Teacher Preparation, Pre-College Human Capital and Student Learning: Evidence from *Enseña Chile*

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Abstract

We estimate teacher value-added from Chilean baseline and follow-up math tests and link it to teachers' pre-college admission scores, classroom experience, and preparation route. The linkage compares *Enseña Chile*, a Teach For America-style alternative pathway and Chilean Teach For All partner, with traditional teacher-college preparation. A one-SD (σ) higher college exam score predicts 0.51σ higher value-added. *Enseña Chile* teachers start with $+0.25\sigma$ from human capital, but average -0.16σ in value-added, driven by inexperience. Teacher-college and *Enseña Chile* training are statistically indistinguishable. Attracting and retaining high-ability recruits is important, but they must stay long enough to gain experience.

Keywords: teacher value-added, alternative certification, teacher selection, Chile, Teach For All
JEL: I20, I21, I28, J45

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1. Introduction

High-quality teachers are among the most important school-based inputs for student achievement and long-run outcomes [14, 18]. Yet beyond well-established returns to experience, observable teacher traits correlate weakly and inconsistently with effectiveness, fueling persistent debate over how to prepare and select teachers. In parallel, many school systems face acute staffing shortages in specific subjects and geographies. Alternative entry pathways, most prominently Teach For America (TFA) and the Teach For All network of country affiliates, aim to attract high-ability graduates to teach in hard-to-staff schools through short, intensive pre-service training followed by structured in-service coaching.

A central question in this literature is *why* TFA-style programs achieve roughly comparable test-score results to traditional preparation, despite radically shorter coursework. Two mechanisms are typically invoked: *selection* on observable cognitive ability and motivation, and *intensive coaching* during the two-year commitment. Distinguishing the two has been difficult because almost no study has both individual-level pre-college ability scores for the relevant teachers *and* student-level panel data sufficient to estimate value-added (VA). Consequently, the literature documents the average result but leaves its composition implicit.

This paper provides such a decomposition. We study *Enseña Chile* (eCh), the Chilean Teach For All affiliate. We link administrative records on teachers' college entrance exam scores (PSU/PAA) and years of experience to research-administered beginning- and end-of-year SEPA[1] mathematics assessments for grades 9–11 across *Enseña Chile* partner schools (those that host eCh teachers) and non-partner comparison schools (which do not). The resulting dataset—114 mathematics teachers (37 eCh, 77 traditional), 169 classes, 3,756 students, 59 schools—lets us measure each teacher's VA and decompose the *Enseña Chile*–traditional gap into pre-college ability, experience, and a route residual that absorbs training, motivation, unobserved selection, and any ability-by-experience interaction.

Three findings emerge. First, raw VA for *Enseña Chile* teachers is 0.32σ below that of traditional teachers (means -0.19 vs. $+0.13$); reweighting the comparison sample to the national common-support PSU/PAA distribution narrows this to -0.16σ. The gap reflects the *Enseña Chile* cohort's inexperience: all 37 *Enseña Chile* teachers in the sample are in their first year (20) or second year (17). Second, *Enseña Chile* teachers are positively selected on ability: a one-SD increase in PSU/PAA is associated with a 0.51σ increase in VA (highly significant), and *Enseña Chile* teachers' PSU/PAA scores exceed those of traditional teachers in partner schools by roughly 0.6 SD in math and 1.0 SD in language. Conditioning on this ability advantage alone *widens* the raw gap rather than closing it. To separate the remaining gap into experience and route-of-preparation, we draw on administrative experience records showing that about half the comparison teachers are themselves early-career, which provides genuine overlap with *Enseña Chile* in the experience distribution. The route residual is statistically indistinguishable from zero, though

[1] *Sistema de Evaluación de Progreso del Aprendizaje*; details in Section 2.

imprecise: second-year *Enseña Chile* teachers reach parity with comparably early-career traditional peers. Third, within-*Enseña Chile* learning is rapid: second-year *Enseña Chile* teachers ($N = 17$) display VA 0.60σ above first-year *Enseña Chile* teachers ($N = 20$), consistent with steep early-career returns and the program's structured coaching model.

We explicitly distinguish between two comparisons. The *staffing* comparison (without ability and experience controls) is the one school leaders face when filling hard-to-staff vacancies; under it, *Enseña Chile* teachers reach rough parity with the traditional teachers already in their schools. The *conditional* comparison (adding PSU/PAA and experience) is the regression-estimated object and isolates the route-of-preparation residual, which is statistically indistinguishable from zero, though imprecise. Because we never observe the marginal teacher a vacancy would otherwise draw, any staffing recommendation is an interpretation rather than an estimate, a distinction we return to in Section 4. Subject to that caveat, *Enseña Chile*'s strong positive selection makes it a plausible lever for raising instructional quality where able candidates are scarce.

This paper makes three contributions. The first establishes the evidence base: a purpose-designed linked dataset built to identify the PSU/PAA–VA gradient. The remaining two use that evidence to speak to teacher selection and preparation.

First, we build a linked dataset for a setting where teacher VA measures are scarce. Estimates remain rare in middle-income countries because the longitudinal, classroom-level testing they require is seldom available. We administered baseline and end-of-year SEPA mathematics assessments across

169 grade 9–11 classrooms and linked the resulting VA estimates to administrative PSU/PAA entrance-exam scores and the MINEDUC *Docentes* experience panel. Because *Enseña Chile* recruits are drawn from selective universities and concentrate at the top of the PSU/PAA distribution, a representative traditional comparison would conflate route with ability; we instead stratified non-partner schools by their teachers' PSU/PAA decile, inviting at least one school per decile and trading representativeness for the within-comparison ability overlap needed to estimate the PSU/PAA–VA gradient. To our knowledge, this is among the first teacher VA datasets for Chile and one of the few for Latin America.

Second, we estimate how teachers' pre-college academic ability predicts VA. To our knowledge, this is the first individual-teacher-level VA estimate using a country's selective-university entrance exam, taken before teacher preparation, as the focal cognitive measure. Prior work instead uses post-training subject knowledge [15, 23, 3], licensure tests [11], pre-hire screening batteries [26, 19], or country-level PIAAC scores [13]. The closest pre-college precedent is Boyd et al. [4], where SAT scores and college selectivity enter as controls among many rather than as the focal measure. Our companion study links pre-college achievement to broader Chilean teacher outcomes [24]; here we isolate the VA margin and the alternative-pathway comparison.

Third, we decompose selection and experience for a Teach For All affiliate at the individual-teacher level. Prior TFA evidence, mostly from the United States, asks whether alternative-pathway teachers are more or less effective than traditional teachers and generally finds parity or modest math gains [12, 27, 7, 10, 9, 2]. Evidence from England and Peru points in a similar direction for Teach For All affiliates outside the United States [1, 21, 8]. What remains unclear is whether these average effects reflect who enters the pathway, what they learn on the job, or the route itself. By linking PSU/PAA scores, experience, and SEPA-based VA, we can separate these channels directly.

The remainder of the paper proceeds as follows. Section 2 describes the data, sample, VA construction, and decomposition approach. Section 3 presents the main results. Section 4 concludes by discussing interpretation, policy counterfactuals, and limitations.

2. Data and Methods

Estimating teacher value-added in a middle-income setting requires linked, classroom-level information that is rarely collected together. We administered baseline and end-of-year mathematics assessments, fielded student perception surveys, and linked the results to administrative records on teachers' pre-college ability and experience; crucially, we designed rather than convenience-sampled the comparison group, stratifying schools by teachers' PSU/PAA scores so the data would span the ability distribution. To our knowledge no existing Chilean or Latin American source combines research-administered achievement gains, perception surveys, and administrative ability and experience records for the same teachers.

The analytic dataset combines a research-administered student testing campaign with two administrative sources. We administered the SEPA mathematics assessment at the beginning (May) and end (November) of the 2016 school year in grades 9–11 through external proctors. The SEPA instrument was developed by MIDE-UC, the measurement center at the Pontificia Universidad Católica de Chile; the baseline test covers prior-grade content and the follow-up covers current-grade content. Alongside the November tests, we administered a Teach For All student perception survey aligned with the Measures of Effective Teaching (MET) study domains (rigorous expectations, engagement, supportive relationships). The teacher records add administrative PSU/PAA math and language scores (standardized at the national scale, mean 500, SD 110) and years of experience from the MINEDUC *Docentes* panel.

The working sample comprises 3,756 students in 169 classes taught by 114 mathematics teachers across 59 schools. It includes 37 *Enseña Chile* teachers in eCh partner schools, 31 traditional teachers in those same partner schools, and 46 traditional teachers in 24 non-partner schools from the Metropolitan Region. Of the 53 eligible eCh partner schools, 38 (72%) participated; 35 contribute teachers to the analytic VA sample. Table 1 reports descriptive statistics by teacher group, including teacher characteristics, school covariates, SEPA scores, and observation counts.

The comparison sample is purposive. Panel A of Table 1 shows that *Enseña Chile* teachers have substantially higher PSU/PAA scores than traditional teachers in partner schools, while the traditional comparison teachers provide overlap across the lower and upper parts of the PSU/PAA distribution; Panel C reports baseline and follow-up SEPA scores by group. Because *Enseña Chile* teachers are recruited from selective universities and concentrate at the high end of the PSU/PAA distribution, we stratified non-partner schools by their teachers' PSU/PAA decile and invited at least one school per decile. Participating non-partner schools cover deciles 1–10, placing high- and low-

PSU/PAA traditional teachers alongside the *Enseña Chile* group (see Online Appendix A for details). The design therefore trades representativeness for the within-comparison ability overlap needed to estimate the gradient. Accordingly, the unweighted group means and gaps are analytic-sample comparisons rather than population mean VA estimates for Chilean teachers.

Table 1: Summary statistics by teacher group.

	***Enseña Chile* Teachers**	**Traditional (eCh schools)**	**Traditional (non-eCh)**	**Total**
Panel A: Teacher Characteristics				
Experience (years)	0.5	8.0	14.0	8.6
	(0.5)	(9.6)	(12.1)	(11.2)
Language PSU/PAA	640	526	565	584
	(58)	(73)	(74)	(81)
Math PSU/PAA	680	616	629	644
	(84)	(79)	(75)	(83)
Panel B: School Covariates				
Metropolitan Region	0.31	0.41	1.00	0.67
Public School	0.28	0.29	0.12	0.20
Low SES School	0.49	0.32	0.18	0.30
Mid-Low SES School	0.46	0.57	0.32	0.41
Mid SES School	0.05	0.05	0.19	0.12
Mid-High SES School	0.00	0.05	0.31	0.16
High SES School	0.00	0.00	0.00	0.00
Class Size	21.2	23.3	29.0	25.5
	(7.2)	(6.5)	(7.1)	(7.9)
Panel C: SEPA Scores				
Baseline	627	639	650	641
	(35)	(37)	(41)	(40)
Follow-up	634	643	655	647
	(31)	(35)	(40)	(37)
Gain	7.2	4.6	5.6	5.9
	(28.4)	(29.7)	(27.4)	(28.2)
Panel D: Observations				
N Students	1,155	754	1,847	3,756
N Classes	64	36	69	169
N Teachers	37	31	46	114
N Schools	30	21	24	59

Notes: Student-level means; standard deviations in parentheses. SES tiers are from the SIMCE socioeconomic classification. Teacher experience is years of service recovered from the MINEDUC *Docentes* panel; about half the comparison teachers are early-career (≤ 5 years).

The empirical strategy has three steps. First, following Chetty et al. [6], we construct teacher VA from the 2016 SEPA gain design. We residualize follow-up scores using baseline achievement and student covariates, estimated from within-teacher variation; aggregate residuals to the class level and adjust for class-mean baseline residuals; and standardize predicted residual learning within the analytic sample.

Second, we assign each student the VA estimate of their mathematics teacher and estimate pooled regressions of the form

$$\widehat{\mathrm{VA}}_{ij} = \alpha + R_j'\theta + \beta \mathrm{PSU}_j + \phi_1 \mathrm{Exp}_j + \phi_2 \mathrm{Exp}_j^2 + X_{ij}'\gamma + \varepsilon_{ij},$$

where i indexes students, j indexes teachers, R_j indicates route/school group, and X_{ij} includes SES tier, grade fixed effects, and class size. Baseline SEPA enters through the constructed VA measure rather than as a separate second-stage control. Model 1 omits PSU/PAA and experience, corresponding to the staffing comparison; Model 2 adds them, corresponding to the conditional comparison; column 3 adds the leave-out school faculty-composition control. We do not estimate school fixed effects because the within-school cells are too thin, with about 1.8 sampled teachers per school. Standard errors are clustered at the classroom level.

Third, we use the Model 2 estimates and corrected experience records to decompose the reweighted *Enseña Chile*–traditional gap into selection on PSU/PAA, early-career experience, and a route residual. These records require correction: experience identifies the early-career overlap underlying the experience and route components, yet an earlier administrative file mis-measured it for many comparison teachers. We detail the correction below. For this exercise, the *Enseña Chile* cohort mean remains unweighted and the sampled comparison teachers are reweighted to the relevant national teacher PSU/PAA distribution over common-support deciles; Online Appendix Section B.8 describes the reweighting procedure in more detail. Online Appendix B also reports robustness checks that restrict the sample to partner schools, exclude first-year *Enseña Chile* teachers, use alternative SEPA standardizations, and vary the clustering level.

Given the purposive design, this empirical exercise is a within-sample VA comparison rather than a population estimate. We estimate VA as cross-sectional teacher fixed effects from a single school year, so we do not exploit within-teacher variation across cohorts and cannot rule out classroom-level unobservables. Three features mitigate this concern. First, the student-level controls include each student's baseline SEPA score, absorbing prior achievement differences within teacher. Second, although the design is too thin in within-school cells to support school fixed effects, we control directly for the academic composition of each school's teaching staff, holding constant the most relevant dimension of school-level sorting. Third, because the sample was built to span the teacher ability distribution, interpretation turns on within-comparison validity, through the baseline-score and faculty-composition controls just described, rather than on external representativeness.

Among eligible *Enseña Chile* partner schools, the recorded reasons for non-participation were administrative rather than performance-related: several already administered SEPA annually, while the rest declined or did not respond (see Online Appendix A). This pattern makes selection on school quality less likely, although we cannot rule out selection on unobservables within the comparison. The direction of any resulting bias is ambiguous: improvement-oriented partner schools could pull *Enseña Chile* estimates upward, while less-needy schools that select out could pull them downward.

We measure experience from the MINEDUC *Docentes (Idoneidad Docente)* administrative panel rather than the earlier 2011 service file, which had mean-imputed about half the comparison teachers to mid-career. The corrected data show that 38 of 77 traditional teachers are themselves early-career (≤ 5 years of service; median three years in *Enseña Chile* partner schools), creating genuine overlap with *Enseña Chile* teachers in the experience distribution. This overlap allows us to estimate the route-of-preparation residual by comparing second-year *Enseña Chile* teachers with traditional teachers at similar early-career experience, rather than relying on the cohort-comparability assumption used in earlier drafts. The within-*Enseña Chile* first-to-second-year contrast provides a separate check on the first-year penalty.

The decomposition imposes an additively separable form: the modest teacher count and thin within-school cells do not identify an ability-by-experience interaction, which therefore loads onto the route residual. Two cautions also apply: no traditional teacher is observed in their literal first year, so a generic first-year dip cannot be fully separated from an *Enseña Chile*-specific one; and the route residual itself is estimated from a small sample, so we report it with its confidence interval in Section 3.

3. Results

At the teacher level, *Enseña Chile* teachers' mean VA is -0.19σ versus $+0.13\sigma$ for traditional teachers, a gap of roughly 0.32σ significant at the 5% level. Because the sampled traditional teachers were deliberately drawn to span the PSU/PAA distribution rather than to represent the national workforce, this raw gap is an analytic-sample contrast; reweighting to the national common-support PSU/PAA distribution narrows it to -0.16σ. As we show below, this reweighted gap decomposes into positive selection on ability, a first-year experience penalty, and a route residual statistically indistinguishable from zero.

Table 2 reports the corresponding regression evidence. PSU/PAA strongly predicts VA: in the pooled Model 2, a one-SD increase is associated with a 0.51σ increase in VA (classroom-clustered s.e. 0.12), significant at the 1% level and robust to teacher- and school-clustered standard errors. The gradient is robust to the school faculty-composition control described below: it is essentially unchanged at 0.49σ per SD (s.e. 0.12) in column 3. Figure 1 visualizes this positive ability–VA relationship.[2]

Consistent with this gradient, our companion study shows that pre-college academic achievement also predicts other long-run Chilean teacher outcomes—exit exams, in-classroom evaluations, wages, and employment [24].

A remaining concern is that the teacher-level gradient may partly reflect where teachers work. To address this, we control for the academic composition of each school's teaching staff. For each sampled teacher, we compute the leave-out mean national entrance-exam (PSU/PAA) percentile of the school's *other* teachers, using the 2011 teacher census linked to DEMRE scores (available for 58 of our 59 schools). This continuous school-input measure is estimable across the sample, unlike school fixed effects, which the design cannot support.

[2]Table 2 coefficients are student-weighted (analytic-sample SD). The decomposition (Fig. 3) weights teachers equally, partials out experience and route, and uses national-teacher SD units—yielding $+0.24\sigma/\text{SD}$, a complementary summary scaled to that exercise.

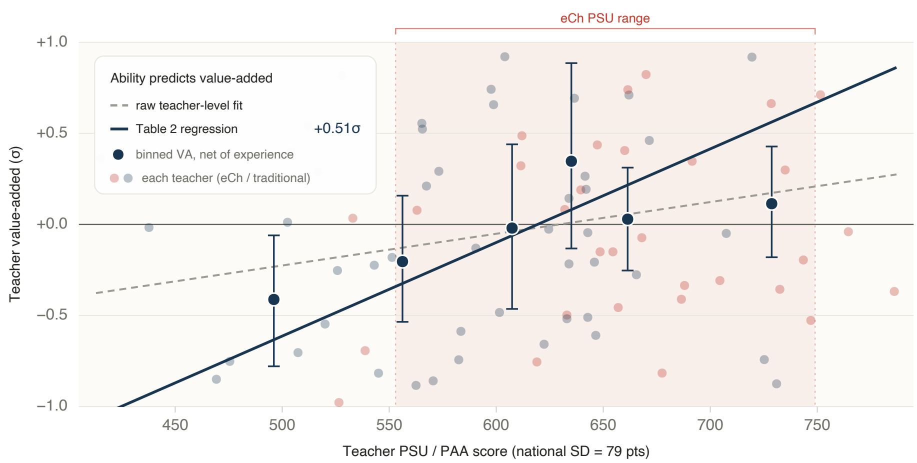
Table 2: Regression models of teacher VA.

	(1) Model 1	(2) Model 2	(3) Model 2 + Faculty
no-yes	0.275	0.457	0.422
	(0.234)	(0.241)	(0.234)
yes-yes	0.160	0.116	-0.006
	(0.208)	(0.288)	(0.306)
PSU/PAA (std.)		0.514***	0.489***
		(0.123)	(0.119)
Experience (years)		0.039	0.019
		(0.033)	(0.037)
Experience^2		-0.0005	0.0000
		(0.001)	(0.001)
School faculty ability (std.)			0.207*
			(0.086)
Mid-Low SES	-0.343*	-0.404*	-0.342*
	(0.158)	(0.157)	(0.165)
Mid SES	-0.226	-0.350	-0.443*
	(0.229)	(0.235)	(0.218)
High SES	0.084	-0.248	-0.349
	(0.335)	(0.291)	(0.294)
Class Size	0.035**	0.042***	0.035**
	(0.013)	(0.010)	(0.012)
Grade	0.340**	0.332***	0.315***
	(0.109)	(0.096)	(0.094)
Constant	-4.254***	-7.642***	-7.038***
	(1.121)	(1.080)	(1.086)
R^2	0.121	0.283	0.312
Observations	3,454	3,454	3,398

Notes: Dependent variable is standardized teacher value-added assigned to students. Route indicators omit no-no (traditional, non-eCh school). Model 2 adds PAA/PSU, experience, and experience squared; column 3 adds the leave-out school-faculty PAA/PSU percentile (the mean for the school's other teachers), which is undefined for teachers with no other PSU-linked teacher in their school; dropping those 56 students reduces the column 3 sample from 3,454 to 3,398, while columns 1 and 2 share the same 3,454-student sample. All models include SES tier, class size, and grade; classroom-clustered standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Faculty composition is predictive in its own right: a one-SD increase raises teacher VA by $+0.21\sigma$ (classroom-clustered s.e. 0.09; column 3 of Table 2), significant at the 5% level but only marginally so under teacher- and school-clustered standard errors (Online Appendix Table B.6). Conditioning on it leaves the main results intact—the PSU/PAA gradient changes little ($0.51\sigma \rightarrow 0.49\sigma$), the experience profile remains flat, and the route contrast remains near zero—so the ability and route findings are not artifacts of

Figure 1: Teacher-level value-added–PSU/PAA relationship.



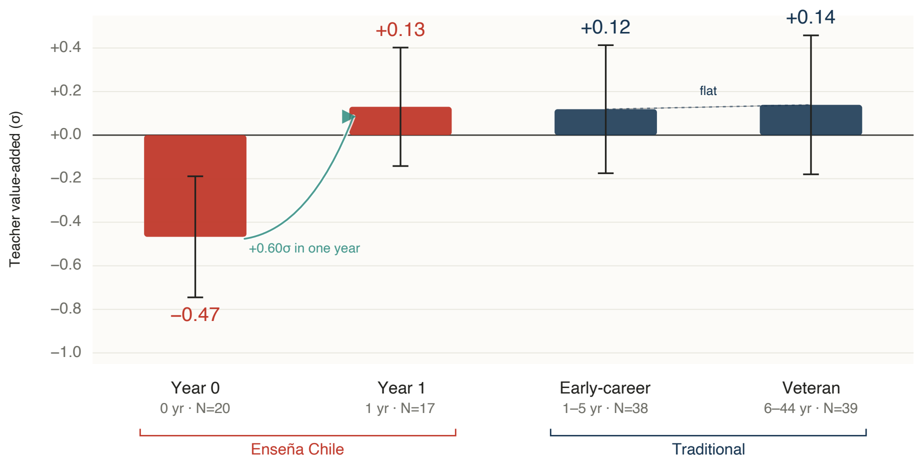
Notes: Blue: student-level Model 2 PSU/PAA coefficient ($+0.51\sigma$, Table 2). Dashed gray: unadjusted teacher-level fit. Faint dots: teachers. Navy: PSU/PAA-binned VA, experience and route partialled out (Robinson); whiskers are teacher-bootstrap 95% intervals.

placement.

Turning to experience, the pooled relationship is flat on average. The marginal-year coefficient is small (0.04, s.e. 0.03) and not statistically distinguishable from zero, consistent with Papay and Kraft [25] and Ladd and Sorensen [20] on diminishing returns past the early career.

The early-career pattern within *Enseña Chile* is much steeper. Because *Enseña Chile* teachers are concentrated in years 0–2, the within-route contrast captures the part of the experience profile most relevant for the program: second-year *Enseña Chile* teachers' VA averages $+0.13\sigma$, about 0.6σ above the first-year average of -0.47σ (Figure 2), a difference significant at the 1% level. This improvement is well above typical first-year returns documented in the U.S. literature. It should not be read as a pure return to experience, however, because it compares the 2015 and 2016 *Enseña Chile* cohorts (different teachers in a single testing wave) and therefore may also reflect cohort selection.

Figure 2: Experience profile by route on the corrected experience data.

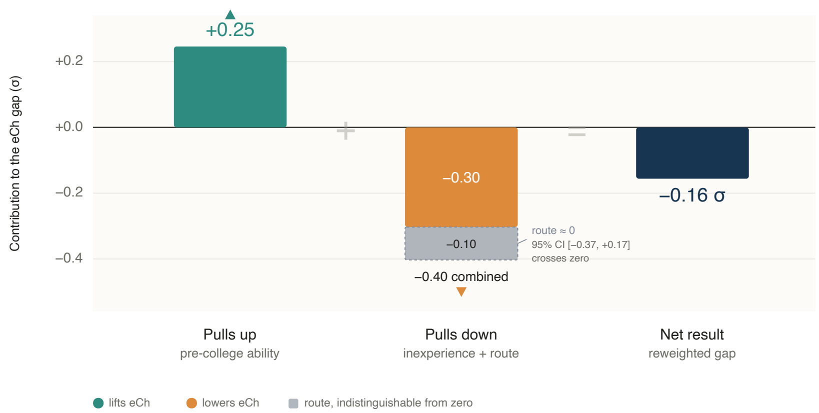


Notes: Mean VA by route and experience tier; whiskers are teacher-bootstrap 95% intervals. Experience corrected via the MINEDUC *Docentes* panel. Group means are reported in the body.

We next separate the selection and experience channels. Using the experience-corrected data and reweighting sampled traditional teachers to the national common-support PSU/PAA distribution, the decomposition-sample gap changes from -0.26σ unweighted to -0.16σ reweighted (Online Appendix Section B.8). Figure 3 decomposes this reweighted gap into three additive components: a $+0.25\sigma$ pre-college ability contribution from *Enseña Chile* teachers' PSU/PAA advantage, a -0.30σ first-year inexperience penalty, and a -0.10σ route-of-preparation residual.

The residual is statistically indistinguishable from zero. Consistent with this, second-year *Enseña Chile* teachers reach the traditional mean in the raw data ($+0.13\sigma$ for both groups), while the regression-adjusted *Enseña Chile* route coefficient in Table 2 is also near zero. Because the residual is estimated from a modest number of teachers, it remains imprecise: the data are consistent with pre-college ability plus rapid early-career learning fully accounting for the reweighted gap, but they cannot rule out a moderate route shortfall.[3]

Figure 3: Decomposition of the reweighted common-support *Enseña Chile*–traditional value-added gap.



Notes: Additive components of the reweighted teacher-level gap (sum within rounding). *Enseña Chile* mean unweighted; sampled traditional teachers reweighted to the 2011 teacher-census PSU/PAA distribution over common-support deciles (Online Appendix B.8). Whisker: traditional-side stratified-bootstrap 95% CI on the route residual ($N = 104$).

Placing *Enseña Chile* teachers in the national distribution makes the selection-versus-experience tension concrete (Online Appendix Figure B.2). Using the fitted ability–experience model to predict VA for the PSU-era

[3]Conclusions survive restricting to partner schools, dropping first-year *Enseña Chile* teachers (Model 1 *Enseña Chile* coefficient +0.35, s.e. 0.23; Online Appendix Table B.1), alternative SEPA standardizations, and bootstrap inference (Online Appendix B).

teacher cohort, *Enseña Chile* recruits sit near the 97th percentile of ability (+1.9 SD above the mean teacher). Their realized VA, pooled across first- and second-year teachers to avoid relying on small year-specific cells, falls at the 42nd percentile. That places them around the national median and close to the typical teacher already working in the schools they serve (51st percentile). In short, selection places *Enseña Chile* teachers near the top of the ability distribution, while their early-career status brings realized VA back toward the middle.

Student perception surveys are consistent with this pattern, though they provide only weak corroborating evidence. *Enseña Chile* classrooms score 0.17 points higher on the 5-point survey average, a statistically significant gap, but the classroom-level survey–VA correlation is small (see Online Appendix B.3).

4. Discussion and conclusion

The main empirical result is a decomposition of the reweighted common-support effectiveness gap between alternative-pathway and traditional teachers in Chile. In our linked data, that gap reflects two offsetting forces: a positive contribution from *Enseña Chile* teachers' pre-college cognitive ability ($+0.25\sigma$) and a first-year inexperience penalty (-0.30σ). What remains is a small route-of-preparation residual (-0.10σ), statistically indistinguishable from zero.

The residual is estimated from the early-career overlap revealed in the corrected experience data (Section 2) and from a modest sample. It is a remainder rather than a structural route parameter, absorbing route of preparation together with any ability-by-experience interaction the additive specification omits and any teacher-level unobservables (motivation, unmeasured selection) correlated with route. The decomposition is descriptive, not causal. The estimates are consistent with selection plus rapid early-career learning explaining most of the gap, in line with U.S. teacher preparation program (TPP) evidence that within-route variance exceeds mean differences between routes [17, 16] and with cross-national evidence that teacher cognitive skills predict student outcomes [13]. The result is best read as a failure to detect a route penalty rather than as definitive evidence of parity.

These findings speak to the margin between preparation routes, not to the value of preparation itself. They are consistent with the marginal year of formal pedagogical coursework adding little measured VA beyond pre-college ability and structured early-career learning. Nor is the contrast between trained and untrained teachers. *Enseña Chile* provides four weeks of intensive pre-service instruction followed by two years of in-service coaching, peer learning, and tutor visits. The relevant comparison is therefore between a traditional model that front-loads preparation over four to five years and an alternative model that compresses pre-service training and combines it with on-the-job support. Evidence from China, where specialized teachers' colleges modestly outperform comprehensive-university preparation [28], also suggests that practice-based pedagogy may add value. Our design cannot distinguish that content margin from total time in coursework.

For policy, the relevant counterfactual is a staffing decision: how an *Enseña Chile* teacher compares with the *marginal* hire who would otherwise take the slot, not the average traditional teacher or the unusually high-ability ones in our purposive sample. We do not observe that marginal hire, so what follows is an interpretation of the evidence, not a direct estimate.

Two facts make the staffing case plausible. First, *Enseña Chile* recruits sit near the 97th percentile of the national teacher ability distribution, well above the teachers who typically staff comparable low-SES schools (Online Appendix Figure B.2)—and local labor markets are thinnest precisely where strong candidates are scarce [22, 5]. Second, Model 1 (no controls; Table 2) places *Enseña Chile* teachers at rough parity with the traditional teachers already working in these schools, and above them once first-year teachers are set aside (Online Appendix Table B.1). Together with the $+0.51\sigma$-per-SD ability gradient, this *suggests* (though absent the counterfactual hire cannot establish) that placing an *Enseña Chile* teacher need not lower, and may raise, instructional quality where recruitment is hardest. The argument weakens wherever a high-PSU/PAA traditional alternative is readily available.

The SES breakdown is consistent with this interpretation, though the cells are small (Online Appendix Table B.3). *Enseña Chile* teachers are at rough parity with traditional teachers in the lowest-SES tier (Traditional VA -0.08 vs. *Enseña Chile* -0.11). In the low-mid and mid tiers, where the traditional comparison teachers are stronger in the raw data, *Enseña Chile* teachers have lower VA (-0.28 vs. $+0.32$ in low-mid; -0.34 vs. $+0.06$ in mid). The small *Enseña Chile* samples in those tiers (14 and 3 teachers, respectively) limit inference, but the pattern is consistent with selective placement of *Enseña Chile* in the most disadvantaged schools, where the counterfactual hire is most likely to have low PSU/PAA.

Several limitations should guide interpretation. First, the VA estimates are cross-sectional teacher fixed effects from a single school year, producing wide standard errors on 114 teachers. Second, the purposive sample describes this analytic group; voluntary school participation could still generate selection on unobservables, though recorded non-participation reasons were administrative (Online Appendix A) and the direction is ambiguous. Third, the first- to second-year *Enseña Chile* improvement spans two small cohorts (20 and 17 teachers) and cannot fully separate within-teacher learning from cohort selection, differential attrition, or transitory VA noise; a multi-year *Enseña Chile* cohort panel would address this.

Three implications follow for *Enseña Chile*. First, we fail to detect a route penalty: conditional on ability and experience, *Enseña Chile*'s compressed pre-service training plus in-service coaching yields VA statistically indistinguishable from the traditional four-to-five-year degree, though the data cannot rule out a moderate shortfall. Second, in the schools where *Enseña Chile* places teachers, its recruits can plausibly sustain, and may raise, instructional quality. Third, experience is the binding constraint. Because the program runs for two years, its teachers mechanically leave just as early-career returns are largest; retention beyond the two-year commitment is therefore the margin with the most measured upside.

The boundary is equally important. This paper evaluates the *teachers* *Enseña Chile* supplies, not the *program*. We do not observe its effect relative to the teachers who would otherwise have been hired, its impact on the broader teacher pipeline, or participants' later trajectories in and out of education. Those are program-evaluation questions that our purposive, cross-sectional VA design is not built to answer.

The broader lesson is necessarily cautious, but it extends beyond *Enseña Chile*. For systems facing shortages in secondary mathematics, selective alternative pathways coupled with structured coaching may expand the supply of effective teachers without a detectable loss of instructional quality, provided they also confront the retention problem that fixed-term models create. On this evidence, the relevant policy margin is not simply whether four-year teacher colleges or short alternative routes produce more effective teachers on average. It is whether a system can attract high-ability candidates, support them through the steep early-career learning curve, and keep them in the classrooms where they are needed most.

CRediT authorship contribution statement

Conceptualization: SDG, CAN, SG. Methodology: SDG, SG. Formal analysis: SDG. Investigation: SDG. Data curation: SDG, CAN. Writing–original draft: SDG. Writing–review & editing: SDG, CAN, SG. Visualization: SDG. Supervision: CAN. Project administration: SDG. Funding acquisition: CAN.

Declaration of competing interest

The authors declare no competing interests.

Data availability

The student assessment and teacher administrative data contain sensitive information and are not publicly available. De-identified replication materials are available from the authors subject to data-sharing agreements with MIDE-UC (SEPA), MINEDUC/DEMRE (PSU/PAA), and Enseña Chile (program rosters).

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Ethics approval

Data collection in 2016 proceeded with the consent of participating schools and parents. The linked analysis uses de-identified assessment, survey, and administrative records governed by data-sharing agreements; reported results are aggregate and do not identify individual students, teachers, or schools.

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Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used OpenAI's ChatGPT/Codex to assist with language editing, formatting, and consistency checks. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

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