	6.71	4.89
	(0.05)	(5.01)	(4.55)	(0.08)	(11.38)	(10.00)
Number of pre-school years						
1 year	0.09	-9.80	-10.18	-0.43	-29.00	-37.54
	(0.12)	(13.26)	(10.62)	(0.23)	(30.48)	(27.25)
2 years	0.37	-8.63	-13.40	0.05	-27.07	-29.41
	(0.12)	(12.41)	(9.87)	(0.20)	(28.06)	(22.95)
3 years	0.52	-3.13	-5.04	0.24	-32.14	-34.48
	(0.12)	(12.26)	(9.79)	(0.20)	(27.39)	(22.04)
4 years	0.33	-0.98	-6.12	-0.09	-36.98	-31.53
	(0.12)	(12.30)	(9.78)	(0.20)	(27.71)	(22.76)
Urban dummy	-0.03	14.90	10.78	0.54	-6.78	-4.23
	(0.05)	(4.51)	(3.94)	(0.11)	(13.87)	(13.59)
Regions						
North-center	-0.02	-7.22	-3.73	-0.01	-0.93	-6.82
	(0.06)	(5.28)	(4.56)	(0.09)	(11.65)	(10.96)
Center	-0.09	1.26	7.05	0.01	8.13	-2.13
	(0.08)	(8.31)	(7.06)	(0.12)	(14.44)	(13.61)
South	0.09	10.10	1.65	0.00	4.27	2.91
	(0.05)	(4.96)	(4.47)	(0.09)	(12.27)	(11.09)
Indigenous school	0.09	-11.24	-13.62			
	(0.09)	(8.83)	(7.86)			
Grade 5	-0.57	-2.82	-1.34			
	(0.04)	(3.87)	(3.42)			
Grade 6	-2.70	11.59	12.87			
	(0.09)	(9.56)	(8.31)			

Table E4: Retention estimates (choice and value-added)

	Choice	Mathematics	Spanish	Choice	mathematics	Spanish
Telesecondary school				0.09	44.43	23.75
				(0.09)	(13.72)	(12.70)
Technical school				-0.57	-1.65	-3.02
				(0.04)	(9.70)	(8.68)
Grade 8				-2.70	9.76	-0.73
				(0.09)	(8.74)	(8.55)
Unobserved types						
Type I	-0.54	1.67	-2.05	-0.54	-8.02	7.69
	(0.09)	(9.48)	(8.19)	(0.09)	(18.99)	(18.28)
Type II	-0.36	2.42	3.32	-0.36	11.82	7.99
	(0.13)	(12.65)	(11.30)	(0.13)	(27.98)	(27.09)
Type III	0.00	5.85	12.99	0.00	-6.53	4.27
	(0.13)	(11.78)	(10.60)	(0.13)	(23.27)	(24.02)
Intercept term	-6.91	246.71	290.06	-6.91	234.37	289.08
	(0.28)	(32.70)	(27.06)	(0.28)	(75.93)	(64.81)
Standard error (σ)		91	79		99	93
		(1.43)	(1.28)		(3.05)	(2.80)

Table E5: Dropout during lower-secondary school

Dropout period	Grade 7	Grade 8
Lag mathematics	-0.0015	-0.0007
	(0.0001)	(0.0001)
Lag Spanish	-0.0029	-0.0032
	(0.0001)	(0.0001)
Prospera by score quartile and gender		
P*Q1 (4%)		
Female	-0.16	-0.14
	(0.15)	(0.13)
Male	-0.47	-0.09
	(0.16)	(0.11)
P*Q2 (13%)		
Female	-0.47	-0.34
	(0.10)	(0.07)
Male	-0.38	-0.20
	(0.08)	(0.07)
P*Q3 (30%)		
Female	-0.32	-0.14
	(0.06)	(0.05)
Male	-0.42	-0.32
	(0.06)	(0.05)
P*Q4 (53%)		
Female	-0.23	-0.18
	(0.05)	(0.05)
Male	-0.58	-0.37
	(0.05)	(0.04)
Education cat. (dad)		
Below primary school	0.29	-0.02
	(0.09)	(0.07)
Primary school completed	0.21	-0.11
	(0.09)	(0.07)
Secondary or below	0.03	-0.09
	(0.09)	(0.07)
College or above	-0.15	-0.22
	(0.09)	(0.07)
Working status (dad)		
Full time	-0.07	-0.03
	(0.07)	(0.06)

Table E5: Dropout during secondary school

Dropout period	Grade 7	Grade 8
Not full time	-0.09	-0.05
	(0.06)	(0.06)
Father present		
At home	0.07	0.02
	(0.05)	(0.04)
Not at home	-0.14	-0.17
	(0.04)	(0.03)
Education cat. (mom)		
Primary school	0.20	-0.09
	(0.16)	(0.14)
Primary school completed	0.12	-0.09
	(0.16)	(0.14)
Secondary or below	0.00	-0.10
	(0.16)	(0.14)
College or above	-0.22	-0.23
	(0.17)	(0.14)
Working status (mom)		
Housework	-0.02	-0.13
	(0.12)	(0.10)
Part time	0.20	0.07
	(0.12)	(0.10)
Full time	0.09	-0.02
	(0.12)	(0.10)
Mother present		
At home	0.14	0.04
	(0.08)	(0.07)
Not at home	-0.01	0.03
	(0.05)	(0.04)
Number of people at home		
4 people	0.01	-0.06
	(0.04)	(0.03)
5 people	0.02	0.00
	(0.04)	(0.03)
$\geq$ 6 people	0.15	0.00
	(0.03)	(0.03)
Age	0.55	0.84
	(0.05)	(0.04)

Table E5: Dropout during secondary school

Dropout period	Grade 7	Grade 8
Age^2	0.06	0.08
	(0.02)	(0.01)
Male	0.18	0.19
	(0.03)	(0.03)
First language spoken at home		
Indigenous	-0.02	0.06
	(0.07)	(0.06)
Both Spanish and indigenous	-0.20	0.01
	(0.09)	(0.07)
Internet access	0.10	0.18
	(0.04)	(0.03)
Computer access	-0.12	-0.11
	(0.04)	(0.03)
Number of pre-school years		
1 year	0.03	-0.06
	(0.08)	(0.07)
2 years	-0.14	-0.03
	(0.07)	(0.07)
3 years	-0.27	-0.08
	(0.07)	(0.06)
4 years	-0.09	-0.06
	(0.07)	(0.07)
Urban dummy	-0.20	-0.16
	(0.17)	(0.15)
Regions		
North-center	0.09	-0.30
	(0.12)	(0.09)
Center	0.09	-0.38
	(0.14)	(0.12)
South	0.58	-0.12
	(0.11)	(0.10)
Telesecondary school dummy	-0.15	0.03
	(0.10)	(0.08)
Technical school dummy	0.27	0.17
	(0.07)	(0.06)
Distance to the closest school	0.11	0.02
	(0.03)	(0.03)

Table E5: Dropout during secondary school

Dropout period	Grade 7	Grade 8
Distance to the closest school (squared)	-0.01	0.00
	(0.00)	(0.00)
Imputed wages	-0.14	0.08
	(0.01)	(0.01)
Imputed wages (squared)	0.005	-0.003
	(0.0003)	(0.0003)
North-center × urbanity dummy	0.02	0.33
	(0.11)	(0.09)
Center × urbanity dummy	0.12	0.48
	(0.13)	(0.11)
South × urbanity dummy	-0.14	0.22
	(0.10)	(0.08)
North-center × distance to the closest school	-0.01	0.00
	(0.03)	(0.02)
Center × distance to the closest school	-0.02	0.00
	(0.04)	(0.03)
South × distance to the closest school	-0.03	0.03
	(0.03)	(0.02)
North-center × telesecondary school dummy	0.18	0.02
	(0.11)	(0.09)
North-center × technical school dummy	-0.22	-0.10
	(0.08)	(0.07)
Center × telesecondary school dummy	0.06	0.04
	(0.14)	(0.11)
Center × technical school dummy	-0.34	-0.21
	(0.11)	(0.09)
South × telesecondary school dummy	-0.23	-0.02
	(0.12)	(0.09)
South × technical school dummy	-0.29	-0.06
	(0.08)	(0.07)
Urban dummy × imputed wage	0.03	0.01
	(0.01)	(0.01)
Unobserved types		
Type I	-0.15	-0.22
	(0.06)	(0.05)
Type II	0.12	-0.17
	(0.07)	(0.07)

Table E5: Dropout during secondary school

Dropout period	Grade 7	Grade 8
Type III	-0.22	0.25
	(0.09)	(0.06)
Intercept term	-0.82	-1.50
	(0.27)	(0.23)

Table E6: Coefficients associated with cheating equation

	Mathematics		Spanish	
	Value	S.D.	Value	S.D.
For non-retained students				
Grade 5				
General	43	1.29	27	1.28
Indigenous	69	6.12	50	5.25
Grade 6				
General	46	1.42	29	1.14
Indigenous	54	6.21	37	4.72
Grade 7				
General	59	3.16	35	2.92
Telesecondary	90	4.61	55	3.70
Technical	74	3.60	48	3.53
Grade 8				
General	80	1.90	46	1.79
Telesecondary	101	2.71	66	2.11
Technical	79	2.09	42	2.06
Grade 9				
General	74	2.91	26	2.53
Telesecondary	49	3.49	20	2.82
Technical	61	2.75	26	2.41
For retained students				
Primary school	65	15	48	12
Secondary school	107	37	77	28