The Role of Private-Voucher Schools in the Education-to-Work Transition*

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Abstract

We study how private-voucher high schools shape the transition from school to work in Chile. They raise their students' wages and their chances of holding a formal job, notwithstanding their inability to shift higher education attainment. Using administrative data that follow a cohort of public-primary eighth graders into higher education and the labor market, we combine value-added regressions with a sequential model of schooling choices and outcomes. We find that what higher education pays depends on the type of program as much as on completing it. A professional degree raises wages by 0.60 log points relative to a high-school diploma; a technical degree raises wages by 0.37 log points. On the other hand, an incomplete professional spell lowers wages by 0.18 log points and formal employment by 9 percentage points (p.p.), while an incomplete technical spell lowers neither. Because fewer than a quarter of those who start a professional program finish it, the expected return to enrolling in one is statistically indistinguishable from zero, against 0.14 log points for a technical program, and it carries substantially more risk. Attending a private-voucher high school raises higher-education enrollment by 1.8 percentage points, and the increase is entirely professional, drawn out of non-enrollment. However, all of that increase in enrollment materializes in professional dropout, that rises by 1.8 p.p. Yet wages rise by 0.06 log points, and three-quarters of students face a positive expected gain. Essentially all of the positive labor market effects of vouchers come from higher pay within schooling states.

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1 Introduction

For decades the promise of higher education has been treated as nearly automatic: enroll, and a labor-market payoff follows. Recent studies have shown that this promise may no longer be a panacea (Stange, 2012; Rodríguez et al., 2016; Heckman et al., 2018; Mountjoy, 2022; Zühlke et al., 2022). Enrolling does not guarantee a return, because a large share of those who enroll never finish, and an incomplete spell can carry a low or even negative labor-market return. As a consequence, a policy that succeeds in pushing more students into higher education need not improve their outcomes. Part (or all) of the additional enrollment may end in dropout, and the programs students are channeled into may be the ones they are least likely to complete. Evaluating such a policy only by the enrollment it generates therefore risks misreading both whether it works and why. A more complete evaluation should include degree completion and the associated labor market returns.

In this paper, we study how private-voucher high schools shape the transition from school to work. Despite the voucher literature having provided plentiful evidence on the effects of voucher schools on test scores (Rouse, 1998; Angrist et al., 2002; Mayer et al., 2002; Peterson et al., 2003; Wolf et al., 2013; Lara et al., 2011; Muralidharan and Sundararaman, 2015; Epple et al., 2017; Mills and Wolf, 2017; Abdulkadiroglu et al., 2018), far less is known about what vouchers do further along the educational sequence, where the decisions that determine labor-market outcomes are actually made.[1] We follow a cohort of Chilean students from the end of primary school into the labor market and ask, in order, whether the high school they attend changes whether they continue past secondary education, which kind of higher-education program they enter, whether they complete it, and what they earn. Posing the question as a sequence uncovers the mechanisms behind long-run outcomes, because the answers at successive stages do not point in the same direction.

Chile is an interesting scenario to study vouchers. With its more than forty years of experience with a nationwide voucher program, it constitutes one of world's most important large-scale experiments of educational choice (Epple et al., 2017).[2] Since its inception in 1981, the basic aspects of the system have remained fairly unchanged, with students exercising unrestricted

[1]There is a small, but growing, literature that has analyzed the effects of vouchers on longer-term educational outcomes, mainly high school graduation and college enrollment (Angrist et al., 2006; Bettinger et al., 2010; Wolf et al., 2013; Chingos and Peterson, 2015). Fewer studies follow voucher recipients into the labor market. Bettinger et al. (2019) track applicants to Colombia's PACES lottery for more than twenty years and find that the program's gains—7 p.p. in tertiary enrollment and 17 percent in formal-sector earnings at about age 33—come entirely from students who had applied to vocational rather than academic secondary schools.

[2]Gauri (1998) describes Chile's voucher system as "...perhaps the world's most ambitious attempt to design and implement a program of educational choice...".

choice among public and subsidized private schools.[3] Each student is entitled to a voucher that is directly paid to the school of her choice, and that covers all or part of the school's tuition.[4] Schools, on the other hand, use the vouchers they receive to fund their operations.

Our empirical analysis describes a specific and policy-relevant population. We follow the 94,453 students who completed eighth grade in 2000 in a public primary school offering no secondary grades and who were therefore required to choose a new high school—about two-fifths of the national eighth-grade cohort and two-thirds of its public-primary students. These students score about 0.14 standard deviations below the national eighth-grade mean and are about 1.3 times as likely as the average Chilean student to have a mother who did not complete secondary school (58% versus 45%). They are, in other words, the students a voucher system is meant to serve, and the forced choice they face at the end of primary school is what makes the secondary-school decision observable and well defined for all of them. Throughout, our estimates describe this population, not the Chilean student body as a whole.

We combine two complementary empirical approaches. We begin with a set of reduced-form value-added regressions, building on the value-added literature (Rockoff, 2004; Rivkin et al., 2005; Kane and Staiger, 2008; Chetty et al., 2014a,b; Bau, 2022), that project each labor-market outcome on the student's final schooling level, or on the type of high school attended, controlling for the four standardized tests the student sat in eighth grade. The identifying assumption is the usual in the value-added literature: conditioning on prior achievement absorbs the ability that drives both schooling choices and later outcomes. As a consequence, these regressions recover average treatment effects cleanly and with little structure. What they cannot deliver is who gains and who loses, or what would happen under counterfactual assignments, both of which are central questions for policy. We therefore develop and estimate a sequential model of schooling decisions and outcomes that incorporates the decision stages students face as they move through the system, beginning with the choice of a private-voucher or a public high school. The model is a generalization of the Roy model (Roy, 1951; Heckman and Honoré, 1990), in which individuals compare the net benefits of the alternatives at hand. We allow observed and unobserved characteristics to influence both decisions and outcomes, and our identification strategy, which recovers the distribution of unobserved heterogeneity from a system of early test scores, supports

[3] In 2008, the Chilean government introduced an important reform to its voucher system, that, among other things, created a differentiated voucher for low-income students. However, since our data cover a period that precedes this reform, we do not go into the details of the implied changes in the system's structure. See Correa et al. (2014), Navarro-Palau (2017), Feigenberg et al. (2019), Gazmuri (2024), Neilson (2025), and Sánchez (2026) for studies that analyze this reform.

[4] Public schools are mandated to be tuition-free. Subsidized private schools are allowed to charge a top-up fee above the voucher, and most secondary ones do: in 2004, the year our cohort completed secondary school and the first year for which the fee register is available, 77 percent of private-voucher schools offering regular daytime secondary education charged one.

interpreting that heterogeneity as a combination of inherent scholastic abilities (Carneiro et al., 2003; Hansen et al., 2004; Heckman et al., 2006, 2018). The model also delivers the distribution of the treatment effects of attending a private-voucher school, and the potential outcomes each student would realize on paths she does not take. Reassuringly, the value-added and structural estimates of the wage returns to each schooling level, and of the vouchers' effects on wages, are closely aligned on both direction and magnitude.

Three findings organize the paper. First, what higher education pays depends on the type of program a student enters as much as on whether she completes it. Relative to a high-school diploma, a professional degree raises wages by 0.60 log points and a technical degree by 0.37, but dropping out of a professional program lowers wages by 0.18 log points and formal employment by 8.9 percentage points (p.p.), whereas dropping out of a technical program leaves wages statistically unchanged and formal employment 2.5 p.p. higher. Interestingly, while completion governs the wage payoff, both degrees leave formal employment about 3 p.p. lower than a high-school diploma. Because professional programs are completed by fewer than a quarter of the students who start them, the expected return to enrolling in one is statistically indistinguishable from zero, against 0.14 log points for a technical program. The professional option also lowers the probability of formal employment by 7.4 p.p., and its outcome distribution is markedly more dispersed than that of enrolling in a technical program. As a result, the professional track is dominated in both expected return and risk, and it remains dominated at every level of ability.

Second, the vouchers send students precisely there. Attending a private-voucher high school raises higher-education enrollment by 1.83 p.p. out of a base of 46.9%, but the entire increase is professional—professional enrollment rises by 1.96 p.p. while technical enrollment does not move—and it does not materializes in a degree. Professional dropout rises by 1.79 p.p. while professional graduation moves by an insignificant 0.17 p.p. The additional enrollment therefore leaves degree attainment no higher.

Third, we estimate private-voucher schools' effects on labor market outcomes and decompose those effects into the schooling flows they induce and the returns those destinations pay. Vouchers raise earnings by 0.057 log points. Importantly, all of the effects of vouchers run through higher wages within each schooling state, and virtually none via transitions across these states. The within-state component accounts for 0.060 log points, while the reallocation across schooling states contributes -0.004. Heterogeneity analyses reveal that the gain from vouchers is widely shared among students. Three-quarters of students face a positive expected wage effect, and a majority do in every ability tercile.

This paper contributes to the literature in three ways. First, it adds to the studies that estimate the effects of school-choice programs on schooling outcomes beyond test scores. Past studies have considered high-school graduation (Angrist et al., 2006; Bettinger et al., 2010; Wolf et al., 2013), college enrollment (Chingos and Peterson, 2015; Chingos, 2018), degree attainment (Deming et al., 2014; Wolf et al., 2019; Yoo et al., 2023; Cohodes and Pineda, 2024), and the two-year/four-year college margin (Angrist et al., 2016; Dobbie and Fryer, 2020; Cohodes and Pineda, 2024). Absent in these studies is the incomplete educational spell as a terminal state. On that line, Wolf et al. (2019) show that Milwaukee voucher users enter four-year colleges at a higher rate than students with no voucher, but degrees do not move in the older cohort.[5] We extend these studies by observing every schooling step for the same voucher-treated students and treating the incomplete spell as a terminal state. Angrist et al. (2016) anticipate the form in Boston, where charters move applicants from two-year to four-year colleges while leaving overall enrollment statistically unchanged. In our case, enrollment itself rises, from non-enrollment into the track we find dominated, and arrives as professional dropout.

Second, we add to the small literature on the effects of voucher and charter schools on earnings (Bettinger et al., 2019; Dobbie and Fryer, 2020; Yoo et al., 2023), and on how schooling attainment and earnings effects relate (Chetty et al., 2014b; Deming et al., 2016). The two effects often diverge. Chile's original voucher reform raises every attainment margin yet leaves mean earnings virtually unchanged (Bravo et al., 2010). Similarly, charters raise four-year enrollment, but not earnings (Dobbie and Fryer, 2020). We contribute to these studies by identifying the mechanisms that translate schooling attainment into earnings, by splitting a sector's earnings effect between the postsecondary states it moves students across, each at its own return, and the change in pay within them. Our evidence points at induced schooling carrying negative value, while the earnings effect is positive. To our knowledge, such a finding is novel in the literature.

Third, we add to the literature on the returns to starting higher education when completion is uncertain. Studies in this literature estimate the effects of starting college integrating over completion (Altonji, 1993), the option to stop (Stange, 2012), each node's continuation value (Heckman et al., 2016, 2018), and vocational against university entry net of failure risk (Zühlke et al., 2022), with state-specific returns net of ability (Carneiro et al., 2003; Heckman et al., 2006; Urzua, 2008; Prada and Urzúa, 2017) and "some college" as a terminal state (Heckman et al., 2006; Urzua, 2008). Among US studies, most of them find that the incomplete state pays above a high-school diploma, so the continuation value only adds to a positive base. An exception is Altonji (1993), who shows that leaving with under two years of college is likely and pays -0.61 percent for men, whose ex-ante return to starting is 2.8 percent.[6] We extend this literature by

[5] In a similar vein, Bettinger et al. (2019) follow PACES applicants to about age 33, and find that the 7 p.p. tertiary-enrollment gain among vocational-school applicants comes with 2.4 p.p. more tertiary graduates. In the US, Massachusetts charter schools raise four-year enrollment by 8.2 to 9.7 p.p. and bachelor's completion by 4.1 to 11.4 p.p. (Cohodes and Pineda, 2024).

[6] In Heckman et al. (2016), a quarter of the return to enrolling in college comes from the continuation value, the option it opens to graduate. Rodríguez et al. (2016) estimate the returns to Chile's two-, four- and five-year degrees,

computing the returns to entering each track, accounting by student's own completion, against a common high-school baseline and net of unobserved ability, and find a ranking. The professional track is dominated in wages, employment and risk at every ability level, its negative direct effect leaving the continuation value as its entire case. Our findings also relocate the heterogeneity. We show that the degree return is flat in ability while the dropout penalty steepens.

This paper is organized as follows. Section 2 describes Chile's voucher system and the facts that motivate our analysis. Section 3 presents the data and the construction of our sample. Section 4 reports the value-added evidence on what each schooling level pays and on the effect of attending a voucher high school. Section 5 presents our model of schooling decisions and outcomes, and discusses its identification and estimation. Section 6 presents the empirical implementation. Section 7 presents our results. Finally, Section 8 concludes.

2 Background and Motivating Facts

Chile has run a nationwide school voucher system since 1981. Families choose schools freely, unconstrained by residence and subject only to the distance they are willing to travel and to any top-up fees a school charges, and funding flows from the government to schools through a per-student voucher. Schools fall into three groups by ownership, management, and subsidy status. *Public* (municipal) schools are owned and managed by local municipalities, are funded by the voucher, and are tuition-free. *Private-voucher* schools are privately owned and managed, receive the subsidy, and may charge top-up fees; about 40% of them charged positive fees in the early 2000s, averaging about US$240 per year. *Private-non-voucher* schools are privately owned and managed, receive no subsidy, and are financed entirely through fees that average more than ten times those of private-voucher schools (Sánchez, 2026). In the early 2000s, 46% of students attended a public school, 45% a private-voucher school, and 9% a private-non-voucher school, so that more than nine in ten students used a voucher. The voucher itself was worth about US$560 per student per year.[7]

We follow standard practice and set aside the private-non-voucher sector, which does not take up vouchers, applies selective admissions, and lies outside the public-versus-subsidized school-choice framework (Contreras et al., 2010; Lara et al., 2011; Correa et al., 2014). Our treatment contrast is therefore the binary choice between a private-voucher and a public high school. We study that choice among students who completed eighth grade in a public primary school offering

but exclude dropout by construction. Prada and Urzúa (2017) estimate the returns to four-year attendance, finding that 19 percent of high-school graduates would earn more by not attending.

[7] For more detailed descriptions of Chile's universal voucher system, see Gauri (1998), McEwan and Carnoy (2000), and Gazmuri (2024).

no secondary grades. These students cannot continue where they are: every one of them must choose a new school at the end of primary education, which makes the secondary-school decision observable and well defined for the entire sample, rather than the inertial decision faced by students whose primary school continues into secondary.

Higher education in Chile is frequently begun and often not finished. Among the students in the broad population who ever enroll, roughly 125,900, about 66% drop out, so that only one in three leaves with a degree. Dropout is higher on the professional track—the academic, university-oriented programs—where 67.5% drop out, than on the technical track of shorter vocational programs, where 64.3% do. The difference is small, but it falls on the track where failure is most costly: as Section 4 shows, the wage return to an incomplete professional spell is negative, the worst labor-market outcome in our data and worse than never having enrolled. Enrollment is therefore a poor summary of what higher education delivers; program type and completion are what matter.

Voucher students attain more than their public-school peers on every raw margin. They enroll in higher education at a rate of 66% against 58%, and the gap appears on both tracks but is wider on the professional one, 44% against 39%, with 22% against 19% on the technical track. They also leave with more degrees, 22% against 20%. This picture is consistent with a generally encouraging body of Chilean evidence, which has tended to find positive effects of the voucher sector on standardized test scores (Contreras et al., 2010; Lara et al., 2011; Correa et al., 2014), in line with the broader literature surveyed by Epple et al. (2017). The composition of the advantage is what gives us pause. The additional enrollment lands disproportionately on the professional track, the one with the highest dropout rate, and conditional on enrolling, voucher students graduate at no higher a rate than public students, 33% against 34%. These are raw differences that mix what the voucher sector does to students with the selection of who attends it, and separating the two is the task of the model in Section 5.

The students for whom this matters most are concentrated in the sector we study. Vulnerable students, those whose mother has fewer than twelve years of schooling, make up 45% of the broad student population, and they drop out more than average once enrolled, 68% against 66%, with the gap widening on the professional track to 70% against 66%. They are also heavily concentrated among public-primary students: the forced-choice population we follow is 57% vulnerable, against 45% in the population at large. It is that population, and the professional-versus-technical and completion margins along which its outcomes are decided, that the rest of the paper is about.

3 Data

Our sample consists in the universe of 8th grade students enrolled in public primary schools that do not offer secondary grades, in the year 2000. The majority of these students are 14 years old by then. We follow these students through high school. Once graduated from high school, we observe whether they enroll in college. Lastly, for students enrolled in college, we observe whether they complete their degree on or before their 30th birthday (year 2016). Finally, we observe their labor-market outcomes—formal employment and labor earnings—in 2013, when the typical student in our sample is about 27 years old, roughly nine years after graduating from high school. Students may enroll in, and graduate from, more than one higher-education program over time; for each student we record the most recently observed program (through 2011) and use it to define college enrollment, the type of higher education (professional, i.e. academic and university-track, versus technical, i.e. vocational and shorter-cycle), and graduation. The type is that of the most recent degree for students who graduate, and that of the most recent program of enrollment for students who enroll but do not graduate.[8]

To understand our selection criteria, it is worth noting some institutional aspects of Chile's education system. Primary education consists in grades 1st–8th, and secondary education in grades 9th–12th. Schools, public and private, offer only primary grades, only secondary grades, or both. Students enrolled in schools offering only primary grades are therefore forced to choose a new school to pursue their secondary education, and for them and their families the period right after completing 8th grade is one of intensive school shopping. This is in contrast with students enrolled in schools offering both levels, for whom the end of 8th grade is not too different from the end of 7th or 9th, and who mostly continue their secondary education in the same school. Since we are interested in students' high school decisions, we restrict our analysis to students enrolled in schools offering only primary education grades.

We then follow Lara et al. (2011) and drop students enrolled in private primary schools, both private-voucher and private-non-voucher. The private-voucher primary sector is by far the larger of the two exclusions, at 83,607 students, or 34.3% of the cohort, against 19,991 in private-non-voucher primary schools; private-voucher primary schools are also much more likely than public ones to continue into secondary grades, so their students do not face the forced choice that defines our design. What remains is arguably the most vulnerable part of the cohort, and the part for which the policy lessons we draw are most relevant. As discussed in Section 2, we also disregard students enrolling in private-non-voucher high schools (Contreras et al., 2010; Lara et al., 2011; Correa et al., 2014); only about 1% of students in public primary schools without secondary

[8]Higher-education enrollment is observed from 2005 and degrees from 2007, in both cases through 2011, before the 2013 labor-market outcomes; we observe labor-market outcomes only for 2013. A student is coded as enrolled in college if she appears in the enrollment registries or earns a degree, and as a graduate if she appears in the degree registries.

grades do so (1,017 of 96,856). Because we do not observe the high school attended for students who do not earn a high school diploma, we drop them as well; they represent about 13% of students. Finally, rather than dropping observations with missing covariates, we retain them by including a missing-data category for each covariate, dropping only the small number of students who lack the 8th-grade test scores that constitute our measurement system. Our final estimation sample consists of 94,453 students.

Our data come from the national standardized exams for primary and secondary education levels (SIMCE), the censuses of students and schools, the national standardized college admission exams (PSU), the census of students attending higher education institutions, the census of students earning a higher education degree, and the administrative records of Chile's unemployment insurance system, from which we recover formal employment and labor earnings. We use several years for each of these data sets, spanning the period 1998-2016. In addition, we use data from the 2003, 2006, and 2009 versions of the national household survey (CASEN) to construct the instruments we use as part of our identification strategy. These variables relate to the local availability of private-voucher schools, local labor market tightness and opportunities, and local availability of higher education institutions. See appendix A for a more detailed description of each of the data sets we use.

4 Reduced-Form Evidence

Before turning to our structural model, we present a set of transparent, design-light estimates that motivate it. Our reduced-form strategy is a value-added (VA) regression, in the tradition of the value-added literature (Rockoff, 2004; Rivkin et al., 2005; Kane and Staiger, 2008; Chetty et al., 2014a,b; Bau, 2022): we project each labor-market outcome on indicators for the student's final schooling level (or for the type of high school attended), controlling for prior achievement as measured by the four 8th-grade SIMCE standardized test scores, and clustering standard errors at the 8th-grade school. Conditioning on these baseline scores absorbs the prior ability that drives both schooling choices and later earnings, so that the resulting coefficients recover average treatment effects (ATEs)—the mean outcome gain of a schooling level over its baseline, averaged across all students. We estimate these effects for our cohort of vulnerable students: the public-primary 8th-graders of 2000 who, upon completing primary school, were forced to choose a secondary school. Throughout, the omitted baseline schooling category is a high-school diploma with no higher education. These estimates deliver the average effect cleanly and transparently. What they do not deliver is who gains and who loses, nor the counterfactuals that are the ultimate object of this paper—which is why the structural model follows.[9]

Concretely, we estimate two specifications. The first relates a labor-market outcome to the student's final schooling level,

$$Y_i \quad = \quad \omega_0 + \omega_1 PG_i + \omega_2 PD_i + \omega_3 TG_i + \omega_4 TD_i + X_i\beta + T_{i,-1}\delta + \varepsilon_i, \tag{1}$$

where Y_i is either formal employment or the log annual wage of student i in 2013; PG_i, PD_i, TG_i and TD_i are indicators for the four final schooling levels—professional graduate, professional dropout, technical graduate and technical dropout—with a high-school diploma and no higher education the omitted category; X_i is a vector of demographic covariates (gender, each parent's education, family structure, household income, and region, each entered as indicators together with a missing-value indicator); $T_{i,-1}$ is the vector of student i's four 8th-grade SIMCE scores in verbal, mathematics, social sciences and natural sciences; and ε_i is an idiosyncratic error term. The parameters of interest are ω_1 through ω_4, the return to each final schooling level relative to a high-school diploma. The second specification replaces the schooling levels with the high-school treatment itself,

$$Y_i \quad = \quad \gamma_0 + \gamma_1 V_i + X_i\beta + T_{i,-1}\delta + u_i, \tag{2}$$

where V_i takes the value of one if student i attended a private-voucher secondary school and zero if she attended a public one, so that γ_1 is the value-added effect of the voucher sector on the labor-market outcome.

We estimate equations (1) and (2) by ordinary least squares, and we cluster standard errors at the 8th-grade school in all regressions—the level at which the forced secondary-school choice is made. We also estimate equation (1) separately in the voucher and the public subsamples, which lets the returns to higher education themselves differ by the sector in which a student completed secondary school. Under the standard value-added assumption that, conditional on the covariates and the prior test scores, the remaining variation in schooling is unrelated to the outcome innovation, ω_1 through ω_4 and γ_1 identify average causal effects.

Returns to education. Table 1 reports the VA returns to each final schooling level, relative to a high-school diploma, on formal employment (column 1) and log wages (column 2). The wage returns to completion are sizable: graduating from a professional (academic, university-track) program raises log wages by 0.658 relative to a high-school graduate, while completing a technical (vocational, shorter-cycle) degree raises them by 0.377. The striking result is that

[9] We deliberately do not pursue an instrumental-variables ("choice-shifter") variant of these regressions. VA recovers the ATE, which is all the reduced form is meant to establish here; recovering the additional moments—the distribution of effects and the margin-specific treatment parameters—requires the structural model.

the returns to higher education are not uniformly positive: students who enroll in but drop out of a professional program earn 0.205 log points less than they would with only a high-school diploma, the lone negative-return outcome in the table, whereas technical dropouts are statistically indistinguishable from high-school graduates (0.019). The employment margin orders the same four states differently. Relative to a high-school graduate, professional dropout lowers the probability of formal employment by 8 percentage points (p.p.) and technical graduation by a modest 1.9 p.p., while professional graduation moves it not at all and technical dropout raises it by 3.5 p.p.—consistent with firms valuing the practical skills acquired in vocational programs even absent a completed degree. Taken together, the headline is that the payoff to higher education hinges on type and completion: an incomplete professional spell is the worst labor-market outcome in our cohort, worse than never having enrolled at all.[10]

Table 1: Value-Added Returns to Education, Relative to a High-School Diploma

	employment (1)	log wage (2)
professional graduate	0.000	0.658***
	(0.006)	(0.018)
technical graduate	-0.019***	0.377***
	(0.006)	(0.016)
professional dropout	-0.080***	-0.205***
	(0.005)	(0.015)
technical dropout	0.035***	0.019
	(0.004)	(0.012)
observations	94,453	72,698

Notes: OLS value-added estimates for the cohort of vulnerable students (public-primary 8th-graders of 2000). Each regression controls for the four 8th-grade SIMCE test scores; standard errors, in parentheses, are clustered at the 8th-grade school. The omitted category is a high-school diploma with no higher education. Column 1 reports effects on formal employment; column 2, on log wages, estimated on the formally employed subsample. *, **, ***: significant at 10%, 5%, 1%.

The effect of attending a voucher high school. We now turn from the type of higher education to the high-school stage itself, and estimate equation (2). Relative to a public-school student with the same baseline achievement, attending a private-voucher high school raises the probability of formal employment by 1.5 percentage points (standard error 0.3) and log wages by 0.065 log points (0.010), or about 7 percent; the employment regression uses all 94,453 students in the sample and the wage regression the 72,698 of them who are formally employed. Both

[10]Labor income is observed only for the formally employed, through the unemployment-insurance (AFC) administrative records; the wage regressions are therefore estimated on the employed subsample (N = 72,698), whereas the employment regressions use the full cohort (N = 94,453). We accordingly report both the extensive (employment) and intensive (wage) margins throughout.

effects are precisely estimated and positive, so the voucher sector confers a direct labor-market advantage on top of whatever it does through the schooling decisions it shapes downstream.

Returns to education by high-school type. Table 2 asks whether the returns to higher education themselves differ by the type of high school a student attended, estimating the returns separately for voucher and public graduates. The branch-specific returns are remarkably similar across sectors, but two consistent gaps emerge. Conditional on completing a professional degree, voucher students earn a slightly lower return over a same-sector high-school graduate than public students do (0.641 versus 0.665), while conditional on completing a technical degree they earn a slightly higher one (0.387 versus 0.371); the penalty to professional dropout is also somewhat smaller for voucher students (-0.191 versus -0.221). A sharper version of the same pattern appears among technical dropouts: relative to a same-sector high-school graduate, an incomplete technical spell carries a positive and significant wage return for voucher students (0.040) but is essentially zero for public students (0.003). The differences are small, but they point in a coherent direction: the voucher sector appears to add the most value precisely where its students are most likely to end up—the technical track—and to cushion the downside of an incomplete professional spell. This is exactly the kind of interaction between the high-school stage and the type and completion of higher education that an average-effect regression can flag but not fully resolve.

Table 2: Value-Added Returns to Education by High-School Type, Relative to a Same-Sector High-School Diploma

	voucher HS		public HS	
	employment	log wage	employment	log wage
	(1)	(2)	(3)	(4)
professional graduate	0.006	0.641***	-0.003	0.665***
	(0.010)	(0.029)	(0.008)	(0.024)
technical graduate	-0.009	0.387***	-0.024***	0.371***
	(0.010)	(0.028)	(0.007)	(0.019)
professional dropout	-0.066***	-0.191***	-0.089***	-0.221***
	(0.007)	(0.023)	(0.006)	(0.019)
technical dropout	0.039***	0.040**	0.032***	0.003
	(0.007)	(0.019)	(0.005)	(0.015)
observations	32,871	25,823	61,582	46,875

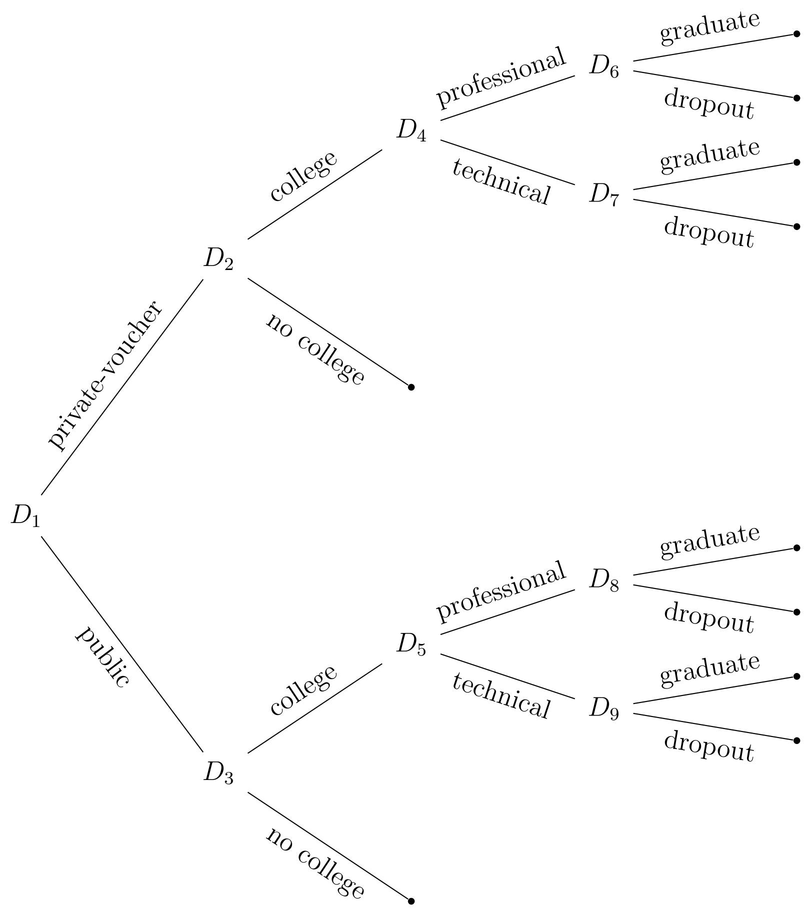
Notes: OLS value-added estimates for the cohort of vulnerable students, estimated separately for private-voucher and public high-school graduates. Each regression controls for the four 8th-grade SIMCE test scores; standard errors, in parentheses, are clustered at the 8th-grade school. The omitted category within each sector is a high-school diploma with no higher education. Columns 1 and 3 report effects on formal employment; columns 2 and 4, on log wages, estimated on the formally employed subsample. *, **, ***: significant at 10%, 5%, 1%.

From averages to distributions. The reduced-form evidence establishes the central averages of this paper: the labor-market payoff to higher education is governed by its type and completion, an incomplete professional spell is the single negative-return outcome on wages, and the voucher high school both raises earnings directly and reshapes which of these outcomes its students reach. What the VA approach cannot deliver is the distribution of these effects—which students gain from a professional track and which would have been better served by a technical one, how the returns vary with unobserved ability, and how employment and wages would respond under counterfactual policies that expand or restrict access to the voucher sector. Answering these questions requires moving from average effects to a model of the individual schooling decisions that generate them. We develop and estimate such a model in the next section.

5 A Sequential Model of Schooling Decisions and Outcomes

We postulate a model of sequential schooling decisions, that starts with the choice of whether to enroll in a private-voucher or public high school, and is followed by all major schooling decisions the student makes thereon, including enrolling in college, choosing between a professional and a technical degree, and obtaining a college degree. Figure 1 displays the complete diagram of the decisions incorporated in our model.

Figure 1: Sequential Schooling Decisions



Notes: This figure displays the multistage decision process we incorporate in our model. Node D_1 relates to the decision of attending a private-voucher or a public high school. Nodes D_2 and D_3 relate to the decision of whether to enroll in college. Nodes D_4 and D_5 relate to the decision of the type of higher education degree to pursue, professional or technical. Nodes D_6–D_9 relate to the decision of whether to graduate from college. A terminal node is denoted by a dot at the end of a branch.

Our model is a generalization of the Roy framework (Roy, 1951; Heckman and Honoré, 1990). We characterize each schooling decision using a flexible discrete choice model, and recognize that individuals make decisions by taking into account the consequences of their choices. In that sense, our model approximates a dynamic discrete choice model without being explicit about the precise rules used by individuals or about their information sets.[11] On the other hand, and similar to reduced-form strategies, we use exclusion restrictions as a source of identification. However, unlike that literature, we are able to define a variety of treatment effects at well-defined margins

[11]See the discussion in Taber (2000), Cameron and Taber (2004), and Heckman et al. (2018).

of choice. We also allow for unobserved heterogeneity to play a key role in determining choices and outcomes. Moreover, we are able to interpret the unobserved heterogeneity as a combination of students' inherent scholastic abilities, following the identification argument in Carneiro et al. (2003), and Hansen et al. (2004).[12]

5.1 Schooling Decisions and Counterfactual Outcomes

Let $D_{j'|j}$ denote the decision of attaining schooling level j' given that the student attained schooling level j. For instance, $D_{j'|j}$ may denote the decision of graduating from college given that the student enrolled in college (nodes D_6–D_9 in Figure 1). The optimal decision process is assumed to be characterized by an index threshold-crossing model,

$$D_{j'|j} = \begin{cases} 1 & \text{if } I_{j'|j} \geq 0 \\ 0 & \text{otherwise,} \end{cases}$$

where $I_{j'|j}$ is the perceived value of attaining schooling level j' for a student with schooling level j.

5.2 Parameterization and Assumptions About Unobservables

We use a linear-in-the-parameters model to approximate student's perceived value of transitioning from schooling level j to schooling level j',

$$I_{j'|j} = Z_{j'|j}\gamma_{j'|j} + \theta\lambda_{j'|j} + \nu_{j'|j},$$

where $Z_{j'|j}$ is a vector of contemporaneous observed variables that affect the schooling decision, θ is student's unobserved (to the econometrician) heterogeneity, and $\nu_{j'|j}$ is an idiosyncratic error term. Notice that we do not impose forward-looking behavior or any type of rationality, which adds flexibility to our model, at the cost of not being able to identify fully structural preference parameters.

The individual's unobserved heterogeneity plays an important role in our methodology.[13] In particular, we assume that, after controlling for observables, the unobserved heterogeneity drives all statistical dependence between the equations in our model. That is, if the researcher were able to observe θ, she could use matching on (Z, X, θ) to fully identify the model (Carneiro et al.,

[12] See, also, Heckman et al. (2006), Urzua (2008), Rau et al. (2013), and Rodríguez et al. (2016).

[13] This is a common feature of the literature on dynamic discrete choice models. See, for example, Keane and Wolpin (1997), and Aguirregabiria and Mira (2010).

2003).[14]

Let $\perp\!\!\!\perp$ denote statistical independence. We assume that, conditional on (Z, X),

$$\begin{aligned} \nu_{j'|j} \perp\!\!\!\perp \nu_{j'''|j''}, &\qquad \text{for any } j'|j \neq j'''|j'' \\ \nu \perp\!\!\!\perp \theta, & \end{aligned}$$

where ν is the stacked vector of all $\nu_{j'|j}$ terms. In addition, we assume that all unobservables are statistically independent from the observable variables, i.e $(\nu, \theta) \perp\!\!\!\perp (Z, X)$.

5.3 Measurement System for the Unobserved Heterogeneity

We adjoin to our model a measurement system of equations to help identify the distribution of the unobserved heterogeneity, θ, as well as to facilitate its interpretation. Notice that the use of a measurement system is not strictly necessary for the model of schooling decisions and outcomes to be identified, and we could instead treat the unobserved heterogeneity as a nuisance or random effect, as is usual in the structural literature (see, e.g., Keane and Wolpin, 1997, and Aguirregabiria and Mira, 2010). However, the use of a measurement system allows us to give a clear interpretation to the unobserved factor as a proxy of the measurements (Carneiro et al., 2003; Hansen et al., 2004).

The measurement system is given by,

$$M_l = X^M \delta_l + \theta \psi_l + \epsilon_l,$$

where M_l is the l-th measurement, X^M is a vector of observed variables determining the measurement, and ϵ_l is an idiosyncratic error term. Notice that the measurements do not depend on the schooling levels, and therefore are observed for all students independent of their schooling choices. In practice, we use measurements taken before the student makes the decisions relevant to our model.

In addition to the independence assumptions mentioned above, we assume that $\epsilon_l \perp\!\!\!\perp \epsilon_{l'} | (Z, X)$ for any $l \neq l'$, and that $\epsilon_l \perp\!\!\!\perp (Z, X, \theta, \nu)$.

5.4 Identification

The main argument for identification of our model uses a version of matching, and follows Heckman et al. (2016). If θ were observed, then we could condition on (Z, X, θ), and identify all parts of the model. Since θ is not observed, we use the measurement system to proxy θ, and allow

[14] (Z, X) denotes the stacked vector of all observables.

for measurement error captured by $\epsilon = (\epsilon_1, \ldots, \epsilon_L)$. Using the conditions presented in Heckman et al. (2016), we can nonparametrically identify our model, including the distributions of θ and ϵ. This last part in the identification requires that we have at least three measurement equations for a one-dimensional factor structure.[15] In practice, we use four measurements.

The factor loadings are identified up to a normalization. We set one of the loadings in the measurement system to be equal to one, which anchors the scale of the factor.

We also benefit from the use of instruments, that is, variables that shift the schooling decisions but that do not enter the outcome equations. This identification argument works even without imposing a factor structure in the error terms or invoking the independence conditional on (Z, X, θ) assumption. See Heckman et al. (2016) and the papers cited therein for a formal proof of identification of a general version of a model that shares the characteristics of ours.

5.5 Estimation

We estimate the model by maximum likelihood. The conditional on (Z, X, θ) independence of the error terms assumption is key when constructing the likelihood function, which is given by,

$$\begin{aligned}\mathcal{L} &= \prod_{i=1}^{N} \int f(Y_i, D_i, M_i \mid Z_i, X_i, \theta) f(\theta) d(\theta) \\ &= \prod_{i=1}^{N} \int f(Y_i, D_i \mid Z_i, X_i, \theta) f(M_i \mid X_i, \theta) f(\theta) d(\theta),\end{aligned}$$

where $f(\cdot)$ denotes a probability density function, and we integrate over the distribution of the unobserved factor. We assume mean zero normal distributions for the error terms, and approximate the distribution of the factor using a mixture of two normals. That is,

$$\theta \sim pN(\mu_1, \sigma_1^2) + (1-p)N(\mu_2, \sigma_2^2),$$

where we constrain the factor mean to be zero, and $p \in [0, 1]$.

We perform the estimation in two stages. First, we estimate the measurement system and factor distribution parameters, using the following first-stage likelihood function,

$$\mathcal{L}^1 = \prod_{i=1}^{N} \int f(M_i \mid X_i, \theta) f(\theta) d(\theta).$$

We obtain estimates $\hat{f}(M_i|X_i, \theta)$ and $\hat{f}(\theta)$, which we use to form the second-stage likelihood

[15] See, also, Carneiro et al. (2003), Hansen et al. (2004), and Theorem 1 in Kotlarski (1967).

function,

$$\mathcal{L}^2 = \prod_{i=1}^{N} \int f(Y_i, D_i \mid Z_i, X_i, \theta) \hat{f}(M_i \mid X_i, \theta) \hat{f}(\theta) d(\theta).$$

By proceeding the estimation in two stages, we reduce the computational time, but also ensure that the estimation of the factor distribution is done using only the measurement system, which reinforces the interpretation of the unobserved factor as a proxy of the measurements. Moreover, since the schooling decisions, D, and outcomes, Y, are independent of the measurement system, M, conditional on (Z, X, θ), we obtain consistent estimates.

We use the Gauss-Hermite quadrature for integration, and correct the second-stage standard errors following the procedure suggested by Murphy and Topel (1985).[16]

6 Empirical Implementation

6.1 Schooling Decisions

As displayed in Figure 1, we model four levels of schooling choices the students make through the course of their education: enrolling in a private-voucher high school in lieu of a public high school, enrolling in college, choosing between a professional and a technical degree, and graduating from college (for students enrolled in college). Table 3 displays summary statistics for the schooling choices observed in our sample (students that attended primary public schools offering only primary grades). About a third of students enroll in private-voucher high schools, and about 48% enroll in a higher education institution. Only about 15% of students in our sample earn a higher education degree. Lastly, there is a considerable number of students that drop out from college: of the students ever enrolled in college, only 31% are able to graduate (i.e. finish their degree).

6.2 Outcomes

The final outcomes in our model are the individual's labor-market outcomes: formal employment and annual labor earnings, both measured in 2013—when the typical student in our sample is about 27 years old, roughly nine years after graduating from high school—from the unemployment-insurance administrative records described in Section 3. About 77% of the students in our sample are formally employed by then. Labor earnings are observed only for the formally employed; we treat the remaining students as not employed, so that employment constitutes the extensive margin of our labor-market outcomes. Table 3 reports summary statistics

[16]See, also, Greene (2008).

for these outcomes.

The outcomes of enrolling in college, and graduating from college are part of the schooling choices in our model, and were analyzed above.

6.3 Measurement System

For the measurement system, we use the 8th grade national standardized exams taken in 2000, which was the first year these tests were administered.[17] The exams are mandatory for all students in 8th grade, and evaluate knowledge in four subjects: verbal, mathematics, social sciences, and natural sciences. We include all four tests as part of our measurement system. These tests help us identify the distribution of the unobserved heterogeneity in our model, and allow us to interpret the unobserved factor as a combination of student's inherent scholastic abilities. In the remainder of the paper, we also refer to the unobserved heterogeneity as ability.

We normalize each test score distribution to have mean zero and standard deviation one in the entire population of test takers. Table 3 displays summary statistics for the tests that are part of the measurement system. All four tests have sample means of about -0.14σ, which tell us that on average the students in our sample perform worse than the students in the entire population.

[17] In 1999, a similar battery of tests were administered to 4th graders. In subsequent years, different national standardized tests have been administered to students in 2nd, 4th, 6th, 8th, 10th, and 11th grades.

Table 3: Summary Statistics — Endogenous Variables

	mean	std. dev.	min	max
schooling decisions:				
voucher high school	0.35	0.48	0.00	1.00
college enrollment	0.48	0.50	0.00	1.00
professional enrollment	0.26	0.44	0.00	1.00
technical enrollment	0.21	0.41	0.00	1.00
college graduation (cond. enrolled)	0.31	0.46	0.00	1.00
college degree (unconditional)	0.15	0.36	0.00	1.00
labor market:				
employed	0.77	0.42	0.00	1.00
log annual income (if employed)	8.12	1.15	-2.39	10.91
measurement system:				
verbal	-0.14	0.91	-2.81	2.90
math	-0.14	0.91	-2.75	2.63
social sciences	-0.15	0.91	-2.84	3.06
natural sciences	-0.14	0.92	-2.76	2.81

Notes: Summary statistics for the endogenous variables in our estimation sample (N=94,453). Professional and technical enrollment are unconditional shares (they sum to college enrollment); college graduation is conditional on having enrolled, and college degree is unconditional. Labor income is observed only for the formally employed (the income row is computed on that subsample); log annual income is in log US dollars. College-admission and 8th-grade (measurement-system) test scores are normalized to mean zero and standard deviation one in the population of test takers.

6.4 Covariates

The exogenous variables we use in our model's equations mostly reflect socio-economic characteristics, and are common in the related education/labor literature (see, e.g., Altonji (1993), Heckman et al. (2006), Heckman et al. (2018), Walters (2017)). Table 4 lists the covariates we use, and displays summary statistics for each of them. The variables for gender, parents' education, broken home, and household income are measured when the student is 14 years old; we also observe region of residence at ages 14 and 18. The variable for broken home is a dummy that takes the value of one if no parent or only one of them resides in the student's home, and zero otherwise. Because these socio-economic variables are missing for a non-trivial share of students, we enter parents' education, broken home, and household income as categorical indicators with a separate missing-data category, rather than dropping the affected observations. About half of our sample is male. Reflecting the vulnerable population we study, most parents did not complete secondary education: only about a quarter of mothers and fathers have twelve or more years of schooling, and roughly a fifth have missing parental education. Household income is reported in brackets, with about 29% of households earning between CLP 100K and CLP 200K per month and 17% earning more than CLP 200K. Lastly, about a sixth of the students in our sample reside in the country's Northern region, and about half reside in the Central region, with the rest in the South.

6.5 Exclusion Restrictions

As stated in Section 5.4, our model is nonparametrically identified without the need of exclusion restrictions, and invoking a form of conditional independence assumption on mismeasured variables. However, we could also identify all parameters in our model with the use of node-specific instruments, or variables that shift the schooling choices but that do not enter the outcome equations. With instruments, identification is secured even without relying on the matching-like assumption just mentioned.

We use a variety of instruments that shift the schooling decisions. Table 4 lists these instruments, and provide summary statistics for each of them. The variable for the percentage of voucher high schools in the municipality is measured when the student is 14 years old. The variables for local unemployment and wages are differences between high-skilled and low-skilled municipality averages. The variable for presence of a college in the municipality is a dummy that is equal to one if there is one or more colleges in the student's municipality of residence, and zero otherwise. On average, students live in a municipality where 53% of high schools are private-voucher. The local labor-market differentials capture the relative tightness of the skilled labor market: high-skill unemployment rates are on average about 4 percentage points lower than low-skill rates, and high-skill wages exceed low-skill wages. About 60% of students reside in a municipality with at least one college, and about 52% in one with a professional college. Notice that we observe many of the instruments at different moments in the course of the student's education.

Table 4: Summary Statistics — Exogenous Variables

	mean	std. dev.	min	max
covariates:				
male	0.51	0.50	0.00	1.00
mother's education: 12 years	0.19	0.39	0.00	1.00
mother's education: more than 12 years	0.06	0.23	0.00	1.00
mother's education: missing	0.18	0.38	0.00	1.00
father's education: 12 years	0.21	0.41	0.00	1.00
father's education: more than 12 years	0.07	0.26	0.00	1.00
father's education: missing	0.20	0.40	0.00	1.00
broken home	0.23	0.42	0.00	1.00
broken home: missing	0.23	0.42	0.00	1.00
household income: CLP 100–200K	0.29	0.46	0.00	1.00
household income: more than CLP 200K	0.17	0.37	0.00	1.00
household income: missing	0.25	0.43	0.00	1.00
region: north (age 14)	0.16	0.37	0.00	1.00
region: center (age 14)	0.51	0.50	0.00	1.00
region: north (high school)	0.16	0.37	0.00	1.00
region: center (high school)	0.51	0.50	0.00	1.00
exclusion restrictions:				
% voucher schools in municipality	0.53	0.25	0.00	1.00
local unemployment gap (high–low skill, 2003)	-0.04	0.05	-0.21	0.32
local wage gap (high–low skill, 2003)	4.56	2.67	0.03	33.59
college in municipality	0.60	0.49	0.00	1.00
professional college in municipality	0.52	0.50	0.00	1.00
local unemployment gap (high–low skill, 2009)	-0.04	0.05	-0.22	0.26
local wage gap (high–low skill, 2009)	4.46	2.67	-0.86	42.18
local unemployment, high-skill (2013)	0.11	0.10	0.00	1.00
local unemployment, low-skill (2013)	0.12	0.06	0.00	0.74
local wage, high-skill (2013)	6.28	0.29	4.74	7.66
local wage, low-skill (2013)	5.71	0.19	4.74	6.23

Notes: Summary statistics for the covariates and exclusion restrictions in our model (see Table B.1 for their allocation across equations). Education, broken-home, household-income, and region variables enter as categorical indicators; the omitted categories are education below 12 years, non-broken home, household income below CLP 100K per month, and the southern region, with a separate missing-data category for each. Variables measured at age 14 (the primary-school stage) enter the measurement system and the high-school choice; their high-school-stage analogues enter the later equations. The exclusion restrictions are the local availability of private-voucher schools, of a college, and of a professional college, and the local high-skill vs. low-skill labor-market differentials measured at different ages.

Finally, Table B.1 in Appendix B displays the exact inclusion of the covariates and instruments in the various equations of our model.

7 Results

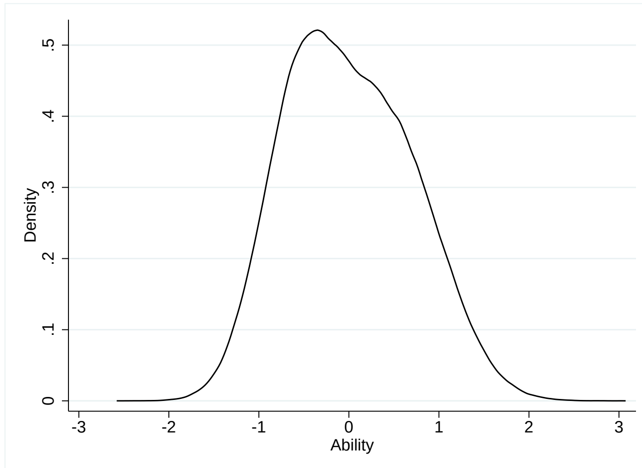
Our results proceed in three steps. We first ask what each schooling option is worth to a student who is choosing among them—not only what it pays on average, but how much risk it carries, since enrolling in a program is a bet on completing it. We then ask what attending a private-voucher high school does to the schooling options students actually take up. Finally, we put the two together, decomposing the effect of vouchers on labor market outcomes into the schooling flows they induce and the returns those destinations pay. That decomposition is where the section arrives: the wage gain documented in Section 4 survives the structural model intact, but almost none of it travels through the schooling the voucher induces. We begin with the estimates themselves and the fit of the model.

7.1 Estimates

7.1.1 Distribution of the Unobserved Heterogeneity

Figure 2 displays the estimates for the distribution of the unobserved heterogeneity. It also plots the estimated distribution. A first look at the plotted distribution suggests its departure from the normal distribution. In particular, the estimated distribution presents two modes, one positive and one negative. This result confirms our approach of allowing for a degree of flexibility in the distribution of the factor.

Figure 2: Distribution of Ability



$$f \sim pN(\mu_1, \sigma_1^2) + (1-p)N(\mu_2, \sigma_2^2)$$

where

$$(\mu_1, \mu_2) = (0.44, -0.08)$$
$$(\sigma_1, \sigma_2) = (0.58, 0.44)$$
$$p = 0.19$$

Notes: This figure displays the estimated distribution of the unobserved factor. Estimated parameters were obtained from a maximum likelihood procedure on the measurement system.

7.1.2 Measurement System

The estimates for the measurement system are reported in Table B.2 in Appendix B. Consistent with other studies for the case of Chile, we find that female students outperform males in verbal, but the opposite occurs in math.[18] A gender effect also exists in social sciences, favoring males. Both mother's and father's education are strong determinant of students' performance, although the effect of mother's education is estimated to be stronger in all subjects. Residing in a household without both parents being present does not affect test scores. That is not the case for household's income per capita, which increases students' performance. Residing in the Southern region also increases test scores. Finally, the unobserved ability factor is estimated to be a strong determinant of test scores, playing a comparable role in all subjects.

[18] See Rau et al. (2013), and Rodríguez et al. (2016).

7.1.3 Schooling Decisions

Estimates for the decision of attending a private-voucher high school in lieu of a public high school are presented in Table B.3 in Appendix B. This decision corresponds to node D_1 in Figure 1. Males are more likely to enroll in a voucher high school. Higher levels of mother's education increases the likelihood that a student attends a voucher high school. The effect of father's education is not statistically different from zero. Higher levels of household income also increase the likelihood of attending a voucher school. Residing in the Northern region decreases the chances of going to a private school, and the opposite occurs with residency in the Central region; all, relative to residing in the South. Notice that the exclusion restriction, which captures local availability of voucher high schools, is a strong determinant of the decision. Lastly, higher levels of the unobserved ability increase the likelihood of enrolling in a voucher school.

Table B.3 also reports the estimates for the decision of enrolling in college, which corresponds to the nodes D_2 and D_3 in Figure 1, for students coming from private-voucher and public high schools, respectively. Female students are more likely to enroll in college. Both mother's and father's education strongly increase the likelihood of enrolling, with the effect rising in the level of parental schooling. Not having both parents at home reduces the chances of enrolling. Higher levels of household income substantially increase enrollment. Among public high school students, residing in the Northern or Central regions lowers enrollment relative to the South, whereas the regional effects are negligible for voucher students. We use three exclusion restrictions in these equations. The first is the difference between the local unemployment rate of high-skilled and low-skilled workers; the corresponding coefficients are negative, though imprecisely estimated. The second is the difference between the local log wage of high-skilled and low-skilled workers, whose estimated effect is close to zero. Our third instrument is the local availability of a higher education institution, whose coefficient is positive and significant, indicating that the presence of a college in the student's municipality of residence increases the likelihood of enrolling. Lastly, the loading for the unobserved ability factor is strongly positive, meaning that higher levels of ability markedly increase the likelihood of enrolling in college.

Estimates for the decision of pursuing a professional (rather than technical) college degree are presented in Table B.4 in Appendix B. This decision corresponds to the nodes D_4 and D_5 in Figure 1, for students who enrolled in college coming from private-voucher and public high schools, respectively; the dependent variable equals one for a professional degree and zero for a technical degree. Female students are more likely to pursue a professional degree. Family endowments matter: higher levels of parents' education and household income increase the likelihood of choosing a professional over a technical degree, while growing up without both parents present reduces it. As an exclusion restriction in these equations we use the local availability of a professional higher education institution, whose coefficient is positive (and statistically significant for public high school students), indicating that the local presence of a professional college raises the probability of pursuing a professional degree. Lastly, the loading for the unobserved ability factor is strongly positive, meaning that higher-ability students are considerably more likely to opt for a professional degree.

The decision of graduating from college is estimated only for students who enroll in college, and its estimates are presented in Table B.5 in Appendix B. This decision corresponds to the nodes D_6–D_9 in Figure 1, which distinguish students by high school type (private-voucher or public) and by the type of degree pursued (professional or technical). Female students are more likely than males to graduate from college across all four paths. Mother's education is generally a positive determinant of graduation, whereas the effect of father's education is smaller and less consistent. Growing up in a household without both parents present decreases the chances of obtaining a degree, while higher household income increases them, particularly for students from private-voucher high schools. As exclusion restrictions we use the local labor market differentials in unemployment and wages between high-skilled and low-skilled workers, whose coefficients are mostly imprecisely estimated. Lastly, the loadings for the unobserved ability factor are positive throughout, indicating that higher-ability students are more likely to earn a college degree. This is, again, in line with the related evidence (Heckman et al., 2006, 2018; Rau et al., 2013; Rodríguez et al., 2016).

7.2 Goodness of Fit

Using the model and its estimates, we simulate 500,000 observations. We test the ability of our model to reproduce the actual data. Tables B.6 and B.7 in Appendix B compare our model's predictions with the data. Table B.6 reports comparisons for the measurement system, and Table B.7 for schooling decisions. In general, our model fits the data satisfactorily well, which allows us to use the model to compute counterfactuals, treatment effects, and learn more about the consequences of schooling decisions (in particular, attending a voucher high school).

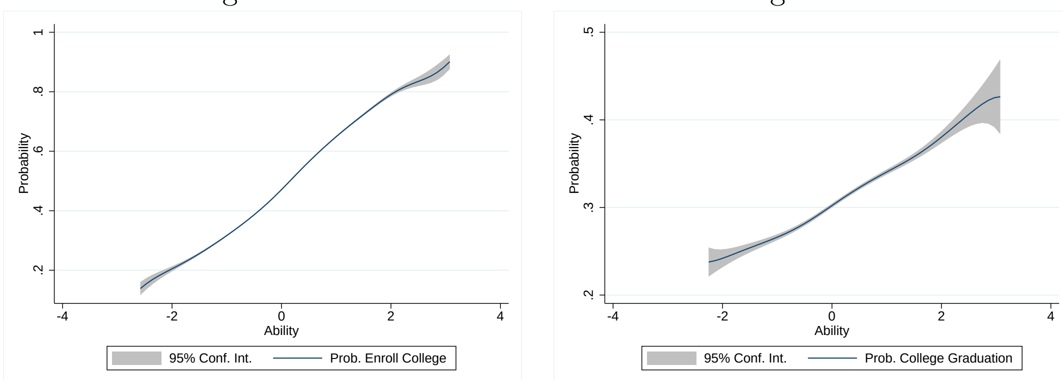
7.3 Effects of and Sorting on Ability

Using our simulated model, we investigate the effects of ability on outcomes. We are interested in the outcomes of college enrollment and college graduation (conditional on having enrolled in college). Figure 3 plots the effects of ability on each of the outcomes. Higher levels of ability strongly determine higher chances of college enrollment and graduation. These results, previewed by the analysis of estimates in Section 7.1, and confirmed with the analysis from the simulations, underscore the key role that inherent abilities have on socio-economic outcomes. They also connect this paper with a growing literature on the effects of skills (Heckman et al., 2006, 2018;

Sarzosa and Urzúa, 2021; Prada and Urzúa, 2017; Sarzosa, 2024), and call for the importance of policies aimed at developing those skills, especially in early stages of children's development (Cunha and Heckman, 2007, 2008; Cunha et al., 2010; Noboa-Hidalgo and Urzúa, 2012; Campbell et al., 2014; Heckman and Mosso, 2014).

Figure 3: The Effects of Ability on Outcomes

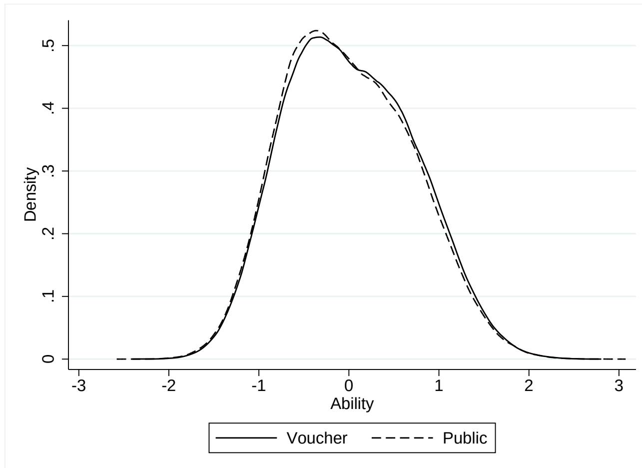
A. College Enrollment B. College Graduation



Notes: This figure plots nonparametric relationships between abilities and outcomes. The nonparametric estimations are performed using the simulations from the model. College graduation is conditional on having enrolled in college.

We also investigate how ability determines the sorting of students into intermediate and final schooling levels. In particular, we are interested in the sorting effect of ability on students' decision to attend a private-voucher high school, and on final schooling levels (i.e. high school degree, college dropout, college degree). Figure 4 plots distributions of ability for students deciding to attend a private-voucher school and for students deciding to attend a public school. The two distributions are very similar one from another. If anything, students in voucher schools have somewhat higher levels of ability, but the differences are almost negligible. We conclude that students of similar ability enroll in private-voucher schools and in public schools.

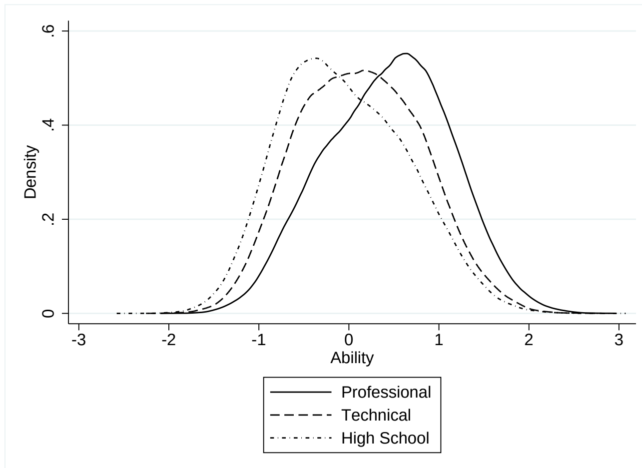
Figure 4: Distribution of Ability by Voucher/Public High School



Notes: This figure plots distributions of ability for students attending private-voucher schools and for students attending public schools. The distributions are nonparametrically estimated using the simulations from the model.

Figure 5 plots distributions of ability for students attaining the following final schooling levels: high school degree, college dropout (i.e. enrolling in college but not earning the degree), and college degree. The sorting on ability is clear. Individuals earning a college degree have higher levels of ability than individuals dropping out from college, who in turn have higher levels of ability than individuals earning a high school diploma. These results are in line with the existing evidence on the role of skills in educational attainment (Heckman et al., 2006, 2018; Rodríguez et al., 2016; Prada and Urzúa, 2017), and, again, highlight the key role of ability in educational success.

Figure 5: Distribution of Ability by Final Schooling Level



Notes: This figure plots distributions of ability for individuals attaining three different levels of schooling: high school degree, college dropout, and college degree. The distributions are nonparametrically estimated using the simulations from the model.

7.4 The Returns to Education, and the Risk of Enrolling

We begin our examination of the returns to education for our sub-population of vulnerable students by comparing the labor market outcomes associated to different final schooling levels, in a pairwise fashion. To facilitate exposition, we contrast each final schooling level with respect to high school diploma. Specifically, we compute,

$$ATE_{j,0} \quad = \quad \int\int E(Y_j - Y_0 \mid X = x, \theta = \bar{\theta}) dF_{X,\theta}(x, \bar{\theta}),$$

which defines the average treatment effect of attaining final schooling level j relative to obtaining a high school diploma (denoted by the subscript 0).[19] The set of final schooling levels we analyze includes professional graduate, technical graduate, professional dropout, technical dropout. This exercise is akin to what is obtained from a linear regression of outcomes projected onto final schooling levels dummies, with high school diploma the base category (Rodríguez et al., 2016).

[19] In practice, we weigh each individuals' treatment effect by her likelihood of attending a private-voucher and a public high school:

$$\begin{aligned} ATE_{j,0} \quad = \quad & \int\int E(Y_j - Y_0 \mid X = x, \theta = \bar{\theta}, D_1 = 1) \Pr(D_1 = 1 \mid X = x, \theta = \bar{\theta}) + \\ & E(Y_j - Y_0 \mid X = x, \theta = \bar{\theta}, D_1 = 0) \Pr(D_1 = 0 \mid X = x, \theta = \bar{\theta}) dF_{X,\theta}(x, \bar{\theta}). \end{aligned}$$

We compute our estimates for the ATE parameters by averaging over the $Y_j - Y_0$ distribution, using our simulations from the model. Table 5 displays the results. Panel A shows returns to education on employment, while panel B does similarly on log annual wages. In addition to the average effect in the overall distribution, we explore heterogeneity by reporting returns separately for students in the bottom, middle, and top tercile of the ability distribution, in columns (2) to (4). We use terciles of ability consistently throughout the paper. All final schooling levels, except for technical dropout, are associated to reducing formal employment. This finding is possibly explained by high school graduates entering the labor market sooner than individuals with some higher education, having accumulated more work experience and on-the-job knowledge by age 27. Graduating from either a professional or a technical degree reduces formal employment by 3 p.p. Dropping out from a professional degree triples that employment reduction effect, while technical degree dropout is associated to increasing formal employment by about 2.5 p.p., possibly reflecting the value that firms put on the practical skills acquired at technical schools, on top of the ability signal of the degree (Arteaga, 2018).

The heterogeneity is concentrated in the dropout states rather than in the degrees. The employment penalty to professional dropout deepens monotonically with ability, from 5.0 p.p. in the bottom tercile to 13.4 p.p. in the top, whereas the penalty to a professional degree steepens less sharply, from an insignificant 1.7 p.p. at the bottom to 4.6 p.p. at the top. Technical dropout, by contrast, raises employment throughout, and by more for higher-ability students.

Panel B shows that higher education degrees are associated to substantial returns to education. Obtaining a professional degree increases wages by 0.60 log points relative to a high school diploma—about 83 percent—while a technical degree increases wages by 0.37 log points, or about 44 percent. Interestingly, the wage returns to dropping out of a professional degree are negative and large, at -0.18 log points, or about 17 percent. Dropping out of a technical program, by contrast, leaves wages statistically indistinguishable from a high-school diploma, at 0.019, so the negative return belongs to the incomplete professional spell specifically rather than to incomplete higher education generally. This negative return to an incomplete professional spell contrasts with evidence for the United States, where some college without a degree still tends to carry a positive wage return (Heckman et al., 2018), and it foreshadows the central role that program type and completion play in what follows. Read together with Panel A, completion governs the wage payoff while program type governs the employment one. The penalty is also strongly graded by ability: it rises from -0.10 for students in the bottom tercile to -0.27 for those in the top, so the students with most to gain from a professional degree are also those who lose most by not finishing it. The return to a professional degree, by contrast, is essentially the same at every ability level (0.60 in each tercile).

Table 5: Returns to Education for Final Schooling Levels

	all (1)	low (2)	middle (3)	high (4)
A. Employment				
professional graduate	-0.031***	-0.017	-0.031***	-0.046***
	(0.007)	(0.011)	(0.007)	(0.006)
technical graduate	-0.030***	-0.037***	-0.031***	-0.022***
	(0.005)	(0.010)	(0.005)	(0.008)
professional dropout	-0.089***	-0.050***	-0.084***	-0.134***
	(0.004)	(0.007)	(0.005)	(0.006)
technical dropout	0.025***	0.017***	0.024***	0.035***
	(0.004)	(0.006)	(0.004)	(0.007)
B. Log wages				
professional graduate	0.602***	0.603***	0.604***	0.598***
	(0.026)	(0.039)	(0.026)	(0.026)
technical graduate	0.367***	0.329***	0.371***	0.401***
	(0.020)	(0.029)	(0.020)	(0.027)
professional dropout	-0.181***	-0.099***	-0.173***	-0.271***
	(0.018)	(0.028)	(0.018)	(0.020)
technical dropout	0.019	0.040**	0.025*	-0.007
	(0.014)	(0.020)	(0.014)	(0.022)

Notes: Estimated average treatment effects (ATE) of each final schooling level relative to a high school diploma, on employment (Panel A) and log annual wages (Panel B). Columns (2) to (4) split students into terciles of the ability distribution. Point estimates are from the model; standard errors in parentheses are from a parametric bootstrap (500 draws of the parameters from their estimated asymptotic distribution). *, **, ***: significant at 10%, 5%, 1%.

The returns vary with ability, but not with family background. Table B.8 in Appendix B splits the sample by mother's education, our marker of vulnerability, and the returns are essentially flat across the two groups: the wage return to a professional degree is 0.61 for vulnerable students and 0.60 for their non-vulnerable peers, and the remaining schooling levels are similarly close. Family background shapes which schooling levels students reach far more than it shapes the return to any given level.

It is reassuring that these structural returns line up closely with the reduced-form value-added estimates of Section 4, which rest on a different identifying assumption. The structural wage return to a professional degree, 0.60, is within roughly six log points of the value-added estimate of 0.66; the structural penalty to professional dropout, -0.18, closely tracks the value-added -0.21; and the technical-degree return is close under both, at 0.38 in the value-added estimates and 0.37 in the structural model. That two methods resting on different assumptions deliver nearly the same average wage returns lends credibility to the structural model, which we now use to isolate the role of vouchers.

Tables 5 and B.8 report the returns to schooling states. A student, however, does not choose a state. She chooses a program, and enrolling in it is a bet: she reaches the graduate state only if she completes it, completion is far from assured—24% in professional programs and 33% in technical ones—and what failing to complete costs her depends on which program she entered. The object relevant to her decision is the return to enrolling, which integrates over her own subsequent graduation decision, together with the risk that comes attached to it.

Our model delivers this object directly. For each student we know the probability she completes each type of program and the wage distribution she faces in every terminal state, so the outcome distribution associated with an option is a mixture of state-specific distributions weighted by her completion probability. We compare the outcome distributions of the options themselves, and differences between their quantiles, rather than the distribution of individual differences $Y_a - Y_b$. This distinction matters for what we can credibly claim: the dependence between potential wages across counterfactual states is imposed by our conditional-independence assumption rather than estimated, and the estimated factor loadings in the wage equations are small relative to the residual standard deviations, so the model has almost no cross-state dependence to speak of. Each option's marginal distribution, by contrast, is pinned down by the data, and that is all our comparisons use.

Table 6 reports the result, and it overturns the natural presumption about the two tracks. Enrolling in a professional program raises the expected log wage by 0.013 relative to stopping at a high school diploma—a return statistically indistinguishable from zero—while enrolling in a technical program raises it by 0.141. The 0.128 log point gap between the two is precisely estimated. On employment the ordering is starker still: the professional option lowers the probability of formal employment by 7.4 p.p., while the technical option leaves it essentially unchanged. Professional programs do pay far more in wages when completed—the 0.60 log point degree premium of Table 5, though even a completed professional degree lowers formal employment by 3.1 p.p.—but few students complete them, and an incomplete professional spell is worse than never having enrolled on both margins. Taken as an option rather than as a destination, the professional track is not a high-return, high-risk choice: over this population it is dominated.

It is also the riskier of the two, for two distinct reasons. The standard deviation of the log wage a student faces is 1.30 under the professional option against 1.08 under the technical one. Part of that gap is the completion lottery itself, which is both less favorable and more consequential on the professional track, where the distance between the graduate and dropout outcomes is widest; completion risk accounts for 7.0% of the variance of the professional option against 2.5% of the technical one. The rest is that professional dropouts face a substantially more dispersed wage distribution than technical dropouts, with a residual standard deviation of roughly 1.3 against 1.0. Both channels push in the same direction, and the second is the larger of the two—a point worth stating plainly, because it means the riskiness of the professional track is not only about the chance of failing to finish.

Table 6: The Returns to Enrolling: Professional versus Technical

	professional (1)	technical (2)	high school (3)
A. What the option delivers			
expected log wage	8.070	8.198	8.057
	(0.014)	(0.010)	(0.007)
return to enrolling	0.013	0.141***	—
	(0.016)	(0.012)	
probability of employment	0.717	0.797	0.791
	(0.003)	(0.003)	(0.002)
return to enrolling	-0.074***	0.006	—
	(0.004)	(0.003)	
expected earnings (USD/yr)	5308	5269	4748
	(102)	(74)	(45)
B. Risk			
standard deviation of log wage	1.303	1.079	1.109
	(0.006)	(0.005)	(0.003)
variance: completion risk	0.118	0.029	—
	(0.008)	(0.004)	
variance: within-state wages	1.551	1.061	1.107
	(0.014)	(0.009)	(0.005)
variance: ability and background	0.028	0.074	0.122
	(0.003)	(0.005)	(0.004)
completion risk (percent of variance)	7.0	2.5	—
	(0.5)	(0.3)	
C. The gamble			
probability of graduating	0.237	0.333	—
	(0.003)	(0.003)	

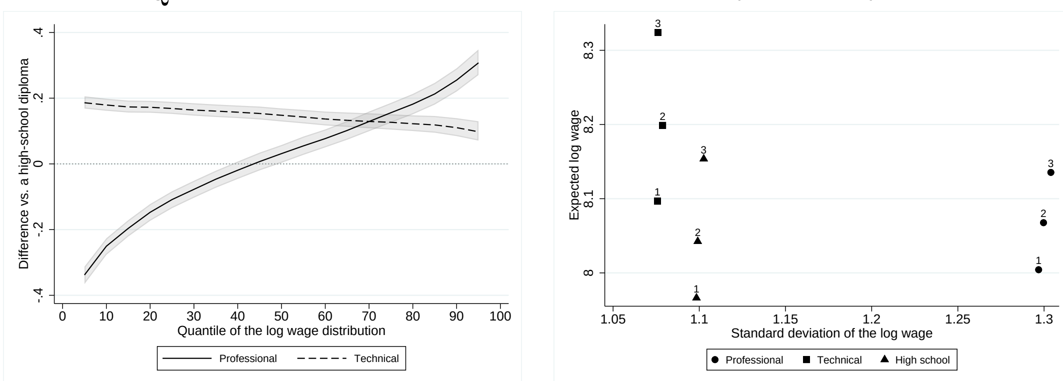
Notes: Each column describes the outcome distribution a student faces upon choosing that schooling option, integrating over her own subsequent graduation decision. Column (3) is the high-school-diploma baseline, so the rows labeled *return to enrolling* are columns (1) and (2) minus column (3). Expected log wage and the variance components are computed from the model's conditional means and completion probabilities given ability and covariates; the variance of an option decomposes into completion risk, the dispersion of wages within a terminal state, and the dispersion of conditional means across students. Standard errors in parentheses are from a parametric bootstrap (500 draws of the parameters from their estimated asymptotic distribution). *, **, ***: significant at 10%, 5%, 1%.

Figure 6 makes the comparison concrete. Panel A plots, at each quantile, the difference between the option's log wage distribution and the high-school baseline. The technical option shifts the distribution up almost uniformly, by between 0.10 and 0.19 log points across the whole range. The professional option instead rotates it: at the fifth percentile it pays 0.34 log points less than a high school diploma, it crosses zero in the low forties, and by the ninety-fifth it pays 0.31 more. The shaded bands make clear that the rotation is precisely estimated: the professional curve is significantly negative below the fortieth percentile and significantly positive above the forty-fifth. Enrolling in a professional program is a spread bet; enrolling in a technical one is close to a sure gain. Panel B places the two options in the mean–standard-deviation plane, by ability tercile, and shows the dominance directly: every professional point lies below and to the right of its technical counterpart.

Figure 6: The Distribution of Returns to Enrolling

A. Quantile Differences

B. Risk and Return



Notes: Panel A plots, at each quantile of the log wage distribution, the difference between the distribution a student faces upon enrolling in a professional (solid) or technical (dashed) program and the distribution she faces with a high school diploma. Shaded areas are pointwise 90% parametric-bootstrap bands over the 500 parameter draws. Because the dependence between potential wages across counterfactual states is imposed rather than estimated, we compare quantiles of the option distributions and never the distribution of individual differences. Panel B plots the expected log wage of each option against its standard deviation, separately by ability tercile (labeled 1, 2, 3). Both panels are computed from the model at the estimated parameters.

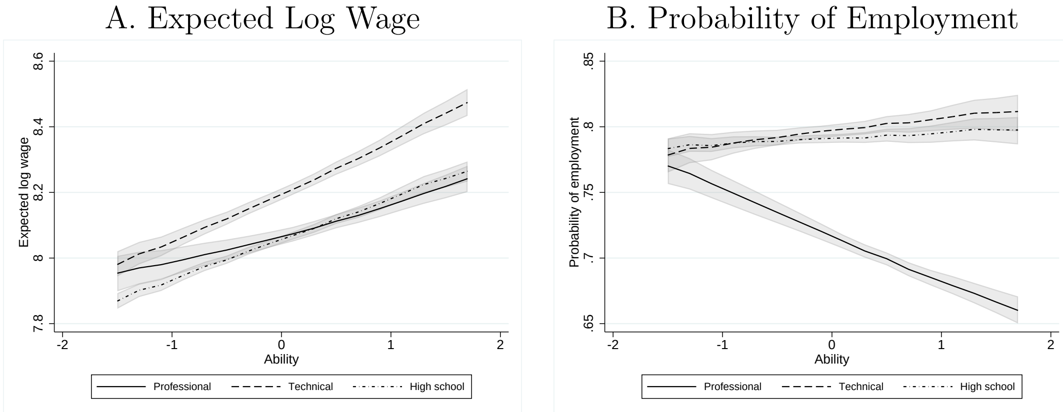
Table 7 and Figure 7 ask whether ability resolves the comparison. It does not. The expected log wage under the professional option rises from 8.01 in the bottom ability tercile to 8.14 in the top, but the technical option rises faster, from 8.08 to 8.32, so the gap widens with ability rather than closing. The employment penalty to the professional option also widens, from 4.3 p.p. at the bottom to 10.6 p.p. at the top. Indeed, for students in the top tercile the professional option delivers a slightly lower expected log wage (8.14) than stopping at a high school diploma altogether (8.16). Higher-ability students are likelier to finish—their professional completion rate is under 30 percent against under 20 percent at the bottom—but not likely enough, and their forgone employment is larger.

Table 7: The Returns to Enrolling, by Ability

	low	middle	high
	(1)	(2)	(3)
A. Enroll in a professional program			
expected log wage	8.005	8.062	8.144
	(0.023)	(0.014)	(0.014)
return to enrolling	0.043*	0.010	-0.013
	(0.025)	(0.016)	(0.018)
probability of employment	0.745	0.718	0.689
	(0.006)	(0.004)	(0.003)
probability of graduating	0.193	0.233	0.286
	(0.005)	(0.004)	(0.004)
B. Enroll in a technical program			
expected log wage	8.082	8.188	8.324
	(0.015)	(0.010)	(0.015)
return to enrolling	0.120***	0.136***	0.167***
	(0.018)	(0.012)	(0.019)
probability of employment	0.789	0.797	0.805
	(0.005)	(0.003)	(0.005)
probability of graduating	0.265	0.326	0.407
	(0.005)	(0.003)	(0.005)
C. Stop at a high-school diploma			
expected log wage	7.962	8.052	8.157
	(0.009)	(0.007)	(0.012)
probability of employment	0.788	0.791	0.794
	(0.002)	(0.002)	(0.004)
D. Technical minus professional			
difference in expected log wage	0.077***	0.126***	0.180***
	(0.027)	(0.018)	(0.021)
difference in employment	0.044***	0.079***	0.116***
	(0.007)	(0.004)	(0.006)

Notes: Columns split students into terciles of the ability distribution. Each panel describes the outcome distribution a student faces upon choosing that option, integrating over her own subsequent graduation decision; *return to enrolling* is measured relative to the high-school-diploma baseline in Panel C. Standard errors in parentheses are from a parametric bootstrap (500 draws); stars are reported only for the rows that are differences, since a test against zero is not informative for a level or a probability. *, **, ***: significant at 10%, 5%, 1%.

Figure 7: The Returns to Enrolling, by Ability



Notes: Each line plots the outcome a student expects upon choosing that schooling option, integrating over her own subsequent graduation decision, against her ability, averaged within twenty equally spaced ability bins. Shaded areas are pointwise 90% parametric-bootstrap bands, computed by recomputing each binned curve on all 500 parameter draws, so they represent uncertainty about the estimated parameters rather than simulation error.

7.5 The Effects of Vouchers on Schooling Choices

We now ask what attending a private-voucher high school in lieu of a public one does to the schooling students end up with. Section 7.4 priced the options; this section establishes which of them vouchers push students toward. Because the model delivers each student's outcomes under both branches, we compute the gain from attending a private-voucher school, $Y_1 - Y_0$, for every simulated individual, and average it over the population of interest. Averaging over all students defines the average treatment effect,

$$ATE \quad = \quad \int\int E(Y_1 - Y_0 \mid X = x, \theta = \bar{\theta}) dF_{X,\theta}(x, \bar{\theta}),$$

the gain a randomly chosen student would obtain, where $D_1 = 1$ denotes attending a private-voucher high school. Every effect we report in this section is an average treatment effect.

Before turning to the schooling margins themselves, we ask whether the returns priced in Section 7.4 depend on the sector in which a student completed secondary school. Table 8 reports the return to each final schooling level a randomly chosen student would earn were all students forced onto the voucher branch, in column (2), and onto the public branch, in column (3). Two patterns emerge. The wage return to a professional degree is lower under the voucher branch (0.57) than under the public one (0.62), while the return to a technical degree is somewhat higher (0.37 versus 0.36). Likewise, the return to an incomplete technical spell is positive under the voucher branch (0.04) and essentially zero under the public one (0.01). The voucher sector adds the most value on the technical track and the least on the professional one—precisely the branch-specific tilt that our reduced-form value-added estimates also flagged (Table 2). Because these are forced-assignment averages over the whole population, they are free of selection into the sector; the corresponding treatment-on-the-treated returns, which condition on the high school each student actually attended, are almost identical and are reported in Table B.9 in Appendix B.

Table 8: Returns to Education by High-School Type (Forced Assignment)

	all	voucher (forced)	public (forced)
	(1)	(2)	(3)
A. Employment			
professional graduate	-0.031***	-0.021*	-0.036***
	(0.007)	(0.011)	(0.008)
technical graduate	-0.030***	-0.028***	-0.033***
	(0.005)	(0.010)	(0.007)
professional dropout	-0.089***	-0.081***	-0.097***
	(0.004)	(0.007)	(0.006)
technical dropout	0.025***	0.031***	0.022***
	(0.004)	(0.007)	(0.005)
B. Log wages			
professional graduate	0.602***	0.568***	0.624***
	(0.026)	(0.040)	(0.033)
technical graduate	0.367***	0.373***	0.360***
	(0.020)	(0.035)	(0.024)
professional dropout	-0.181***	-0.196***	-0.181***
	(0.018)	(0.028)	(0.024)
technical dropout	0.019	0.042*	0.011
	(0.014)	(0.024)	(0.019)

Notes: Estimated returns to each final schooling level relative to a high school diploma, on employment (Panel A) and log annual wages (Panel B). Unlike Table B.9, columns (2) and (3) report the average treatment effect for a randomly chosen student were all students forced to attend a voucher (2) or public (3) high school, computed from the model's counterfactual outcomes for the whole population. Column (1) is the overall ATE. Standard errors in parentheses are from a parametric bootstrap (500 draws). *, **, ***: significant at 10%, 5%, 1%.

Table 9 reports the effects on every schooling margin the model distinguishes: enrollment overall and by track in Panel A, the five terminal schooling states in Panel B, completion in Panel C, and the composition of higher education among students who enroll under both branches in Panel D. Column (1) gives the share under the public branch, so each effect can be read against its own base, and column (2) reports the average treatment effect. Standard errors throughout are from a parametric bootstrap over the estimated parameters.

Attending a voucher high school raises higher-education enrollment by 1.83 p.p. on average, against a public-branch enrollment rate of 46.9%. The effect is precisely estimated, and its magnitude is in line with the small existing literature on vouchers and college enrollment (Chingos and Peterson, 2015; Chingos, 2018). What that average conceals is where the additional students go. The entire increase is professional: professional enrollment rises by 1.96 p.p., while technical enrollment does not move at all (-0.13 p.p., statistically zero). Vouchers do not expand higher education evenly across the two tracks; they expand it exclusively along the one that Section 7.4 showed to be dominated.

Panel B follows those students to where they end up, and this is the central result of the section. The 1.83 p.p. pulled out of non-enrollment do not arrive as degrees. Professional dropout rises by 1.79 p.p., while professional graduation moves by only 0.17 p.p. and cannot be distinguished from zero; technical graduation actually falls, by 0.65 p.p. Roughly nine of every ten students the voucher sends into a professional program leave it without a degree. Panel C draws the implication: attending a voucher high school lowers the probability of holding any higher-education degree by 0.48 p.p., and raises the probability of having started higher education and not finished it by 2.31 p.p. The voucher's effect on attainment, measured by degrees rather than by enrollment, is negative.

Panel D shows that this is not only a composition effect. Among students who would enroll under either branch, the voucher raises the professional share by 1.88 p.p.—the tilt again—but it also lowers graduation by 1.49 p.p., and the decline is present within both tracks, at -0.92 p.p. on the professional side and -2.16 p.p. on the technical. So the voucher does not merely reshuffle students toward a harder track; conditional on track, its students are also somewhat less likely to finish. Both margins work against completion.

Table 9: The Effects of a Voucher High School on Schooling Choices

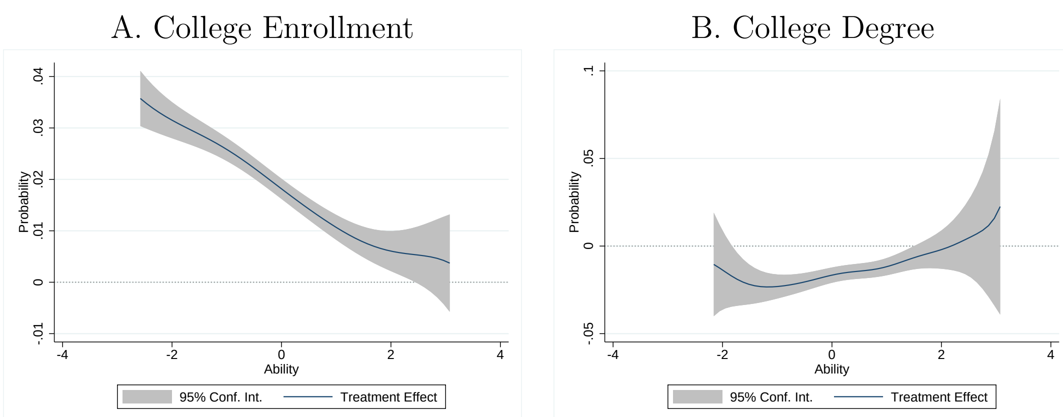
	public base (1)	ATE (2)
A. Enrollment		
any higher education	46.91	1.83***
		(0.33)
professional	25.63	1.96***
		(0.30)
technical	21.28	-0.13
		(0.28)
B. Terminal schooling state		
not enrolled	53.09	-1.83***
		(0.33)
professional graduate	7.51	0.17
		(0.19)
professional dropout	18.12	1.79***
		(0.27)
technical graduate	7.48	-0.65***
		(0.18)
technical dropout	13.80	0.52**
		(0.25)
C. Completion		
any degree	14.99	-0.48**
		(0.24)
any dropout	31.92	2.31***
		(0.32)
D. Conditional on enrolling under both branches		
professional share	60.91	1.88***
		(0.52)
graduation, any degree	—	-1.49***
		(0.50)
graduation, professional	31.98	-0.92
		(0.69)
graduation, technical	35.41	-2.16***
		(0.76)

Notes: Average treatment effects of attending a private-voucher rather than a public high school on schooling choices, in percentage points. Column (1) is the share under the public branch, so each effect can be read against its own base. Panels A–C are shares of all students, so professional and technical enrollment sum to any higher education, and any degree and any dropout sum to it as well. Panel D conditions on students who enroll under both branches. Standard errors in parentheses are from a parametric bootstrap (500 draws). *, **, ***: significant at 10%, 5%, 1%.

We next examine how the voucher effects vary with students' ability. Using the simulated sample, we estimate the statistical relation between the individual treatment gains, $Y_1 - Y_0$, and ability—what we call the effect of ability on the treatment effect. Figure 8 plots this relation for the two higher-education outcomes. For college enrollment (panel A), the effect declines steeply with ability: low-ability students benefit far more from attending a voucher high school than high-ability students, with the enrollment effect falling from about 6 p.p. at the bottom of the ability distribution to near zero at the top. For college graduation conditional on enrolling (panel B), the effect is negative over most of the ability range—about -2 p.p. for below-median students—and attenuates toward zero at higher ability levels; it is the less precisely estimated of the two, especially in the tails, where few students would enroll under both alternatives. Taken together, the voucher's gains in access accrue disproportionately to lower-ability students.

The analysis presented in Figure 8 is key to better understand how vouchers affect students. They show that the treatment effects are highly heterogeneous, and can certainly help policy-makers better target the implementation of policies.

Figure 8: The Effect of Ability on Treatment Effects



Notes: This figure plots the statistical relation between the treatment effect, $Y_1 - Y_0$, and ability, for the outcomes of college enrollment and college degree attainment. The relations are estimated nonparametrically using simulations from the model.

They are supplied by non-enrollment, not by the technical track. Non-enrollment falls by 1.83 p.p., technical graduation by 0.65 p.p., and technical enrollment as a whole does not move (-0.13 p.p.). Vouchers do not divert students from technical programs into professional ones so much as draw into professional programs students who would otherwise have stopped with a high-school diploma—the margin at which, by the estimates of Section 7.4, the professional option is worth 0.013 log points and lowers the probability of formal employment by 7.4 p.p.

The ability columns locate the effect, and the destinations differ by ability in the way those returns imply. Non-enrollment falls by 2.96 p.p. in the bottom tercile against 0.95 p.p. in the top, so more than four-fifths of the students vouchers bring into higher education come from the bottom two terciles. In the bottom tercile the inflow splits 2.17 p.p. into professional dropout and 1.08 p.p. into technical dropout against only 0.54 p.p. into professional graduation, while technical graduation falls by 0.82 p.p.: six of every seven students the voucher moves into higher education land in an incomplete spell—though the third of those who land in a technical one lose nothing by it—and the share holding any degree does not rise (-0.28 p.p.). In the top tercile professional dropout still rises, by 1.18 p.p., while professional graduation does not (-0.34 p.p., statistically zero). The effect on holding any degree is negative in all three terciles, and precisely estimated in the middle one at -0.69 p.p.

A word on what the entries are. They are changes in the share of students reaching each state rather than counts of individual transitions: the voucher leaves 1.83 p.p. fewer students unenrolled and 1.79 p.p. more holding an incomplete professional spell, which is not the same statement as that those particular students moved from the one state to the other. These net flows are what the next subsection prices at the returns each state pays.

Table 10: Where the Voucher Moves Students, by Ability

	public base (1)	all (2)	low (3)	middle (4)	high (5)
A. Terminal schooling state					
not enrolled	53.09	-1.83***	-2.96***	-1.58***	-0.95*
		(0.33)	(0.45)	(0.35)	(0.51)
professional graduate	7.51	0.17	0.54***	0.32	-0.34
		(0.19)	(0.17)	(0.20)	(0.37)
professional dropout	18.12	1.79***	2.17***	2.04***	1.18**
		(0.27)	(0.29)	(0.28)	(0.52)
technical graduate	7.48	-0.65***	-0.82***	-1.01***	-0.12
		(0.18)	(0.23)	(0.19)	(0.31)
technical dropout	13.80	0.52**	1.08***	0.24	0.24
		(0.25)	(0.35)	(0.26)	(0.39)
B. Summary					
any higher education	46.91	1.83***	2.96***	1.58***	0.95*
		(0.33)	(0.45)	(0.35)	(0.51)
professional	25.63	1.96***	2.70***	2.36***	0.83
		(0.30)	(0.33)	(0.32)	(0.53)
technical	21.28	-0.13	0.25	-0.77**	0.12
		(0.28)	(0.38)	(0.30)	(0.49)
any degree	14.99	-0.48**	-0.28	-0.69***	-0.47
		(0.24)	(0.28)	(0.26)	(0.45)

Notes: The voucher-induced change in the share of students reaching each terminal schooling state, in percentage points, for all students and within terciles of the ability distribution. Column (1) is the share under the public branch. The five entries of Panel A sum to zero within each column, so the states with negative entries are the ones that supply the students the voucher moves. Entries are changes in the share of students reaching each state rather than counts of individual transitions. Standard errors in parentheses are from a parametric bootstrap (500 draws). *, **, ***: significant at 10%, 5%, 1%.

7.6 Decomposing the Effect of Vouchers

The two preceding subsections establish separately what each schooling destination pays and where vouchers send students. This subsection combines them, and in doing so replaces the single number usually reported as "the effect of vouchers" with a set of effects that can be interpreted one at a time.

Let S^1 and S^0 denote the terminal schooling state a student reaches under the voucher and public branches, and $Y(s,d)$ her outcome in state s under branch d. Then, individual by individual,

$$Y^1 - Y^0 \quad = \quad \underbrace{\sum_s \left[\mathbf{1}\{S^1 = s\} - \mathbf{1}\{S^0 = s\}\right] Y(s,0)}_{\text{reallocation}} \; + \; \underbrace{\sum_s \mathbf{1}\{S^1 = s\} \left[Y(s,1) - Y(s,0)\right]}_{\text{within-state}},$$

an identity rather than an approximation. The first term asks what the voucher accomplishes by moving students between schooling states, valuing each state at what it pays under the public branch. The second asks what it does to students who reach the same state either way. Taking expectations and writing $\Delta\pi_s$ for the voucher-induced change in the share of students reaching state s and $\bar{R}_s$ for the average outcome in that state, the reallocation term splits further into $\sum_s \Delta\pi_s \bar{R}_s$, the net flows valued at average returns, and a covariance term that is positive when the students the voucher moves into a state are those with above-average returns to it—selection on gains at the voucher-induced margin.

Table 11 reports the decomposition. Essentially none of the voucher's labor market benefit comes from the schooling it induces. On log wages the total effect is 0.057; the within-state term accounts for 0.060, and the reallocation term is negative, at -0.004. The same holds for employment, where a total effect of 1.05 p.p. decomposes into 0.99 p.p. within state and essentially nothing through reallocation. Vouchers raise pay, but not by changing which degree students obtain—if anything, the schooling reallocation they cause subtracts from their labor market effect.

Table 11: Decomposing the Effect of a Voucher High School

	employment (1)	log wage (2)	earnings (USD/yr) (3)
A. All students			
total effect	0.0105***	0.0566***	109.8
	(0.0029)	(0.0117)	(79.7)
reallocation across states	0.0006	-0.0036*	16.2
	(0.0004)	(0.0019)	(10.9)
net flows times returns	-0.0015***	-0.0045***	-7.7
	(0.0003)	(0.0015)	(7.7)
selection on gains	0.0021***	0.0009	23.9***
	(0.0003)	(0.0009)	(6.8)
within-state effect	0.0099***	0.0603***	93.6
	(0.0029)	(0.0117)	(80.3)
B. Low ability			
total effect	0.0046	0.0366**	-35.6
	(0.0044)	(0.0157)	(85.2)
reallocation across states	-0.0002	-0.0011	16.1
	(0.0005)	(0.0020)	(16.1)
within-state effect	0.0049	0.0377**	-51.7
	(0.0045)	(0.0157)	(87.1)
C. Middle ability			
total effect	0.0104***	0.0550***	95.3
	(0.0031)	(0.0124)	(81.9)
reallocation across states	0.0018***	-0.0113***	-21.6
	(0.0005)	(0.0022)	(13.3)
within-state effect	0.0086***	0.0663***	117.0
	(0.0031)	(0.0123)	(81.2)
D. High ability			
total effect	0.0165***	0.0783***	269.6**
	(0.0046)	(0.0168)	(122.8)
reallocation across states	0.0002	0.0015	54.2***
	(0.0009)	(0.0036)	(20.8)
within-state effect	0.0163***	0.0767***	215.4*
	(0.0045)	(0.0164)	(123.1)

Notes: The effect of attending a private-voucher rather than a public high school is written as the sum of a reallocation term, which values the voucher-induced change in the distribution of terminal schooling states at the returns those states pay under the public branch, and a within-state term, which holds the schooling state fixed and asks what the voucher does to the outcome. The reallocation term is split further into net flows times average returns and a selection-on-gains covariance. The three terms add to the total by construction. Standard errors in parentheses are from a parametric bootstrap (500 draws). *, **, ***: significant at 10%, 5%, 1%.

Table 12 shows where that subtraction comes from. The 1.79 p.p. inflow into professional dropout, valued at the -0.18 that state pays, contributes -0.0033; the 0.65 p.p. of technical graduates the voucher costs, valued at +0.36, contributes another −0.0024. Against these, the 0.17 p.p. of additional professional graduates contributes only +0.0011, because so few of them are created. The selection-on-gains term is small and, for wages, statistically indistinguishable from zero: the students vouchers move are not systematically those who would gain most from moving. Effects on annual earnings measured in dollars point the same way but are far less precisely estimated—a total of roughly 110 USD with a bootstrap standard error near 80—because exponentiating log wages amplifies parameter uncertainty. We therefore read the log wage and employment decompositions as the informative ones.

Table 12: Where the Voucher Sends Students, and What Those States Pay

	net flow (pp) (1)	return (2)	contribution (3)
A. Log wages			
professional graduate	0.17	0.624***	0.00106
	(0.19)	(0.033)	(0.00117)
professional dropout	1.79***	-0.181***	-0.00325***
	(0.27)	(0.024)	(0.00067)
technical graduate	-0.65***	0.360***	-0.00235***
	(0.18)	(0.024)	(0.00066)
technical dropout	0.52**	0.011	0.00006
	(0.25)	(0.019)	(0.00012)
B. Employment			
professional graduate	0.17	-0.036***	-0.00006
	(0.19)	(0.008)	(0.00007)
professional dropout	1.79***	-0.097***	-0.00174***
	(0.27)	(0.006)	(0.00028)
technical graduate	-0.65***	-0.033***	0.00022***
	(0.18)	(0.007)	(0.00008)
technical dropout	0.52**	0.022***	0.00012*
	(0.25)	(0.005)	(0.00007)

Notes: Column (1) is the voucher-induced change in the share of students reaching each terminal schooling state, in percentage points; a negative entry for *not enrolled* (omitted) absorbs the sum. Column (2) is the return to that state relative to a high-school diploma under the public branch, and equals column (3) of Table 8. Column (3) is the product, the state's contribution to the net-flows-times-returns term of Table 11. Standard errors in parentheses are from a parametric bootstrap (500 draws). *, **, ***: significant at 10%, 5%, 1%.

The within-state term carries the entire effect. Its magnitude, 0.060 log points, is close to the 0.065 value-added estimate of the voucher wage effect in Section 4, which does not rest on the factor structure at all. Its interpretation is the conventional one in this literature: whatever voucher schools do for their students' earnings—through the quality of the schooling itself, through networks, or through unmeasured skills—they do it in a way that shows up at a given level of attainment rather than by changing that level. That is our answer to why attending a voucher school pays: not by moving students to better schooling destinations, but by raising what the destinations they already reach are worth.

The decomposition reports means. The model also delivers the distribution behind them. For a student with characteristics (X, Z) and ability θ, let $\pi_d(s)$ be the probability she reaches terminal state s under branch d, and m_s^d the outcome she expects in that state. Her expected gain from attending a voucher high school is

$$\Delta \;=\; \sum_s \pi_1(s)\, m_s^1 \;-\; \sum_s \pi_0(s)\, m_s^0,$$

the difference between what she expects on the voucher branch and what she expects on the public one, each term averaging over her own distribution of schooling destinations. Its mean across students is the total effect of Table 11; its dispersion says how differently the same policy would treat different students.

Table 13 reports that distribution and Figure 9 draws it. The mean expected log-wage gain is 0.056, reproducing the decomposition's total, but the standard deviation across students is 0.085, and about a quarter of students—25.8 percent—expect to be worse off under the voucher branch. Panel A of the figure locates them: the expected gain runs from -0.066 log points at the tenth percentile to $+0.162$ at the ninetieth, crossing zero around the twenty-sixth. The same picture holds in the other two outcomes, where 67.5 percent of students expect to earn more and 70.0 percent expect to be more likely to hold a formal job.

Ability shifts this distribution rather than merely its mean. The share of students with a positive expected wage gain rises from 66.7 percent in the bottom tercile to 83.4 percent in the top, and Panel B orders the three quantile functions accordingly at nearly every percentile. It is worth saying what ability does not do, however. A linear projection of the expected gain on ability accounts for 4.7 percent of its variance in wages and 10.5 percent in employment, so most of the variation in who benefits lies elsewhere, in the covariates and local labor-market conditions that shape which destinations a student is likely to reach. Ability sorts students across tracks and grades the penalty for not finishing, as Section 7.4 showed; it is not the main axis along which the voucher's benefits are distributed.

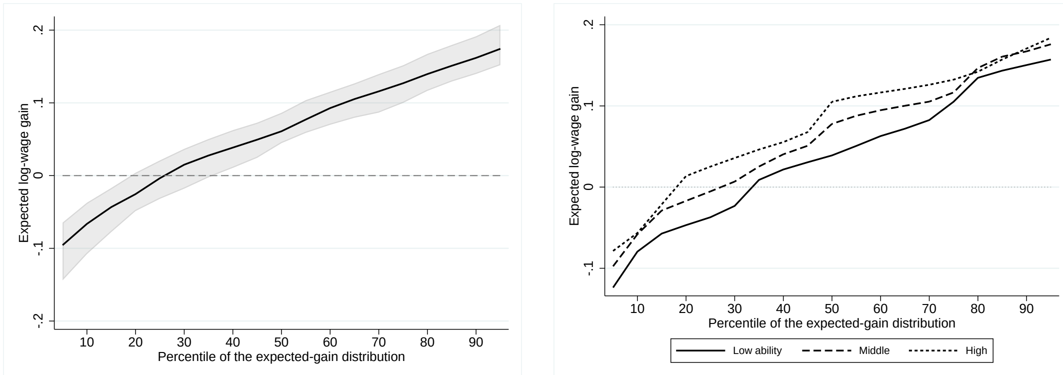
Table 13: The Distribution of Expected Gains from a Voucher High School

	all (1)	low (2)	middle (3)	high (4)
A. Expected log-wage gain				
mean	0.0563***	0.0347**	0.0589***	0.0753***
	(0.0117)	(0.0157)	(0.0123)	(0.0168)
standard deviation	0.0847	0.0872	0.0829	0.0788
	(0.0087)	(0.0087)	(0.0085)	(0.0099)
share positive (percent)	74.2	66.7	72.5	83.4
	(5.2)	(5.9)	(6.6)	(5.9)
B. Expected earnings gain (USD/yr)				
mean	122.7	-32.3	118.4	281.8**
	(80.1)	(85.0)	(81.0)	(124.5)
standard deviation	540.2	510.6	520.1	542.9
	(55.7)	(51.1)	(53.3)	(68.7)
share positive (percent)	67.5	60.9	66.1	75.6
	(5.4)	(7.0)	(5.0)	(7.5)
C. Expected employment gain (pp)				
mean	1.11***	0.37	1.06***	1.90***
	(0.29)	(0.44)	(0.31)	(0.45)
standard deviation	2.11	1.78	1.98	2.26
	(0.30)	(0.29)	(0.29)	(0.35)
share positive (percent)	70.0	55.0	73.0	82.0
	(5.5)	(10.3)	(6.6)	(4.1)

Notes: For each student the expected gain is the difference between the outcome she expects under the voucher branch and the outcome she expects under the public branch, each computed as the average over that branch's own distribution of terminal schooling states, and each a function of that branch's parameters alone. Column (1) covers all students and columns (2) to (4) terciles of the ability distribution. The mean of Panel A equals the total effect in column (2) of Table 11 up to simulation error. Standard errors in parentheses are from a parametric bootstrap (500 draws); stars are reported only for the means, since a test against zero is not informative for a dispersion or a share. *, **, ***: significant at 10%, 5%, 1%.

Figure 9: The Distribution of Expected Gains from a Voucher High School

A. All Students B. By Ability



Notes: The quantile function of the expected gain in log wages from attending a private-voucher rather than a public high school, where a student's expected gain averages the outcome she expects in each terminal schooling state over her own probability of reaching it, separately under each branch. Panel A covers all students, with the shaded area giving pointwise 90 percent parametric-bootstrap bands over 500 parameter draws. Panel B splits students into terciles of the ability distribution.

7.7 The Role of Vouchers: A Counterfactual

The treatment effects above establish that vouchers raise pay, that they raise enrollment and tilt it toward the professional track, and that completing that track is hard. We now bring these threads together in the counterfactual that motivates the paper. We take the vulnerable students who attended a public high school—those whose mothers did not complete secondary school—and ask how their schooling and earnings would have changed had they instead attended a voucher high school. Because the model delivers each student's outcomes under both branches, this counterfactual is a direct comparison of potential outcomes within this group.

Table 14 reports the resulting change in the distribution of terminal schooling states. Moving these students to the voucher branch pulls them out of non-enrollment (by 1.2 p.p.) and out of technical graduation (by 0.8 p.p.), and channels them into professional dropout, which rises by 1.5 p.p.—the dominant flow, and a precisely estimated one. The inflow into professional graduation, by contrast, is small (0.2 p.p.) and statistically indistinguishable from zero. The voucher's professional tilt for vulnerable students thus lands overwhelmingly in the one state that carries a negative wage return. The implied change in mean annual earnings is positive (about \$103) but imprecisely estimated (a bootstrap standard error of roughly \$77), so we cannot reject a null effect: for these students the voucher reshapes which schooling state they reach far more reliably than it raises their pay.

Table 14: The Voucher Counterfactual for Vulnerable Public-School Students

	Δ (voucher − public)
not enrolled (pp)	-1.16***
	(0.45)
professional graduate (pp)	0.22
	(0.25)
professional dropout (pp)	1.47***
	(0.32)
technical graduate (pp)	-0.81***
	(0.23)
technical dropout (pp)	0.28
	(0.32)
annual earnings (USD/yr)	102.7
	(77.0)

Notes: Effect of moving vulnerable public-high-school students (mother's education below 12 years) from the public to the voucher branch, computed from the model's counterfactual outcomes. Each row is the change in the share reaching that terminal schooling state (percentage points); the last row is the change in mean annual earnings (USD). Standard errors in parentheses are from a parametric bootstrap (500 draws of the parameters from their estimated asymptotic distribution). *, **, ***: significant at 10%, 5%, 1%.

This average masks sharp heterogeneity by ability—the margin along which the professional gamble pays off or fails. Table 15 splits the counterfactual by ability tercile. The inflow into professional dropout is concentrated among lower- and middle-ability students (1.8 p.p. in each) and shrinks, losing significance, for the high-ability (0.8 p.p.); completion among professional enrollees rises steeply with ability and is uniformly a touch lower under the voucher branch. The earnings consequences track this completion margin: the point estimates climb from about −\$49 for the lowest tercile to +\$280 for the highest, but only the high-ability gain is statistically distinguishable from zero. The share of students who expect to gain is estimated more sharply than the mean, and it tells the same story from a different angle: 56.6 percent of the lowest tercile expect higher earnings under the voucher branch, against 62.7 percent of the middle and 71.2 percent of the highest. The average loss at the bottom is therefore not a loss for most of those students. It is a large loss for a minority of them, and that minority shrinks as ability rises. Read together, the counterfactual separates two things that are easily conflated. Vouchers expand access to the professional track precisely for the vulnerable students least equipped to finish it, so the schooling they induce is worth little to them: the modal consequence is an incomplete professional spell, a worse labor-market outcome than the high-school diploma they began with. Their pay still rises—a majority gain in every ability tercile—but it rises for reasons that have little to do with the schooling change, and in dollars the gain is estimated too imprecisely to separate from zero outside the top of the ability distribution. College, in this setting, is not a panacea; whether it pays depends on who is induced to attempt it and on whether they finish.

Table 15: The Voucher Counterfactual by Ability

ability	Δ prof. grad. (pp)	Δ prof. dropout (pp)	Δ earnings (USD/yr)	share gaining (percent)	P(grad|prof) vch / pub
all	0.22	1.47***	102.7	63.5	0.280 / 0.293
	(0.25)	(0.32)	(77.0)	(5.8)	
low	0.49***	1.82***	-48.8	56.6	0.227 / 0.232
	(0.14)	(0.26)	(84.4)	(7.5)	
middle	0.41*	1.82***	76.4	62.7	0.258 / 0.266
	(0.24)	(0.33)	(80.2)	(5.4)	
high	-0.25	0.77	280.4**	71.2	0.307 / 0.319
	(0.45)	(0.59)	(117.9)	(8.3)	

Notes: Effect of moving vulnerable public-high-school students to the voucher branch (the counterfactual of Table 14), by ability tercile. Δprof. grad. and Δprof. dropout are the changes (in percentage points) in the shares reaching professional graduation and professional dropout; Δearnings is the change in mean annual earnings (USD); *share gaining* is the percentage of students whose expected annual earnings are higher under the voucher branch, an object that does not depend on the dependence between outcomes across branches; the last column reports the completion rate among professional enrollees under the voucher and public branches. Standard errors in parentheses are from a parametric bootstrap (500 draws). *, **, ***: significant at 10%, 5%, 1%.

8 Conclusions

We ask how private-voucher high schools shape the transition from school to work in Chile. To that end we use administrative data that follow into higher education and the labor market a cohort of students required to choose a new high school because their public primary school offered no secondary grades, and we answer the question twice: with value-added regressions conditioning on the four tests they sat in eighth grade, and with a sequential model of schooling decisions and outcomes that conditions on the unobserved ability those same tests recover. On wages, the two approaches agree wherever they overlap. Our answer is that vouchers do improve these students' labor-market outcomes, but not through the schooling they induce, which if anything works against them.

Three findings support this conclusion. First, what higher education pays depends on the type of program as much as on completing it. A professional degree raises wages by 0.60 log points relative to a high-school diploma and a technical degree by 0.37, but an incomplete professional spell lowers wages by 0.18 log points and formal employment by 9 percentage points, while an incomplete technical spell leaves wages statistically unchanged and formal employment 2.5 p.p. higher. Because so few students finish, the expected return to enrolling in a professional program—integrating over the student's own graduation decision—is statistically indistinguishable from zero, against 0.141 log points for a technical program, and the professional option is also the riskier one, both because completion is less likely and because professional dropouts face a far more dispersed wage distribution. Second, the voucher moves students toward exactly that option: it raises higher-education enrollment by 1.83 percentage points, entirely on the professional track, and the induced enrollment arrives as dropout rather than as degrees—professional dropout rises by 1.79 percentage points, the flow our bootstrap estimates most precisely, while professional graduation moves by an insignificant 0.17—so that the additional enrollment leaves degree attainment no higher. Third, decomposing the voucher's effect on labor-market outcomes, essentially all of it comes from higher pay within schooling states, while the reallocation across schooling states that the voucher causes adds nothing and, if anything, subtracts.

These results revise the policy reading that a focus on access alone would suggest, though not into a case against vouchers. Lower-ability students do gain from attending one: their wages rise by 0.037 log points, and a majority of them are better off. What those gains do not come from is the schooling the voucher induces. The enrollment it buys them lands in the track with the lowest completion-integrated return, their professional completion rates are the lowest of any group, and their inflow into professional dropout is as large as any group's. Access and attainment therefore come apart: expanding access raises pay without raising degrees, so a policy judged on enrollment counts will appear to work for a reason that is not the reason it works. If the objective is the education-to-work transition rather than enrollment counts, the binding margins are completion and track composition—the information students have about their prospects in each track, and the support they receive once enrolled—rather than the number of students who start.

Our analysis has limits worth naming. Our factor structure is what supplies the dependence between potential outcomes across counterfactual states, and the estimated loadings in the wage equations are small relative to the residual dispersion, so we report option-specific distributions and comparisons of their quantiles rather than the spread of individual treatment effects. Effects expressed in dollars are far less precisely estimated than those in logs, because exponentiating amplifies parameter uncertainty. And we observe a single cohort, in a system whose regulation of the voucher sector has since changed—and one part of that cohort: the students required to choose a new high school at the end of eighth grade, who are poorer and lower-scoring than the Chilean average. They are the students a voucher system is meant to serve, but the returns, the ranking of the two tracks, and the voucher effects we report are theirs.

Three questions seem to us the natural next ones. Whether students understand the completion risk they take on when they choose a professional program is an open empirical question, and one with clear policy content: if the professional track is chosen partly on optimistic beliefs, information may do more than subsidies. Whether the within-state wage advantage of voucher schools reflects instruction, networks, or unmeasured skills matters for whether it can be reproduced elsewhere. And whether the pattern we document—access expanding along the track least likely to be completed—generalizes to other settings in which school choice is expanded without a corresponding change in the higher-education options available is, we suspect, the question on which the external validity of these results turns.

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A Data

The following are the various administrative and survey data sets for Chile that we use in our analysis.

- *National standardized exams (SIMCE) for 8th graders, student-level, 2000.*
  These data provide information on students' test scores for four different subjects: verbal, mathematics, social sciences, and natural sciences. They also provide information on students' gender and grade.
- *8th grade SIMCE's questionnaire to parents and tutors, 2000.*
  These data consist in the responses to a survey that parents and tutors answer during the days when the national standardized tests are taken. The survey is voluntary, though about more than 90% of parents respond it. It provides information on students' household size, house amenities, and time use, total number of books available in the household, household total monthly income, parents and tutors' time use, education, indigenous identification, occupation, health insurance, participation in social programs, reasons for the choice of the school, beliefs on the student's future educational attainment, satisfaction with the school, knowledge of school's average performance in standardized tests, total monthly expenses related to the student's education other than tuition, and school's admission criteria, tuition, and fees.
- *National standardized exams (SIMCE), school-level, 8th grade in 2000, 10th grade in 1998.*
  These data provide information on schools' average test scores for verbal, mathematics, and social and natural sciences, municipality, management type (public, private-voucher, private-non-voucher), socio-economic category of the population served by the school, urban status, and number of students taking the tests.
- *Registry of students, 2004-2008.*
  These data provide information on students' gender, date of birth, age, municipality of residence, type and level of education, grade, class, grade repetition status, special education status, and various characteristics of the school of attendance, such as municipality, management type, single/double shift schedule, and urban status.
- *Registry of schools, 2000, 2004-2009.*
  These data provide information on schools' municipality, management type, urban status, address, and type and level of education offered.
- *National standardized college admission exams (PSU), student-level, 2005-2010.*
  These data provide information on students' college admission test scores for four different subjects: verbal, mathematics, social sciences, and natural sciences. These exams are not

mandatory, but are essential in determining students' chances of attending higher education. The data also provide information on students' birth date, home address, level of education of the parents, occupation of the parents, high school attended, high school GPA, preference ranking of higher education institutions-majors, among others.

- *National socio-economic characterization household survey (CASEN), 2003, 2006, 2009.* The CASEN series of household surveys corresponds to the most important piece of socio-economic information in Chile. It is used to inform public polices, and contains extensive information on individuals' education, health, employment, etc. It is nationally representative at the municipality level.

- *Registry of students in higher education institutions, 2007-2016.* These data provide detailed information on students' enrollment in higher education institutions, including the institution, major, geographical location where the classes are taken, as well as students' age, gender, and birth date.

- *Registry of individuals completing a higher education degree, 2007-2016.* These data provide information on the students completing a higher education degree, by year. It also contains detailed information on the degree (major and higher education institution), the time needed to complete the degree, the official duration of the degree, the exact date when the degree was completed, as well as students' age, gender, and birth date.

- *Unemployment Insurance (Seguro de Cesantía) administrative records, 2013.* These data come from Chile's unemployment insurance system, administered by the *Administradora de Fondos de Cesantía* (AFC), which registers the monthly earnings and the employing firm of every salaried worker holding a formal private-sector contract. We use these records to measure each individual's formal employment status and labor earnings in 2013, when the typical student in our sample is about 27 years old—roughly nine years after high-school graduation. Individuals who never hold a formal-sector job are retained and treated as not employed, which defines the extensive (employment) margin of our labor-market outcomes.

B Additional Tables

This appendix collects the supporting tables. Table B.1 sets out the exact inclusion of the covariates and instruments in each equation of the model. Tables B.2–B.5 present the maximum-likelihood estimates discussed in Section 7, and Tables B.6 and B.7 compare the simulated model with the data. Tables B.8 and B.9 report the returns to education by mother's education and conditional on the high school attended.

Table B.1: Variables Used in the Empirical Analysis

		schooling choices				labor market	
	measurement system	high-school type (D_1)	college enrollment (D_2–D_3)	professional vs. technical (D_4–D_5)	college graduation (D_6–D_9)	earnings	employment
covariates							
male	Y	Y	Y	Y	Y	Y	Y
mother's education: 12 years	Y	Y	Y	Y	Y		
mother's education: more than 12 years	Y	Y	Y	Y	Y		
mother's education: missing	Y	Y	Y	Y	Y		
father's education: 12 years	Y	Y	Y	Y	Y		
father's education: more than 12 years	Y	Y	Y	Y	Y		
father's education: missing	Y	Y	Y	Y	Y		
broken home	Y	Y	Y	Y	Y		
broken home: missing	Y	Y	Y	Y	Y		
household income: CLP 100–200K	Y	Y	Y	Y	Y		
household income: more than CLP 200K	Y	Y	Y	Y	Y		
household income: missing	Y	Y	Y	Y	Y		
region: north (age 14)	Y	Y					
region: center (age 14)	Y	Y					
region: north (high school)			Y	Y	Y	Y	Y
region: center (high school)			Y	Y	Y	Y	Y
exclusion restrictions							
% voucher schools in municipality		Y					
local unemployment gap (high–low skill, 2003)			Y				
local wage gap (high–low skill, 2003)			Y				
college in municipality			Y				
professional college in municipality				Y			
local unemployment gap (high–low skill, 2009)					Y		
local wage gap (high–low skill, 2009)					Y		
local unemployment (2013)							Y
local wage (2013)							Y
unobserved heterogeneity							
factor	Y	Y	Y	Y	Y	Y	Y

Notes: This table shows the allocation of the covariates and exclusion restrictions across the equations of our model. The column headers are the measurement system (the four 8th-grade tests), the four schooling-choice nodes of Figure 1 (high-school type D_1; college enrollment D_2–D_3; the professional-vs.-technical choice D_4–D_5; and college graduation D_6–D_9), and the two labor-market outcomes (earnings and employment). A "Y" means that the variable enters the corresponding equation. Education, broken home, household income, and region enter as categorical indicators; the omitted categories are education below 12 years, non-broken home, household income below CLP 100K per month, and the southern region, and a missing-data category is included for each. Variables measured at age 14 (the primary-school stage) enter the measurement system and the high-school choice D_1; their high-school-stage analogues enter the remaining equations. The exclusion restrictions are the local availability of private-voucher schools (D_1); the local high–low-skill unemployment and wage gaps and the local availability of a college (D_2–D_3); the local availability of a professional college (D_4–D_5); and the local labor-market gaps measured later in life (D_6–D_9 and employment). The factor is the unobserved (ability) heterogeneity.

Table B.2: Estimates - Measurement System

subject:	verbal		math		social sciences		natural sciences	
	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
male	-0.216	0.006	0.064	0.006	0.164	0.006	0.007	0.006
mother's education: 12 years	0.194	0.008	0.178	0.008	0.197	0.008	0.161	0.008
mother's education: more than 12 years	0.298	0.013	0.266	0.013	0.307	0.013	0.285	0.013
mother's education: missing	-0.163	0.016	-0.180	0.016	-0.152	0.016	-0.161	0.016
father's education: 12 years	0.120	0.008	0.113	0.008	0.138	0.008	0.112	0.008
father's education: more than 12 years	0.256	0.012	0.254	0.012	0.274	0.012	0.238	0.012
father's education: missing	-0.140	0.014	-0.161	0.014	-0.147	0.014	-0.123	0.015
broken home	0.019	0.007	0.010	0.007	0.021	0.007	0.014	0.007
broken home: missing	0.268	0.017	0.296	0.017	0.253	0.018	0.283	0.018
household income: CLP 100–200K	0.088	0.007	0.072	0.008	0.088	0.008	0.056	0.008
household income: more than CLP 200K	0.133	0.009	0.130	0.009	0.136	0.009	0.103	0.009
household income: missing	0.015	0.017	0.010	0.016	0.023	0.017	-0.008	0.017
region: north (age 14)	-0.061	0.009	-0.068	0.008	-0.067	0.009	-0.082	0.009
region: center (age 14)	-0.077	0.006	-0.081	0.006	-0.067	0.006	-0.090	0.006
constant	-0.142	0.008	-0.266	0.008	-0.352	0.008	-0.219	0.008
factor	1.000	-	0.931	0.004	0.946	0.004	0.980	0.004
$\log(\sigma)$	-0.677	0.003	-0.552	0.003	-0.566	0.002	-0.586	0.003
observations	94,453		94,453		94,453		94,453	

This table reports the estimated coefficients of the maximum-likelihood measurement system: the four 8th-grade SIMCE tests (verbal, math, social sciences, natural sciences), all measured at age 14. The factor loading for verbal is normalized to one. The four test scores are normalized to mean zero and standard deviation one in the population of test takers; $\log(\sigma)$ is the natural logarithm of the standard deviation of the corresponding test's distribution. Estimates obtained by maximum likelihood (first estimation step). Omitted categories: mother's/father's education <12 years, non-broken home, household income below CLP 100K per month, and the southern region. Household income brackets are monthly figures. The factor is the unobserved (ability) heterogeneity; its loading is reported in the *factor* row.

Table B.3: Estimates - Schooling Decisions D_1, D_2, D_3

conditional on:			$D_1 = 1$		$D_1 = 0$	
decision node:	D_1: voucher school		D_2: enroll college		D_3: enroll college	
	coef.	std. err.	coef.	std. err.	coef.	std. err.
male	0.108	0.009	-0.093	0.015	-0.168	0.011
mother's education: 12 years	0.074	0.012	0.448	0.021	0.470	0.017
mother's education: more than 12 years	0.121	0.021	0.668	0.040	0.715	0.032
mother's education: missing	0.025	0.024	-0.380	0.042	-0.500	0.031
father's education: 12 years	0.001	0.012	0.346	0.021	0.366	0.016
father's education: more than 12 years	0.023	0.019	0.588	0.036	0.594	0.028
father's education: missing	0.063	0.021	-0.292	0.037	-0.314	0.028
broken home	-0.016	0.011	-0.082	0.019	-0.079	0.015
broken home: missing	-0.129	0.028	0.393	0.050	0.463	0.037
household income: CLP 100–200K	0.117	0.012	0.235	0.020	0.284	0.015
household income: more than CLP 200K	0.205	0.015	0.634	0.026	0.575	0.020
household income: missing	0.069	0.027	0.273	0.048	0.302	0.035
region: north (age 14)	-0.457	0.014	-	-	-	-
region: center (age 14)	0.162	0.010	-	-	-	-
region: north (high school)	-	-	0.019	0.031	-0.134	0.016
region: center (high school)	-	-	0.015	0.018	-0.177	0.013
% voucher schools in municipality	0.893	0.018	-	-	-	-
local unemployment gap (high–low skill)	-	-	-0.438	0.162	-0.059	0.108
local wage gap (high–low skill)	-	-	-0.005	0.003	0.002	0.002
college in municipality	-	-	0.066	0.016	0.082	0.012
constant	-1.047	0.014	-0.441	0.027	-0.383	0.019
factor	0.037	0.007	0.563	0.012	0.609	0.009
observations	94,453		32,871		61,582	

This table reports the estimated coefficients for schooling decision nodes D_1, D_2, and D_3 in Figure 1. D_1 is the choice of a private-voucher (vs. public) high school; D_2 and D_3 are the decision to enroll in college, conditional on attending a private-voucher ($D_1 = 1$) or public ($D_1 = 0$) high school. Covariates for D_1 are measured at age 14; covariates for D_2 and D_3 at the high-school stage. Estimates obtained by maximum likelihood (first estimation step). Omitted categories: mother's/father's education <12 years, non-broken home, household income below CLP 100K per month, and the southern region. Household income brackets are monthly figures. The factor is the unobserved (ability) heterogeneity; its loading is reported in the *factor* row.

Table B.4: Estimates - Schooling Decisions D_4, D_5

conditional on:	$D_2 = 1$		$D_3 = 1$	
decision node:	D_4: prof. degree		D_5: prof. degree	
	coef.	std. err.	coef.	std. err.
male	-0.166	0.021	-0.052	0.016
mother's education: 12 years	0.227	0.026	0.260	0.021
mother's education: more than 12 years	0.341	0.042	0.465	0.034
mother's education: missing	-0.234	0.065	-0.216	0.052
father's education: 12 years	0.168	0.026	0.199	0.020
father's education: more than 12 years	0.422	0.040	0.396	0.031
father's education: missing	-0.154	0.060	-0.284	0.048
broken home	-0.071	0.027	-0.067	0.021
broken home: missing	0.277	0.071	0.313	0.055
household income: CLP 100–200K	0.089	0.030	0.141	0.022
household income: more than CLP 200K	0.322	0.034	0.344	0.026
household income: missing	0.096	0.071	0.086	0.055
region: north (high school)	0.087	0.040	0.084	0.022
region: center (high school)	-0.075	0.025	-0.084	0.018
professional college in municipality	0.013	0.022	0.055	0.016
constant	-0.130	0.034	-0.336	0.023
factor	0.451	0.016	0.562	0.013
observations	16,519		28,353	

This table reports the estimated coefficients for schooling decision nodes D_4 and D_5 in Figure 1: the choice of a professional (vs. technical) college degree, conditional on having enrolled in college from a private-voucher (D_4) or public (D_5) high school. The dependent variable equals one for a professional degree and zero for a technical degree. Estimates obtained by maximum likelihood (first estimation step). Omitted categories: mother's/father's education <12 years, non-broken home, household income below CLP 100K per month, and the southern region. Household income brackets are monthly figures. The factor is the unobserved (ability) heterogeneity; its loading is reported in the *factor* row.

Table B.5: Estimates - Schooling Decisions D_6, D_7, D_8, D_9

conditional on:	$D_4 = 1$		$D_4 = 0$		$D_5 = 1$		$D_5 = 0$	
decision node:	D_6: degree		D_7: degree		D_8: degree		D_9: degree	
	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
male	-0.336	0.028	-0.327	0.032	-0.344	0.022	-0.378	0.023
mother's education: 12 years	0.074	0.035	0.094	0.041	0.069	0.027	0.111	0.031
mother's education: more than 12 years	0.057	0.049	0.182	0.071	0.067	0.038	0.174	0.056
mother's education: missing	-0.201	0.127	-0.291	0.101	-0.059	0.103	-0.160	0.071
father's education: 12 years	-0.008	0.036	0.070	0.041	0.034	0.027	0.049	0.030
father's education: more than 12 years	0.013	0.047	-0.081	0.069	0.049	0.037	-0.025	0.051
father's education: missing	-0.260	0.120	-0.203	0.093	-0.428	0.098	-0.231	0.064
broken home	-0.183	0.038	-0.086	0.041	-0.134	0.030	-0.107	0.030
broken home: missing	0.131	0.105	0.285	0.104	0.127	0.082	0.118	0.079
household income: CLP 100–200K	0.072	0.046	0.050	0.045	0.046	0.033	0.052	0.031
household income: more than CLP 200K	0.175	0.049	0.188	0.052	0.066	0.036	0.059	0.039
household income: missing	0.144	0.107	0.159	0.103	0.068	0.083	0.202	0.077
region: north (high school)	-0.189	0.050	-0.208	0.065	-0.107	0.029	-0.221	0.034
region: center (high school)	-0.226	0.032	-0.107	0.037	-0.152	0.025	-0.125	0.026
local unemployment gap (high–low skill)	0.059	0.321	0.230	0.352	0.110	0.202	0.235	0.210
local wage gap (high–low skill)	0.004	0.006	-0.008	0.006	-0.005	0.004	-0.019	0.005
constant	-0.460	0.055	-0.287	0.054	-0.417	0.038	-0.084	0.036
factor	0.193	0.022	0.308	0.026	0.201	0.017	0.233	0.019
observations	9,377		7,142		15,479		12,874	

This table reports the estimated coefficients for schooling decision nodes D_6–D_9 in Figure 1: graduating from college (earning a degree), conditional on the high-school type and degree-type path. D_6/D_7 are for private-voucher students pursuing a professional/technical degree ($D_4 = 1/0$); D_8/D_9 are for public students pursuing a professional/technical degree ($D_5 = 1/0$). Estimates obtained by maximum likelihood (first estimation step). Omitted categories: mother's/father's education <12 years, non-broken home, household income below CLP 100K per month, and the southern region. Household income brackets are monthly figures. The factor is the unobserved (ability) heterogeneity; its loading is reported in the *factor* row.

Table B.6: Goodness of Fit - Measurement System

	mean		std. dev.	
	actual	model	actual	model
verbal	-0.105	-0.106	0.912	0.908
math	-0.108	-0.108	0.907	0.902
social sciences	-0.112	-0.111	0.918	0.912
natural sciences	-0.112	-0.112	0.920	0.916

Notes: This table compares our simulated model with the actual data. Simulations consist in 500,000 draws taken from the model and its estimates.

Table B.7: Goodness of Fit - Schooling Decisions

node	decision	actual	model
D_1	private-voucher high school	0.348	0.348
D_2	enroll in college | voucher	0.503	0.504
D_3	enroll in college | public	0.460	0.463
D_4	professional degree | voucher, enrolled	0.568	0.567
D_5	professional degree | public, enrolled	0.546	0.542
D_6	college degree | voucher, professional	0.277	0.272
D_7	college degree | voucher, technical	0.321	0.322
D_8	college degree | public, professional	0.299	0.294
D_9	college degree | public, technical	0.355	0.352

Notes: This table compares the actual data with our model's predictions for the schooling-decision nodes of Figure 1. The model column is based on 500,000 simulated observations from the model and its estimates. Each entry is the mean of the decision indicator over the indicated conditioning subsample.

Table B.8: Returns to Education by Mother's Education

	all	vulnerable	educated
	(1)	(2)	(3)
A. Employment			
professional graduate	-0.031***	-0.030***	-0.031***
	(0.007)	(0.007)	(0.007)
technical graduate	-0.030***	-0.031***	-0.031***
	(0.005)	(0.005)	(0.005)
professional dropout	-0.089***	-0.089***	-0.087***
	(0.004)	(0.004)	(0.004)
technical dropout	0.025***	0.026***	0.024***
	(0.004)	(0.004)	(0.004)
B. Log wages			
professional graduate	0.602***	0.609***	0.596***
	(0.026)	(0.026)	(0.026)
technical graduate	0.367***	0.367***	0.373***
	(0.020)	(0.020)	(0.020)
professional dropout	-0.181***	-0.179***	-0.181***
	(0.018)	(0.018)	(0.018)
technical dropout	0.019	0.017	0.026*
	(0.014)	(0.014)	(0.014)

Notes: Estimated average treatment effects (ATE) of each final schooling level relative to a high school diploma, on employment (Panel A) and log annual wages (Panel B). Column (2) restricts to vulnerable students (mother's education below 12 years) and column (3) to non-vulnerable students (12 years or more). Point estimates are from the model; standard errors in parentheses are from a parametric bootstrap (500 draws of the parameters from their estimated asymptotic distribution). *, **, ***: significant at 10%, 5%, 1%.

Table B.9: Returns to Education by High-School Type (Conditional on Attendance)

	all (1)	voucher HS (2)	public HS (3)
A. Employment			
professional graduate	-0.031***	-0.022**	-0.036***
	(0.007)	(0.010)	(0.008)
technical graduate	-0.030***	-0.025***	-0.033***
	(0.005)	(0.009)	(0.007)
professional dropout	-0.089***	-0.075***	-0.097***
	(0.004)	(0.006)	(0.006)
technical dropout	0.025***	0.033***	0.021***
	(0.004)	(0.006)	(0.005)
B. Log wages			
professional graduate	0.602***	0.555***	0.627***
	(0.026)	(0.039)	(0.033)
technical graduate	0.367***	0.378***	0.361***
	(0.020)	(0.034)	(0.024)
professional dropout	-0.181***	-0.181***	-0.181***
	(0.018)	(0.028)	(0.024)
technical dropout	0.019	0.038	0.009
	(0.014)	(0.024)	(0.019)

Notes: Estimated average treatment effects (ATE) of each final schooling level relative to a high school diploma, on employment (Panel A) and log annual wages (Panel B). Unlike Table 8, columns (2) and (3) condition on the high school the student actually attended, and are therefore treatment-on-the-treated parameters, averaging the gain over the students who made each high-school choice: $\int\int E(Y_j - Y_0 \mid X = x, \theta = \bar{\theta}, D_1 = 1)\, dF_{X,\theta|D_1=1}(x, \bar{\theta})$ in column (2), among students who attended a voucher high school, and the analogous average over students who attended a public high school in column (3). Point estimates are from the model; standard errors in parentheses are from a parametric bootstrap (500 draws of the parameters from their estimated asymptotic distribution). *, **, ***: significant at 10%, 5%, 1%.