Enhancing Human Capital at Scale*

Francesco Agostinelli† Ciro Avitabile‡ Matteo Bobba§

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Abstract

A two-year randomized evaluation shows that the effectiveness of mobile mentors on schooling outcomes crucially depends on their training. While a standard training modality in highly marginalized communities in Mexico generates null results, enhanced training yields sizable treatment effects on primary school children's cognitive, behavioral, and educational achievements. This difference cannot be explained by remedial educational activities or pedagogical support, but it can be reconciled with higher parental aspirations and investments. Evidence gathered on the subsequent national roll out of the intervention with enhanced training substantiates the scalability of the experimental design.

Keywords: Educational Investments; Family Investments; Remedial Education; Pedagogical Practices; Scaling-up Effects.

JEL codes: H43, I10, I20, I38.

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†Economics Department, School of Arts and Sciences, University of Pennsylvania (US). E-mail: fagostin@sas.upenn.edu

‡World Bank (US). E-mail: cavitabile@worldbank.org

§Toulouse School of Economics, University of Toulouse Capitole (France). E-mail: matteo.bobba@tse-fr.eu

1 Introduction

There is a wide learning gap among school-aged children between urban and rural areas in developing countries (World Bank, 2018). Efforts to improve education outcomes in rural areas are often hampered by low levels of student readiness (Gertler et al., 2014), high rates of teacher absenteeism and turnover (Duflo et al., 2012; Albornoz et al., 2020), and inadequate parental support (Attanasio et al., 2020a).

In this paper, we provide new insights on whether and how a multifaceted approach can improve child outcomes in remote areas of Chiapas—the poorest state of Mexico. In the program, the government enlists recent university graduates as mobile mentors in an attempt to improve the quality of schooling in disadvantaged communities. These young professionals often have first-hand experience as community instructors and speak the local language. They provide pedagogical support to the local instructors, organize one-on-one remedial education sessions for the students who are lagging behind, and encourage parental involvement in children's education through home visits.

The evidence is drawn from a field experiment recently implemented in close collaboration with the government agency that is in charge of delivering education services in such low-resource environments. We randomly vary both the presence and the training intensity of the mentors among a subset of multi-grade primary schools. One treatment arm of the evaluation design features a standard training modality for mobile mentors of a pre-existing intervention implemented by the government. A second treatment embeds a significant change in the training module, in the form of extra sessions with hands-on strategies to teach basic reading and math competencies as well as periodic peer-to-peer meetings during the school year.

Training matters for the effectiveness of the educational mentors in our context. While the standard training modality yields null results on child outcomes after two years of exposure when compared to the control group with no mentors, enhanced training leads to sizable gains in children's reading scores (+0.32 standard deviations), math scores (+0.24 standard deviations), socio-emotional scores (+0.20 standard deviations), and a marginally significant effect on the probability of enrolling in lower secondary education (+12.7 percentage points, out of a basis of 62 percent enrollment in the control group). Mentors with enhanced training seem to be particularly effective for lower-achieving children, thereby leading to more homogeneous classroom environments in terms of education achievement.

The large difference in effect sizes between the two training modalities does not seem to be explained by either changes in the pedagogical practices of the community instructors or by differences in the relative effectiveness of the remedial educational sessions. Instead, we provide evidence that mentors with enhanced training engage more with parents, in terms of both the quantity and the quality of their periodic interactions. We show that mentors with enhanced training are more likely to inform parents about their children's learning difficulties, as well as to provide concrete advice to parents on how to tackle these difficulties. This mechanism is corroborated by direct evidence of differential impacts of the training modality on parental aspirations toward children's education attainments as well as different measures of parental investments.

In recent years, scholars and policy makers alike have been increasingly concerned about the ability of randomized controlled trials (RCTs) to inform policy decisions, since interventions that have been found effective in small-scale pilots often fail to hold to their promises when implemented at scale (Bold et al., 2018; Cameron et al., 2019; Muralidharan and Singh, 2020). The subsequent national roll out of the enhanced training modality provides us with a unique opportunity to study the effectiveness of the program for the same set of communities under both the experimental regime and the large-scale policy environment. The setting allows us to rule out the conventional challenges to scalability, such as context dependence and randomization/site-selection bias (Heckman, 1991). The relatively large units of randomization (communities) is also robust to possible local spillover effects. Importantly, we can precisely tease out the differences in implementation between the pilot and the large-scale program.

We find that one year of exposure to the intervention at scale increases the probability of children's transitioning into secondary school by 9.4 percentage points, which is remarkably close to the experimental estimate. The cumulative effect of three years of exposure to the enhanced training modality implies that a sixth grader from a disadvantaged rural area in Chiapas is as likely to enroll in a secondary school as a sixth grader from urban Mexico. In other words, a scalable intervention can achieve the remarkable objective of closing the urban-rural gap in secondary school enrollments.

We also find that schools are more likely to remain open and to continue their service in the community two years after the end of the experiment. One possible explanation behind this result is that changes in educational aspirations and parental involvement in the school activities may have kept open some schools thereby bolstering the success of the program at scale. This interpretation is consistent with the hypothesis that educational investments at the community level are a socially determined outcome (List et al., 2019). Moreover, the importance of keeping schools open for the development of children has recently gained the attention of educational studies on the impact of the COVID-19 lock downs on children in developed countries (see, e.g., Agostinelli et al., 2020b; Engzell et al., 2020; Maldonado and De Witte, 2020). In the context of a developing country, Duflo (2001) finds that opening schools causes an increase in both education attainments and subsequent wages.

There is a consensus that gaps in family investments are behind the gaps in children's achievements among different socioeconomic groups (Heckman and Mosso, 2014). Family inputs are essential in the formation of skills during the critical periods of childhood (Cunha et al., 2010; Agostinelli and Wiswall, 2020). Moreover, parental investments and parenting styles are responsive to the environments that families face (Doepke and Zilibotti, 2017; Agostinelli, 2018; Agostinelli et al., 2020a). More closely related to our paper, Attanasio et al. (2014); Fernald et al. (2017); Carneiro et al. (2019); Attanasio et al. (2020b) document that home visits and parenting interventions can spur cognitive and socio-emotional outcomes for children in the first years of life. We build on this literature by establishing a complementarity between mentors' practices and parental investments in the production of cognitive and socio-emotional skills for school-aged children.

Parental beliefs and attitudes towards education are another important channel through which mobile mentors may have enhanced children's achievement outcomes in our setting. These findings speak to a recent and growing body of literature that studies how information provision about children's education shapes parental subjective expectations and investments (Andrabi et al., 2017; Dizon-Ross, 2019; Bergman, 2021). Related work by Boneva and Rauh (2018) documents that perceived returns to early parental investments are positively associated with household income, while Bursztyn and Coffman (2012) use a lab experiment with low-income families in Brazil to show that parents are willing to pay substantial amounts of money for information on their children's school attendance. Our results highlight the key role of training and peer-to-peer interactions of mobile mentors to overcome possible information frictions between disadvantaged parents and their children.

This paper further contributes to the literature on the evaluation of education programs by showing that a scalable intervention in a low-resource environment can be effective in the production of children's human capital (Avvisati et al., 2013; Fryer et al., 2015; Carneiro et al., 2020; Andrew et al., 2020). The evidence reported here is consistent with the notion that teachers' pedagogical practices are difficult to improve, especially in disadvantaged contexts (Yoshikawa et al., 2015; Ozler et al., 2016; Bassi et al., 2020). While there is evidence on the effectiveness of remedial education interventions for under-performing children (Banerjee et al., 2007, 2017), we show that this cannot explain the observed difference between training modalities on children's outcomes.

2 Context and Design of the Evaluation

2.1 CONAFE and the Mobile Pedagogical Mentors

CONAFE is a semi-autonomous government agency responsible for providing schooling services in highly marginalized areas of Mexico with a significant share of indigenous population. In those communities, CONAFE offers all education services from pre-school until the end of lower secondary school (9th grade). Primary schools typically have a single multi-grade classroom with 10–15 students. Parents organize a local association aimed at promoting community education through which they contribute in the definition of the class schedule, the development of the curriculum, the distribution of school material and scholarships, and the decision to keep the school open when the number of enrolled students falls below the statutory threshold of five.

CONAFE instructors are generally community residents between 15 and 29 years old. They do not have formal teacher training and they are supposed to stay in the same school for two years. Only 2.6 percent report having a college degree, while 19 percent report having only completed lower secondary education. Instructors should receive between five and seven weeks of training, but more than half report four weeks of training or less. They receive a stipend of MXN $1,427 per month (USD $95 in 2015). After one year of service, instructors become eligible to receive a scholarship of MXN $982 per month for up to 30 months, which is conditional on enrolling in a higher education institution. As a result of the very low compensation and extremely challenging conditions, about one quarter of the instructors drop out before completing the first school year.

In 2009, CONAFE launched the "Mobile Mentors" (*Asesores Pedagogicos Itinerantes,* API henceforth) program as an attempt to improve the quality of education provision in schools located in remote and disadvantaged areas. The mentors are selected from recent university graduates. CONAFE advertises the program both with on-campus visits and announcements through the media. Preference is given to applicants with degrees in pedagogy, psychology, sociology and social services who have previous experience as community instructors and who speak an indigenous language. They are usually hired for a two–year period and receive a monthly salary of MXN $6,000 (USD $345 in 2015). Partly due to the fixed-term contract, they are not covered by either social security or healthcare assistance.[1]

CONAFE schools receive the API program in pairs, with the mentors spending an equal amount of time in both communities. Each pair consists of one school, which is the main target of the intervention, and another school that is included on the basis of proximity to the target school. Target schools are selected according to the following criteria: (i) they have at least 30 percent of the students classified as "insufficient" in the Nationwide Standardized test ENLACE; and (ii) they have at least six students enrolled in primary school. Among the schools that met the above criteria, preference was given to the municipalities with communities that are characterized by high levels of poverty, difficulty of access, and a large presence of indigenous communities. Both target and auxiliary schools can, in principle, refuse the assignment of a mentor, which may happen if, for instance, the local communities lack resources to satisfy the mentors' basic needs (lodging and meals).

The mentors carry out three main activities in each school community with a predetermined time allocation: (i) one-on-one tutoring to the least-performing students in remedial sessions (60 percent of their time), (ii) pedagogical support to teachers (15 percent), and (iii) visiting parents at their homes to provide them with information on their children's progress in school and promote their participation in school activities (25 percent). Each mentor is assigned to a maximum of six students for the personalized remedial sessions, which in principle should take place outside of regular school hours. During the regular school hours, the mentor is supposed to observe and take notes about the teaching practices of the local instructor, help them with the students who have learning difficulties, and work outside the classroom with those students who cannot attend one-on-one tutoring in the afternoon. In addition to working on behavioral issues directly with the children, the mentors are supposed to address them with parents as part of the home visits.

Student eligibility for the remedial education sessions is determined by a joint assessment of the instructor and the mentor, which is based on a diagnostic evaluation that the instructor conducts at the beginning of the school year as well as the student's difficulties in

[1] Teachers under the regular system have a permanent contract with a monthly salary starting at MXN$ 8,000 and have both healthcare and social security benefits.

reading and basic math and having repeated one or more grades. The evaluation covers the material that students should have mastered in the previous grade. Preference is given to students in the 3rd to 6th grades. Once the eligible students have been identified, the mentor administers an additional exam to establish the effective grade to which the student's knowledge corresponds. With this information in hand, the mentor prepares a personalized plan. Throughout the school cycle, the mentor provides the students with constant feedback and constantly monitors their progress through a personalized evaluation form.

In the year 2013, CONAFE schools accounted for 10 percent of the roughly 99,000 primary schools and 7 percent of the 38,000 lower secondary schools across the 31 Mexican states. In total there were 2,099 mentors operating in CONAFE schools. Dropout among mentors is higher than among community instructors (about 40 percent per school year), as they are more likely to find jobs that are either better paid or are based in better locations. Indeed, it is quite common for the mentors to switch to a regular teacher job as a result of periodic national recruitment drives.

2.2 Evaluation Design

In close collaboration with CONAFE, we developed and unrolled a field experiment with the objective of evaluating the relative effectiveness of one modality aimed at strengthening the API program. The API Standard modality is meant to track the benchmark intervention described in Section 2.1 with two minor differences. First, the ability to speak the main indigenous language in the community would become the most important criterion for the assignment of the mobile mentors across program-eligible communities. Second, the supervisors of the mobile mentors would receive a salary increase in exchange for a mandatory increase in the frequency of their visits to the targeted communities. The API Plus modality embeds all the features of the API Standard, with a significant change in the training module. The module entails two weeks rather than one week of initial training sessions, with the second week focused on hands-on strategies to teach basic reading and math competencies. In addition, mobile mentors attend four peer-to-peer training sessions during the school year. These three-day sessions (18 hours per session) allow participants to share experiences and design common strategies to better organize their day-to-day activities in the communities.

We randomly select 230 schools in rural Chiapas from a set of CONAFE schools that were not previously part of the API program. The assignment of the API modalities is carried out using a randomized block design clustered at the school level, with the strata represented by the deciles of the 2012 school average in a national standardized achievement score. As a result, 60 schools are assigned to the API Plus, 70 schools are assigned to the API Standard, and the remaining 100 schools are in the control group with no API intervention.

The intervention was rolled out in August 2014. While the official enrollment threshold for closing CONAFE schools is six students, the schools in our sample were allowed to remain open if they had at least three enrolled students in either school years 2014–2015 and 2015–2016. In addition, evaluation schools with more than 29 enrolled students were not required to transition into the regular public-school system. To avoid refusal of the assigned mentor among the communities of the target schools, each mentor in the evaluation sample would donate two food baskets to the community leaders (one at the beginning and the other at the end of the school year). This strategy was indeed effective as no schools in the evaluation sample refused the assigned mentors.[2]

As a way to attenuate the potentially detrimental consequences of mentors' dropping out of the program during the evaluation period, the CONAFE delegation in Chiapas arranged for a replacement within two weeks from the day of the departure from a community. The replacement candidate would be randomly selected from those who had served during the previous school cycle and who complied with the new eligibility criteria outlined in Section 2.1. If the dropout was part of the Plus group, the replacement would receive an additional three-day training session that would make up for the content covered during the extra week of the initial training session.

2.3 Data and Measurement

We rely on administrative data sources for baseline information collected in the year 2013 about school and student characteristics. These variables are shown in Table 1 and they are well balanced with respect to treatment assignment. Table C.1 in the Appendix shows a randomization check across a much wider array of socio-demographics for the households, the community instructors, and the mentors in our sample taken from the survey data (discussed below). All of these variables are balanced except for a larger share of indigenous households in the Plus group, which is statistically significant at the 10 percent level.

The schools in our sample are broadly comparable to the average CONAFE school in Chiapas in terms of size, number of local instructors and student characteristics. The communities where these schools are located are very small, averaging 100 inhabitants, and they are difficult to access, with one-fifth of the schools in our sample having no road access whatsoever. Households in our sample are on average very poor, with 80 percent of them report being *Oportunidades* beneficiaries and only one fourth having access to the sewage system.[3] About two thirds of the primary care givers of the children in our sample did not complete primary education, and almost 30 percent are illiterate. Consistently with the targeting design of the API program, the majority (80 percent) of the mentors in our sample have previous experience in CONAFE schools as community instructors (see Appendix Table C.1).

The first round of data collection took place by the end of the second school year since the inception of the API program, in the spring of 2016. The survey contains modules on instructors' characteristics and pedagogical practices, parental attitudes and practices as well as information about the mentors' activities in the communities, among others. The

[2]Similar arrangements could not be put in place in the context of auxiliary schools that were matched with the target schools in the evaluation sample. More broadly, limited financial resources did not allow either to monitor the implementation of the intervention or to collect survey data in auxiliary schools.

[3]The *Oportunidades-Prospera* program was a large-scale welfare program that targeted poor households in both rural and urban Mexico over the last two decades (Parker et al., 2008).

Table 1: Baseline Characteristics and Covariate Balance

Sample	CONAFE Chiapas	Control	API Standard	API Plus	All Evaluation	
(Number of Schools)	(2,175)	(100)	(60)	(70)	(230)	
Statistic	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)	Standard-Control (SE)	Plus-Control (SE)
Panel A: School Characteristics						
Average Test score Spanish	444.30	431.88	431.65	431.36	-0.223	-0.516
	(68.80)	(64.43)	(66.87)	(66.60)	(2.581)	(2.783)
Average Test score Math	479.47	455.75	454.85	451.68	-0.902	-4.076
	(95.63)	(80.71)	(83.50)	(81.06)	(5.790)	(6.911)
Average Test score Science	430.41	440.15	441.24	441.27	1.095	1.120
	(63.53)	(52.52)	(48.66)	(50.89)	(4.273)	(4.784)
Community Instructors	1.170	1.220	1.300	1.200	0.080	-0.020
	(0.420)	(0.416)	(0.462)	(0.403)	(0.066)	(0.067)
Number of Enrolled Students	11.610	15.160	15.314	14.233	0.154	-0.927
	(8.750)	(5.839)	(5.714)	(5.782)	(0.901)	(0.946)
Panel B: Student Characteristics						
Baseline Age (Months)	109.17	104.99	104.29	105.54	-0.818	0.647
	(24.84)	(16.38)	(17.53)	(14.92)	(1.169)	(1.247)
Male	0.510	0.533	0.519	0.543	-0.012	0.011
	(0.500)	(0.499)	(0.500)	(0.499)	(0.033)	(0.043)
Grade in Spanish	7.510	7.499	7.634	7.546	0.143	0.043
	(0.850)	(0.887)	(0.829)	(0.778)	(0.100)	(0.089)
Grade in Math	7.560	7.632	7.720	7.617	0.096	-0.018
	(0.870)	(0.931)	(0.842)	(0.833)	(0.100)	(0.092)
Grade in Natural Science	7.570	7.579	7.710	7.582	0.137	-0.003
	(0.820)	(0.856)	(0.773)	(0.731)	(0.097)	(0.092)
Grade in Social Sciences	7.420	7.416	7.585	7.451	0.174	0.031
	(0.800)	(0.859)	(0.782)	(0.750)	(0.102)	(0.097)
Repeater	0.010	0.024	0.003	0.012	-0.022	-0.013
	(0.100)	(0.152)	(0.057)	(0.107)	(0.011)	(0.012)

Notes: The first three columns of the table reports mean and standard deviations in parenthesis for various characteristics collected before the assignment of the API program in the evaluation sample. The school variables in panel A are computed from the 2013 national standardized tests (ENLACE) and from the 2013 school census. Student characteristics reported in panel B come from the CONAFE administrative records collected at the beginning of the school year 2014–2015. See Appendix A for more details on the data sources. The differences reported in the last two columns of the table are based on OLS regressions that control for stratification dummies. Standard errors of the mean differences for the student characteristics are reported in parenthesis in the last two columns and they are clustered at school level.

survey information can be conveniently linked with the administrative records through a unique student identifier. We rely on the Early Grade Reading Assessment (reading score) and the Early Grade Math Assessment (math score) as our main measures of children's cognitive achievement. Those are individually administered oral student assessments that have been conducted in more than 40 countries and in a variety of languages (Dubeck and Gove, 2015; Platas, 2016). While these instruments are typically applied to students in first, second, or third grade, we administer them to 3rd-6th students to account for the large learning gaps of the children in our sample when compared to the national average (see below).[4] In order to measure the impact of the intervention on socio-emotional skills, we consider different measures of behavioral problems as reported by a caregiver such as antisocial behavior, anxiety/depression, headstrongness, hyperactivity and peer conflicts.

[4] Only 5 percent of the children in our sample score at the maximum of the scale in two or more sub-domains of the reading score (out of eight sub-domains) and in three or more sub-domains of the math score (out of a total of seven sub-domains).

The resulting behavioral problem index is re-scaled in such a way that higher values are associated with fewer behavioral issues (socio-emotional score). A follow-up survey with a subset of these modules was conducted in the fall of 2018. A full description of the different datasets as well as the key variables employed in the analysis is provided in Appendix A.

By the time of the first data collection, six schools out of the original 230 schools in the evaluation sample closed either temporarily or permanently, one quarter of the community instructors reported eight or fewer months of tenure in the school, and only 48 out of the original 130 mobile mentors were working in the same schools where they had been originally assigned. All these outcomes are well-balanced across treatment arms.[5] There is no evidence of composition changes between the Standard and Plus groups induced by the mentors' turnover (see Table C.2 in the Appendix). This evidence, together with the fact that the characteristics of those mentors who were active in the evaluation schools two years after the inception of the program are also balanced across treatment arms (see panel C of Table C.1 in the Appendix), suggests that the replacement process of the mentors mentioned in section 2.1 did not invalidate the original random assignment.[6]

The main sample of the analysis is comprised of 224 schools. While the cognitive scores have been collected for all students enrolled in third to sixth grade in these schools, the household survey—that contains the individual items of the socio-emotional score, as well as parental expectations and investments toward children's education, among others—has been collected for a random sample of five households within a 5 kilometer radius from each school. This gives a total of 1,020 households with complete records and 1,045 students enrolled in third to sixth grade by the end of the second school year since treatment assignment. For these schools, the average standardized scores in math and Spanish are, respectively, 0.5 and 0.7 standard deviations below the national averages.

The sample reduces to 182 schools when we consider the 468 sixth graders in 2016 who may have potentially transitioned to a secondary school during the following school year. The choice of this cohort of students is meant to maintain the same length of exposure to the API program of the main sample of the analysis. Less than two thirds of these sixth graders in the control group enroll in secondary schooling while the corresponding national average is 95 percent.

[5] Two schools were found closed in the control group, two in the API Standard group, and two in the API Plus group. The p-values of the Komolgorov-Smirnoff statistic for the equality of the distributions of tenure in the school of the community instructors in each treatment arm and the control group are 0.773 and 0.892, respectively. The p-value of the Plus-Standard difference in the share of mentors who drop-out from the API program during the experiment is 0.957.

[6] Similarly, panel B in Appendix Table C.1 shows balance with respect to the assignment of the treatment for the characteristics of those community instructors who were active in the evaluation schools as of spring 2016.

3 Standard versus Plus

3.1 Main Program Impacts

The causal effects of the two API treatments can be estimated via the following regression model

$$Y_i = \beta_0 + \beta_1 Standard_{j(i)} + \beta_2 Plus_{j(i)} + X_{i,j}\gamma + u_i \,, \tag{1}$$

where Y_i is an outcome of student i in school j, which is observed by the end of the second consecutive school year since the assignment of the mobile mentors to the communities. The two variables $Standard_{j(i)}$ and $Plus_{j(i)}$ take a value of one if school j is assigned to either the API Standard or the API Plus group, respectively. Both variables take a value of zero if school j is assigned to the control group. The vector $X_{i,j}$ consist of individual and community–level characteristics. Individual-level characteristics include gender and an indicator for whether the child speaks an indigenous language. Community-level characteristics include the strata indicators that account for the block randomization design, a set of indicators for survey weeks and survey routes, as well as the school average scores in math, Spanish, and science as measured in 2013.[7] Standard errors are clustered at the school level so as to account for correlated shocks that vary at the same level as the treatment indicators.

Table 2 displays the main estimates of the average impact of the API program. When compared to the control group, children who are enrolled in a school that received the Standard API model increased their reading scores by 0.13 standard deviations, as opposed to a 0.32 standard deviations improvement for those attending schools served by the API Plus modality. While the estimated effect of the API Standard on reading scores is not statistically different from zero, we can reject the hypothesis of a null effect of API Plus at the 99 percent confidence level. Quantitatively, the API Plus effect is approximately 2.5 times higher than the API Standard effect. This difference is statistically different from zero (p-value =0.043).[8]

We find similar patterns when we look at math scores. On the one hand, the API Standard had a small (0.06 standard deviations) and not statistically significant effect. On the other hand, we find a sizable effect of API Plus, with an estimated treatment effect of 0.24 standard deviations. This effect is statistically significant, and we can reject the hypothesis that the two treatment arms have the same effect at the 95 percent confidence level.

We also find that the assignment of the API Standard produces a small improvement of 0.07

[7] During the data collection, a few schools had to be surveyed on a second or third visit due to adverse weather conditions or high political instability. The inclusion of survey weeks and survey routes indicators in the vector $X_{i,j}$ in (1) is meant to control for the different timing of the survey in these communities.

[8] In Appendix Table C.4 we test for possible changes in classroom composition during the experimental period resulting from grade retention, attrition, inflows of students from outside the CONAFE system, and inflows of students from other CONAFE schools. We do not find any difference among the treatment arms in any of the these outcomes.

Table 2: Average Program Impacts

	Reading Score	Math Score	Socio-emotional Score	Transition to Secondary School (6th grade)
API Standard	0.126	0.056	0.071	0.074
	(0.077)	(0.075)	(0.087)	(0.065)
API Plus	0.315	0.237	0.199	0.127
	(0.083)	(0.089)	(0.087)	(0.069)
H0: Standard=Plus	0.043	0.043	0.178	0.449
Mean Control Group	0.000	0.000	0.000	0.620
SD Control Group	1.000	1.000	1.000	0.487
Observations	1044	1044	1045	468
Clusters	224	224	224	182

Notes: This table shows the estimates of the two API modalities: API Standard and API Plus. Each coefficient represents the estimated effect of the program relative to the control group as depicted in the regression model (1). Reading, Math, and Socio-emotional scores are standardized with respect to the mean and the standard deviation of the control group. One child has missing information on reading and math scores, hence the difference in the number of observations between the first two columns and the third column. For a detailed descriptions of the reading, math and socio-emotional scores used in this table, see Appendix A.2. The last column is a linear probability model for the sub-set of students enrolled in the sixth grade in spring 2016, where the transition to Secondary School is an indicator variable of whether or not students enroll in a secondary school during the 2016–2017 school year, which is computed from administrative school records (see Appendix A.1). Standard errors reported in parentheses are clustered at the school level.

standard deviations in a child's socio-emotional skills, although the estimated effect is not statistically different from zero. On the contrary, the API Plus group generates a sizable improvement in the socio-emotional score of 0.2 standard deviations. This larger effect for the API Plus is consistent with qualitative evidence documenting that mentors in the API Plus group shared more effective strategies to best deal with children's emotions during the bimonthly peer-to-peer sessions.

The last column in Table 2 reports the estimated effects on the transition to secondary school. In this case, the outcome is a dummy variable for whether or not children enrolled in secondary school for the subset of children who were enrolled in 6th grade during the school year 2015–16.[9] The API Plus treatment increases the probability of a child's enrolling in a lower secondary school by 12.7 percentage points. Although marginally significant, the effect is quantitatively sizable, as it represents a 20 percent increase in the share of students who transit to secondary school for the API Plus group relative to the mean in the control group. As for the rest of the outcomes displayed in Table 2, the effect of the API Standard

[9]The estimates reported in Appendix Table C.4 document no program effects on grade repetition and attrition, which suggest that conditioning on grade attainment is not problematic in our context.

Table 3: Average Program Impacts by Sub-domains of the Reading and the Math Scores

Panel A: Share of Correct Reading Answers by Sub-Domain

	Letter Name	Initial Name	Initial Sound	Word Recogn.	Word Reading	Read Comprehen.	Listing	Dictation
API Standard	0.103	0.006	0.122	0.129	0.075	0.118	-0.004	0.129
	(0.086)	(0.079)	(0.085)	(0.076)	(0.072)	(0.073)	(0.078)	(0.083)
API Plus	0.240	-0.019	0.042	0.318	0.197	0.321	0.123	0.378
	(0.084)	(0.082)	(0.073)	(0.081)	(0.079)	(0.084)	(0.084)	(0.076)
H0: Standard=Plus	0.180	0.771	0.343	0.039	0.183	0.023	0.094	0.005
Mean Control Group	-0.000	-0.000	0.000	-0.000	-0.000	0.000	-0.000	-0.000
SD Control Group	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Observations	1044	1044	1044	1044	1044	1044	1044	1044
Clusters	224	224	224	224	224	224	224	224

Panel B: Share of Correct Math Answers by Sub-Domain

	Number Identif.	Number Discrim.	Missing Number	Add	Subtract	Problem Solving	Shape Recogn.
API Standard	0.094	0.036	0.099	0.011	0.061	-0.051	0.022
	(0.082)	(0.081)	(0.076)	(0.068)	(0.072)	(0.073)	(0.081)
API Plus	0.259	0.201	0.204	0.215	0.111	0.116	0.099
	(0.091)	(0.089)	(0.089)	(0.072)	(0.068)	(0.082)	(0.098)
H0: Standard=Plus	0.095	0.103	0.218	0.008	0.500	0.046	0.396
Mean Control Group	-0.000	0.000	0.000	0.000	0.000	-0.000	0.000
SD Control Group	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Observations	1044	1044	1044	1044	1044	1044	1044
Clusters	224	224	224	224	224	224	224

Notes: This table shows the estimates of the two API modalities on each sub-domain of Reading and Math scores. Each coefficient represents the estimated effect of the program relative to the control group (see the regression model in equation (1)). The scores in each sub-domain are standardized with respect to the mean and the standard deviation of the control group. For a detailed descriptions of the reading and math scores as well as their sub-domains, see Appendix A.2. Standard errors in parentheses are clustered at the school level.

(an increase of 7.4 percentage points) is not statistically different from zero.

In Table 3 we report the results by sub-domains of the reading scores (panel A) and math scores (panel B). While the estimates are erratic and not statistically significant for the API Standard modality, the API plus intervention seems to increase students' proficiency in reading across various domains (familiar-word reading, reading comprehension, and dictation). There are no improvements in sound-related questions (initial sound and initial name), which is probably due to the fact that children whose first mother tongue is an indigenous language might struggle to capture Spanish alphabet pronunciation. For math scores, the API Plus modality seems particularly effective on numbers' identification and discrimination as well as additions. There are no improvements in more involved tasks such as problem solving and shape recognition.

Table 4: Quantile Program Impacts

Panel A: Reading Score	10th Perc.	30th Perc.	50th Perc.	70th Perc.	90th Perc.
API Standard	0.181	0.135	0.159	0.061	0.008
	(0.179)	(0.133)	(0.093)	(0.056)	(0.039)
API Plus	0.288	0.306	0.295	0.094	0.065
	(0.212)	(0.154)	(0.098)	(0.057)	(0.048)
H0: Standard=Plus	0.673	0.239	0.199	0.627	0.283
Mean Control at Percent.	-1.673	-0.496	0.377	0.704	1.031
Observations	1044	1044	1044	1044	1044
Clusters	224	224	224	224	224

Panel B: Math Score	10th Perc.	30th Perc.	50th Perc.	70th Perc.	90th Perc.
API Standard	0.137	0.044	0.073	0.007	0.044
	(0.175)	(0.140)	(0.115)	(0.085)	(0.089)
API Plus	0.312	0.270	0.213	0.152	0.155
	(0.143)	(0.149)	(0.125)	(0.104)	(0.097)
H0: Standard=Plus	0.313	0.151	0.306	0.180	0.231
Mean Control at Percent.	-1.346	-0.555	0.111	0.690	1.230
Observations	1044	1044	1044	1044	1044
Clusters	224	224	224	224	224

Panel C: Socio-Emotional Score	10th Perc.	30th Perc.	50th Perc.	70th Perc.	90th Perc.
API Standard	-0.058	0.096	0.041	0.058	0.136
	(0.103)	(0.097)	(0.106)	(0.164)	(0.202)
API Plus	0.022	0.120	0.191	0.270	0.147
	(0.114)	(0.118)	(0.098)	(0.168)	(0.186)
H0: Standard=Plus	0.517	0.835	0.212	0.186	0.964
Mean Control at Percent.	-1.329	-0.554	-0.088	0.532	1.462
Observations	1045	1045	1045	1045	1045
Clusters	224	224	224	224	224

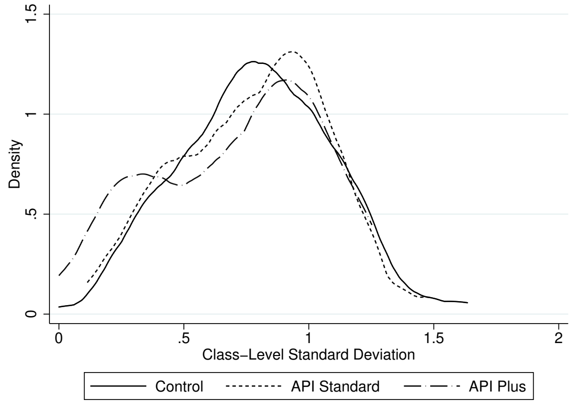
Notes: This table shows the estimates of quantile regressions for reading, math, and socio-emotional scores at different quantile levels (columns). The omitted category refers to the control group. For a detailed descriptions of the reading, math and socio-emotional scores used in this table, see Appendix A.2. Standard errors in parentheses are clustered at the school level (Parente and Santos Silva, 2016).

3.2 Further Evidence

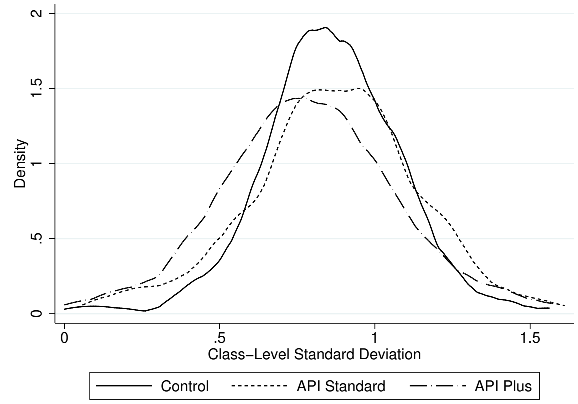
Table 4 presents treatment effect estimates at various percentiles of the test score distributions. Results show a more pronounced impact of both API Standard and API Plus on reading and math achievement for pupils who perform relatively worse on those achievement tests (panels A and B). Effect sizes for the API Plus are larger throughout the achievement distribution, although we cannot reject that they are equal to those of the API Standard. Estimates are more erratic for the socio-emotional score (panel C).

These heterogeneous effects may induce a possible reduction in the within-school inequality

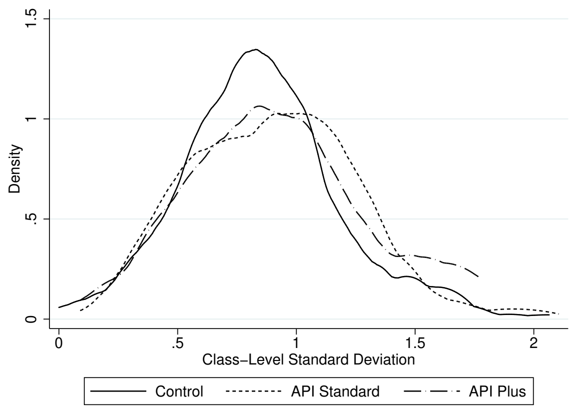
Figure 1: Program Impacts on the School-Level Dispersion of Learning Outcomes



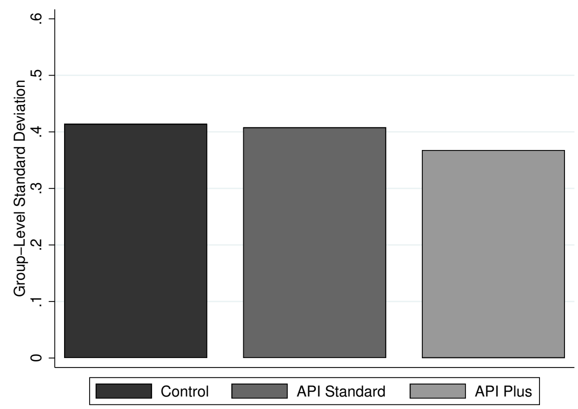
Panel A: Reading Score



Panel B: Math Score



Panel C: Socio-Emotional Score



Panel D: Transition to Secondary

Notes: Panels A, B, and C of this figure show the empirical distributions of the school-level standard deviation in outcomes for the two treatment arms and the control group. For a detailed descriptions of the reading, math and socio-emotional scores used in this table, see Appendix A.2. Panel D plots the standard deviations of the school-average transitions to secondary schooling separately for the two treatment arms and the control group.

of achievement outcomes due to the API Plus. We inspect this possibility in Figure 1, which plots the empirical frequencies of the school-level standard deviation for reading, math, and socio-emotional scores (panels A, B and C, respectively) as well as the standard deviation of the school-level average transitions to secondary schooling (panel D). While the distributions of the API Standard group mimic closely those of the control group, it is clear from the figure that the API Plus generates a decrease in the level of within-school inequality in reading and math skills as well as in the extent of across-school inequality in education attainments. Appendix Table C.3 shows the corresponding OLS results of school-level regressions. The emerging patterns on the effect of the intervention on the level of inequality in schooling achievement outcomes are broadly consistent with those depicted in

Figure 1. While estimates are noisy and relatively small in magnitudes for the API Standard, the API Plus modality leads to a significant decrease in the school-level standard deviation of the cognitive measures by 0.10-0.13 points out of average dispersion levels in the range of 0.78-0.84 points.

Overall, the results presented in this section show that the API intervention had differential impacts according to the training received by the mobile mentors. While the API Standard modality did not significantly boost any of the outcomes of interest, the API Plus modality had sizable average effects on children's cognitive and socio-emotional scores, as well as on schooling attainments. Mentors with enhanced training seem to be particularly effective for lower-achieving children, thereby leading to more homogeneous classroom environments in terms of education achievement.

4 Standard versus Plus: Potential Mechanisms

As mentioned in Section 2.1, the activities of the pedagogical mentors are organized around three main areas of intervention: (i) one-on-one tutoring sessions with academically weaker students, (ii) pedagogical support to teachers, and (iii) parental engagement through periodic home visits and other meetings at school. In this section, we leverage detailed survey information collected in the spring of 2016 to provide direct evidence on the possible channels behind the differential impacts between the API Plus and the API Standard on children's schooling outcomes.

4.1 Parents and Community Instructors

The first two columns of panel A in Table 5 present the average impacts of the two treatments on parental outcomes. The estimated coefficients reported in the first column show that the API Plus modality seems effective in boosting parental expectations about children's educational achievement. The point estimate implies that parents are 9.3 percentage points more likely to expect their child to complete secondary schooling, which represents a 12 percent increase with respect to the sample mean in the control group. The corresponding effect size for the API Standard is very small in magnitude and not statistically different from zero—we can reject the null hypothesis of equal treatment effects at the 95 percent confidence level. A similar asymmetric response emerges when we consider a parental investment index (second column), generated as the principal component of five different measures of educational investments, such as helping with homework and participating in school activities. Parents who have been exposed to the API Plus indeed seem to be significantly more engaged in their children's education.[10]

[10] We also estimate treatment effects of both the API Standard and API Plus treatments for each of the individual measures of the parental investment index. The results are reported in Appendix Table C.6 and they are broadly consistent with the overall index.

Table 5: Standard vs. Plus—Parents and Community Instructors

Panel A: Parental Inputs and Pedagogical Practices

	Parent Expect Complete Sec.	Parental Investment Index	Instructor is Out of the Class	Instructor Keeps Rhythm
API Standard	0.010	-0.070	-0.969	0.066
	(0.036)	(0.091)	(0.799)	(0.030)
API Plus	0.093	0.239	-1.039	0.086
	(0.039)	(0.099)	(0.581)	(0.042)
H0: Standard=Plus	0.044	0.009	0.899	0.639
Mean Control Group	0.755	-0.002	2.374	0.018
SD Control Group	0.431	1.001	7.300	0.133
Observations	1016	963	259	252
Clusters	224	222	209	206

Panel B: Mentors' Interactions with Parents

	Meetings in Last 60 Days	Visits in Last 60 Days	Inform About Child	Advise About Child
API Plus	1.015	0.688	0.105	0.100
	(0.711)	(0.468)	(0.051)	(0.045)
Mean API Standard	5.037	3.039	0.714	0.749
Observations	482	491	354	353
Clusters	123	124	113	112

Notes: This table shows the estimates for the behavioral responses in parental investments, instructors' pedagogical practices, and mentor-parent interactions. The first two columns in panel A show the estimates for parental aspirations and parental investments taken from the household survey. The last two columns in panel A show the estimates for two measures of teaching practices taken from the classroom observation survey. See Appendix A.2 for a detailed description of the two surveys, which were both administered in the spring of 2016. For each regression model in panel A, the omitted category refers to the control group. Panel B shows the estimates for the mentor-parent interactions as reported by the parents in the household survey. For each regression in panel B, the omitted category refers to the API Standard group (the control group does not receive home visits according to the experimental design). Standard errors in parentheses are clustered at the school level.

We next focus on the pedagogical practices of the community instructors, as measured by their time use and different learning activities. We adapt a version of the Stallings classroom snapshot, a rubric for timed observations that has been previously used in Mexico (Bruns and Luque, 2015). The last two columns in panel A of Table 5 report estimates of the effect of the two API modalities using data at the instructor-school level.[11] The estimated coefficients in the third column show that the presence of the mentors in the school leads to a reduction in teachers' unjustified absence by roughly one minute in a representative hour of teaching time in both modalities. Albeit small in absolute terms and noisily estimated, the relative effect size is about half of the mean in the control. The estimates shown in the

[11]The average number of instructors per school is 1.2 in the school year prior to the start of the API program (see panel A in table 1).

fourth column reveal a positive effect of the two API modalities on an indicator variable for whether the instructor is able to keep the rhythm of the class while teaching. The estimated effects are sizable when compared to the very low share of teachers in the control group who are effective in this pedagogical dimension. For both instructors' outcomes, the effects are quantitatively and statistically similar across API modalities.[12]

The asymmetric effects on parental outcomes are consistent with the estimates reported in panel B of Table 5, which show that the API Plus modality improved the quantity (columns 1 and 2) and the quality (columns 3 and 4) of the mentor-parent interactions relative to the API Standard. On average and over a two-month period, mobile mentors in the Plus group meet one time more with parents at school and 0.7 times more at home (sample means in the API Standard group are 5 and 3, respectively) compared to those in the Standard modality, although the effects are noisily estimated. The last two columns of panel B show more-precise estimates on two outcomes: (i) an indicator variable for whether the mentors have informed the parent about his/her child's learning difficulties; and (ii) whether the mentors provide concrete advice to the parent on how to tackle these difficulties. For both outcomes, the effect sizes imply a 14 percent increase in the probability of informing parents with respect to the sample means in the API Standard group.

4.2 Remedial Education

We finally evaluate the role of the tutoring sessions in potentially explaining the differential treatment effects of the API Standard and the API Plus on children's outcomes. As mentioned in Section 2.1, those take the form of individual meetings between each of the mentors and the students outside of regular school hours. The six weakest students in the class are deemed eligible for the tutoring sessions, based on three criteria: (i) the grades in a diagnostic test in Spanish, math, and natural science, (ii) the assessment of the community instructor, and (iii) an ad hoc evaluation applied by the mentor.

We only have information on the first eligibility criterion for all the schools in our sample. We do not know the weight given to each of the three subjects of the test and hence we use the average grade across the three subjects as a predictor for the child's participation in the tutoring sessions. To check this, we restrict the sample to the schools that are assigned to either the API Standard or the API Plus and run a Probit model for the relationship between the (realized) probability of participating in the tutoring sessions and the school-level reverse ranking of students as implied by their average score in the three subjects, with the worst-performing student being ranked first and so on. The estimated marginal effects are plotted in Figure C.1 in the Appendix, which confirm statistically significant drop in the predicted probability of participation in the tutoring sessions for all students who are ranked

[12] In Appendix Table C.5 we complement these instructors' outcomes with additional measures of pedagogical practices computed from the classroom observation survey (see Appendix A.2). The results show erratic patterns with no significant effects of neither API modalities.

Table 6: Standard vs. Plus—Remedial Education Sessions

	Reading Score	Math Score	Socio-Emotional Score
API Standard×Rank Above 7	0.209	0.044	0.163
	(0.122)	(0.117)	(0.147)
API Plus×Rank Above 7	0.436	0.292	0.223
	(0.129)	(0.146)	(0.139)
API Standard×Rank Below 7	0.037	-0.003	0.045
	(0.098)	(0.097)	(0.099)
API Plus×Rank Below 7	0.262	0.228	0.196
	(0.098)	(0.105)	(0.105)
Rank Below 7	-0.088	-0.180	0.100
	(0.100)	(0.102)	(0.113)
H0: Standard=Plus (Below 7)	0.045	0.035	0.185
H0: Standard=Plus (Above 7)	0.079	0.087	0.708
Observations	1044	1044	1045
Clusters	224	224	224

Notes: This table shows the estimates for the linear regression model depicted in equation (1) once we interact the treatment assignment dummies with indicators of whether a child is among the six lowest-performing children in the class on the diagnostic test (Rank Below 7 and Rank Above 7), which is one of the main determinants for participation in the one-on-one remedial sessions with the mentors (see Appendix Figure C.1). Reading, math, and socio-emotional scores are standardized with respect to the mean and the standard deviation of the control group. See Appendix A.2 for a detailed description of the outcome variables. Standard errors in parentheses are clustered at the school level.

seventh or above in the average grade for the three subjects.[13]

We consider a simple variant of the regression model depicted in equation (1) in which the API Standard and API Plus categories are interacted with indicator variables for whether each child is among the six weakest students in the school—i.e. the reverse rank is smaller than 7.[14] The estimates for the reading and the math scores are reported in the first two columns of Table 6, and they confirm the larger and significant effect of the API Plus modality when compared to the API Standard. Importantly, the magnitude of the differences between API modalities is very comparable between students who are ranked sixth or below—and hence who are more likely to be targeted by the remedial education sessions—and for those who are ranked seventh or above. Consistent with the evidence reported in panel C of Table

[13]We observe a similar drop when conducting the same exercise for each subject separately (results available upon requests).

[14]Individual and household characteristics are balanced for the sub-samples of children who are below and above rank 7 (see Tables C.7 and C.8 in the Appendix).

4, the estimates are more erratic for the socio-emotional score and no clear patterns emerge.

Although we cannot directly isolate the individual effect of the remedial education sessions from the direct effect of each program modalities, we can bound its relative contribution in explaining the differential effect of API Plus with respect to API standard on student outcomes.[15] The estimates displayed in Table 6 suggest that there is no differential effect in the relative effectiveness of the two training modalities between children who perform relatively well (i.e. above rank 7) and the others in the class (i.e. below rank 7). The estimates reported in Table 4 document that the differential impact of API Plus versus API Standard decreases over the quantiles of the distribution of reading and math scores. Taken together, these two pieces of evidence suggest that the remedial education sessions cannot explain the differential impacts between the API Plus and the API Standard documented in section 3.1.[16]

In summary, while remedial education sessions or pedagogical support from the mentors are unlikely to explain the asymmetric impacts of the two API modalities, the increased mentor-parent interactions, which may have triggered higher parental aspirations and investments in children, can potentially explain these results.

5 Program Scale-Up and Persistence of the Impacts

Over the summer of 2016, after learning about the experimental impacts of the program, CONAFE decided to expand the API Plus model to all its primary schools, including the 224 schools that were part of the original evaluation sample. The Plus modality became the only modality in place for the API intervention thereafter. Schools were assigned a score between one and four, with one denoting the highest priority level. The scores are based on a combination of criteria that include school performance in the national learning assessment, whether the school had received an API during the period between 2008 and 2015, the level of marginalization of the community where the school was based, and whether the community was targeted by an anti poverty program. In this section, we test whether the effects observed during the experimental evaluation persist at scale.

5.1 Program Impacts at Scale

We observe the schools that were part of the evaluation sample up two years after the end of the experiment. By the spring of 2018, the years of exposure of schools to the API Plus range from zero to four depending on both the original experimental assignment in 2014

[15]The effects of remedial education cannot be separately identified from the bundle of the API program in our context since (i) we don't know which children in the control group would have had access to the remedial sessions had they been assigned a mentor and (ii) the assignment to the remedial sessions was not separately randomized within the Standard and Plus groups. See Appendix B for a more detailed discussion.
[16]The correlation between the school-level rankings as implied by the average diagnostic test and the math and reading scores is 0.51 and 0.52, respectively.

as well as on the 2016 and 2017 CONAFE reassignment. Approximately 60 percent of the schools in any of the treatment arms and the control group of the experiment received the API Plus program for at least one year during the national roll out.

One may be concerned that comparing children with respect to their total exposure to the intervention may confound differences in unobservable characteristics between schools. For this reason, we exploit the original treatment assignment in our experiment in 2014 to predict the school-level years of exposure to the API Plus program during the roll out. The schools that were originally assigned to the API Plus treatment can be exposed to the program from two to four years, while schools that were assigned to either the original control group or the API Standard group can be exposed to the API Plus program for up to two years (during the program roll out).[17]

We implement this research design using the following regression model

$$ExpPlus_{j(i)} = \psi_0 + \psi_0 Plus_{j(i)} + \psi_1 Standard_{j(i)} + X_{i,j}\psi_2 + \nu_i \tag{2}$$

$$Y_i = \gamma_0 + \gamma_1 ExpPlus_{j(i)} + \gamma_2 Standard_{j(i)} + X_{i,j}\gamma_3 + \eta_i \,, \tag{3}$$

which we estimate via an instrumental variable (IV) estimator. The coefficient γ_1 is our parameter of interest, which represents the marginal effect on the outcome Y_i of increasing the API Plus exposure ($ExpPlus_{j(i)}$) by one additional year.

In panel A of Table 7 we show that, on average, after one year of the program scale-up, children in the original API Plus schools are 18 percentage points more likely to enroll in secondary school relative to children in the original control schools. Within the same time horizon, children in the original API standard schools are 10 percentage points more likely to enroll in secondary school, but the effect is not statistically different from zero. Because both the original control schools and the original API standard schools started to participate in the API Plus program after the scale-up initiative, these reduced-form effects suggest—in line with the results shown in Table 2—that API Plus is the effective format.

By predicting years of exposure based on the initial randomization, the second-stage estimates in panel A of Table 7 show that an additional year of the API program increases the probability of children's transitioning into secondary school by 9.4 percentage points. The magnitude of this effect closely resembles the experimental estimate of the API Plus displayed in the last column of Table 2.[18]

[17] The first-stage estimates are reported in the Appendix Table C.9. They confirm that the original treatment assignments predict the total number of years of API exposure, with an average of approximately two additional years relative to the control group schools.

[18] The main experimental estimate of API Plus in Table 2 is for the sample of sixth graders in the 2015–2016 school year (two years of API Plus exposure). We find that the API Plus increases the probability of transitioning to secondary school by 6.3 percentage points for the sample of sixth graders in the 2014–2015 school year (one year of API Plus exposure). The pooled estimated effect is 9 percentage points.

Table 7: Program Impacts at Scale

Panel A: Transition to Secondary School (Fall 2017)

	Reduced-Form	IV	OLS
Original API Standard	0.100	0.121	0.115
	(0.062)	(0.066)	(0.066)
Original API Plus	0.185		
	(0.066)		
Years of Exposure to API Plus		0.094	0.087
		(0.033)	(0.028)
Observations	625	625	625
Clusters	207	207	207

Panel B: School is Open (Fall 2018)

	Reduced-Form	IV	OLS
Original API Standard	0.133	0.146	0.152
	(0.057)	(0.057)	(0.053)
Original API Plus	0.152		
	(0.061)		
Years of Exposure to API Plus		0.076	0.083
		(0.029)	(0.022)
Observations	224	224	224

Notes: This table shows the estimates for the model in (2)–(3). In panel A we focus on the probability a student in 6th grade enrolling in a secondary school in the fall of 2017. In panel B we focus on the probability of a school in our original experiment being open by the fall of 2018. The first column shows the reduced-form estimates for both outcomes with respect to the original treatment assignments in our experiment. The second column shows the IV estimates for the years of total API exposure, when we use the original treatment assignments as excluded instruments. Finally, the third column shows the OLS estimates. Standard errors (in parentheses) are clustered at the school level in panel A.

5.2 Challenges and Mechanisms of Scalability

Based on the insights of recent work (Banerjee et al., 2017; Muralidharan and Niehaus, 2017; Al-Ubaydli et al., 2019), the experimental evaluation under study has many features of an "at scale" intervention. First, the field experiment was implemented by the same agency that was in charge of the scale-up. Second, the implementing agency and the research team designed the Plus modality bearing in mind the financial and human resources constraints at scale. Third, the overall scale of the API program operation remained the same before, during, and after the experiment. Finally, the relatively large units of randomization (schools-communities) allow our experimental estimates to be robust to possible local spillover effects, and for this reason to be informative about the possible scale-up of the program.[19]

There are a few implementation details of the experiment that may threaten its scalability. In particular, the threshold number of enrolled students to keep the school open was reduced from six to three as a way to minimize sample attrition during the experiment (see section 2.2). However, the original requirement of six enrolled students was restored during the national roll out. For this reason, a threat to the success of the program at scale may come from the possible school closures following the API assignment. If instead the API assignment increases the probability of the school being open, this mechanism could bolster the scalability of the program.

We test this hypothesis using data on school closures through the follow-up survey conducted in the fall of 2018 (two years after the scale-up initiative and four years after the inception of the experiment). We present the results in panel B of Table 7. In this case, the reduced-form estimates show that schools in both the original API standard group and API plus group, after four years, are more likely to remain open in the fall of 2018 (+13 percentage points and +15 percentage points). This result suggests that the API assignment during the experiment created incentives for schools to remain open in the medium run.

One possible interpretation of these effects is that changes in educational aspirations and parental investments as reported in section 4 can generate spillover effects within a community, positively affecting the destiny of a school (List et al., 2019). For instance, the parents' association can ultimately decide whether to prevent permanent school closures irrespective of the statutory enrollment requirements (see section 2.1). Increased parental involvement in the school activities (see Appendix Table C.6) may also have a monitoring effect on the community instructor thereby avoiding temporary school closures due to sporadic absences.

The IV estimates imply that an additional year of API exposure leads to a 7.6 percentage points increase in the probability of a school's remaining open. This is a relatively large effect to the extent that 30 percent of the schools in the original control group were found to be closed by the fall of 2018.

5.3 Persistence of Program Impacts

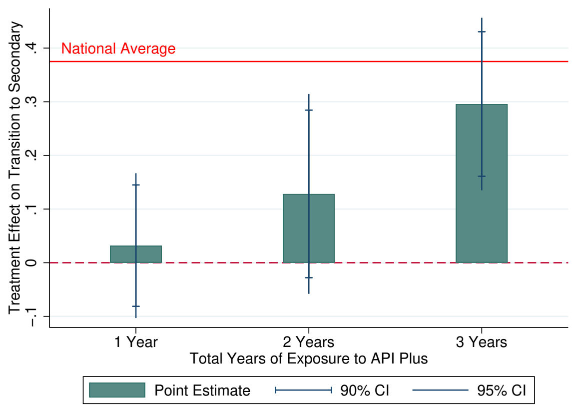
The OLS estimates of the regression model in (3) are remarkably similar to the corresponding IV estimates in both panels of Table 7, suggesting that the scale-up initiative did not select schools based on unobserved factors that determine our outcomes of interest. We leverage this last result to provide additional evidence of the exposure effects of the program. In particular, we study the differential API Plus exposure across schools after the program scale-up.

Panel A in Figure 2 shows that the probability of transitioning to secondary school is increasing in the years of exposure to the program. The exposure effect of the API Plus modality goes from 3.2 percentage points after one year of exposure, to 30 percentage points

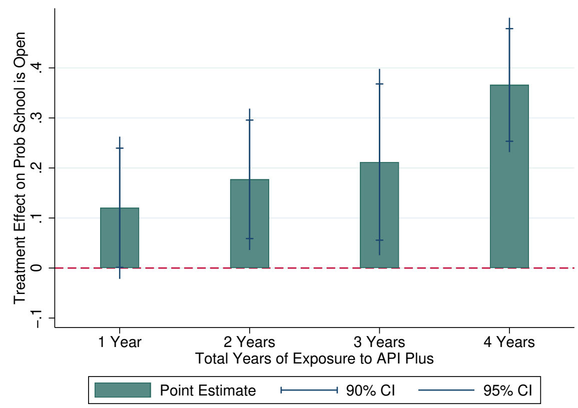
[19] There is always one school per community in our sample.

Figure 2: The Effects of the Length of Program Exposure

Panel A: Transition to Secondary School



Panel B: Probability of School Being Open



Notes: The figure shows the differential effects of API exposure through the program national scale-up on the probability of transition to secondary school in the fall of 2017 (panel A), on the probability of the schools to remain open in the fall of 2018 (panel B). We divide the schools based on the API exposure from 2014 to 2018 into four categories: schools in the original control group with no exposure (omitted category), schools in the control group with 1 or 2 years of exposure, schools in either one of the two treatment arms with 2 years of exposure, and school in the original treatment arms with 3 or more years of exposure.

after three years of exposure. We believe this result is quantitatively sizable, as it implies that a three-year exposure to the API Plus program can effectively close the enrollment gap between urban and rural secondary schools. To further illustrate this important point, the red, solid line in Figure 2 represents the difference in the secondary school enrollment rates between the national average (95 percent) and the schools with zero years of API Plus exposure in our sample (58 percent).

The probability of a school being open by fall 2018 is also increasing in the years of exposure to the program. Panel B in Figure 2 shows that the probability of a school being open increases by 12 percentage points after one year of exposure, and by 37 percentage points after four years. This result implies that, in our sample, no school closed in Fall 2018 after receiving four years of API Plus program.

Panels A and B of Appendix Figure C.2 document positive exposure effects of the API program on the reading and math test scores collected during the follow-up survey of the fall of 2018. However, these last results are affected by the differential school closures that occurred over the same period (see panel B of Figure 2), and for this reason are hard to interpret. In the the school year 2016–2017 only two schools were permanently closed at the beginning of the program roll out. Therefore, the sample of sixth graders for which we observe the transitions into secondary school in 2016–2017 (see panels A in Table 7 and in Figure 2) is not affected by differential attrition.

Overall, our results highlight the benefits of prolonged exposure for the program beneficiaries, which contrasts with the short-time span of the original design of the intervention (two years). After three years of exposure, six graders from disadvantaged rural areas in Chiapas are as likely to enroll in secondary school as children from urban Mexico. In addition, four years of API Plus exposure completely offset the probability of school closures, substantially enhancing the educational opportunities for children in these disadvantaged communities.

6 Conclusion

We provide evidence on both experimental and at scale impacts of a mobile mentor program implemented in highly marginalized areas of Mexico. The program is particularly effective when augmented with an extra week of hands-on training on foundational skills and peer-to-peer sessions. Parental aspirations and investments seem to be the main channel through which the program affects children's outcomes.

This result provides new insights into the role of parents in shaping the success of education interventions targeted at school-age children, building upon the previous evidence on the key role of parents during early childhood (Heckman et al., 2010, 2013; Fryer et al., 2015; Chaparro et al., 2020). More broadly, our findings suggest that a scalable intervention in a low-resource environment can effectively contribute to closing the gap between children in urban and rural communities.

Finally, our study demonstrates the benefits of tailoring educational interventions in disadvantaged environments to the local context. The mobile mentors in our setting have first-hand experience as community instructors, and they are familiar with the local language as well as with the local social norms. This specific aspect of the intervention with enhanced training may be one of the key driver for the increased engagement of parents. These findings have potential policy implications for the various educational programs that enlist recent college graduates to teach in disadvantaged communities, such as the Teach for America (TFA) program and its variants recently implemented in a variety of countries.

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Appendices

A Data Description

A.1 Administrative Data

School census. The Ministry of Education runs a school census (*Formato 911*) at the beginning and at the end of each school cycle that covers all public schools in Mexico. In the case of CONAFE schools, the census asks the school representative about the number of students enrolled in every grade and whether they are new students or repeaters. Additional information includes the number of instructors and the number of classrooms per school. Information from the 2013 Census is used to construct the baseline school variables that are displayed in panel A of Appendix Table 1. Census data for the years 2016 and 2017 are used to track the schools in the evaluation sample during the scale-up.

Standardized test scores. Between 2007 and 2013, all Mexican students in grades 3 through 9 were required to take a standardized test, the ENLACE (*Evaluación Nacional de Logro Academico en Centros Escolares*). The test was administered by external proctors at the end of each academic year, and it assessed student knowledge in three areas: math, Spanish, and, starting in 2008, a third subject that rotated between science, ethics/civics, history and geography. We use the school-level average of the 2012 data to construct the strata for the school-level randomization and school-level averages for 2013 as controls in all our regression models.

CONAFE records. We link the enrollment records of the sixth graders in our sample across the first grade of secondary education (7th grade) both in the CONAFE and the regular public education (SEP) system during the following academic year. Individual transitions computed in the school years 2016–2017 are reported in Table 2 and Figure 1, while transitions computed in the school year 2017–2018 are reported in Table 7 and in Figure C.2.

We use student-level longitudinal information for the CONAFE primary schools in Table C.4 to construct various measures of school-level changes in student composition: whether the student has to repeat a grade in school year 2015-2016, attrition from the CONAFE school system in Chiapas between the school years 2014-2015 and 2015-2016, and whether in 2015-2016 the student attends the same school as in 2014-2015. The number of observations drops from 1045 to 1019 due to incomplete school identifiers (CURP) for 26 students.

All students in the CONAFE system, irrespective of whether they receive the API program or not, have to undergo a diagnostic test at the beginning of each school year. The test covers three subjects: math, Spanish, and natural science. The score for each subjects ranges between 5 and 10. We use the individual level average across the three subjects in the diagnostic tests at the beginning of the school year 2014–2015 to construct the student ranking within each CONAFE school displayed in Figure C.1. From that variable, we construct the indicator variables "Rank Above 7" and "Rank Below 7" reported in Table 6, which proxy for the individual eligibility to the one-on-one remedial education sessions.

CONAFE also collects information about their instructors and mobile mentors, such as age, gender, education attained, overall experience and tenure in the school. These characteristics are measured in the year 2013 and are reported in panels A and C of Table C.1.

A.2 First follow-up survey in 2016

The first round of data collection took place in the spring of 2016 in the 224 schools that form part of the evaluation sample. It entails the following array of survey modules and measurement tools.

Measures of Cognitive Achievement. The reading score reported in Tables 2, 4, 6, and 7 is given by the latent factor of an exploratory factor analysis of the following eight domains: 1) letter name, 2) initial name, 3) initial sound, 4) word recognition, 5) word reading, 6) reading comprehension, 7) listing, 8) dictation. The math score reported in Tables 2, 4, 6, and 7 is given by the latent factor of an exploratory analysis of the following seven domains: 1) number identification, 2) number discrimination, 3) missing number, 4) addition, 5) subtraction, 6) problem solving, 7) shape recognition. The individual components of the math and reading score are reported in Table 3. An orthogonal rotation is applied before standardizing each factor with respect to the mean and the standard deviation in the control group.

Measures of Socio-Emotional Development. The household survey contains a set of measures of behavioral problems reported by the caregivers of the children in our sample. The socio-emotional score reported in Tables 2, 4, 6 is the sum of the following thirty two items on how often the child displays a given emotion/behavior (1 = Never, 2 = Sometimes, 3 = Always).

1. Has serendipitous mood changes
2. Feels or complains that nobody loves him/her
3. Is tense or nervous
4. Lies or cheats
5. Is scary or anxious
6. Talks and argues too much
7. Has difficulties in focusing on a specific activity for an extended amount of time
8. Gets easily confused. It seems that his/her head is in the clouds

9. Threatens or is mean with other children
10. Tends to challenge parental authority
11. Does not feel guilty after a bad dead
12. Does not get along with other children
13. Is impulsive or act "fast" without thinking
14. Feels has inferiority issues
15. Has no friends
16. Has difficulties to let go certain thoughts
17. Is hyper-active
18. Has a bad temper, or is irascible
19. Looses easily his/her temper
20. Feels unhappy, sad, or depressed
21. Is shy, does not socialize with others
22. Breaks objects on purpose
23. Is too attached to the adults
24. Cries too much
25. Demands a lot of attention
26. Is too much dependent on others
27. Afraid of other people's judgement
28. Tends to be in bad company
29. Reserved, keeps things for himself/herself
30. Worries about every thing
31. Misbehaves at school
32. Does not respect the instructor

Household Survey. Since surveying the universe of households was not feasible given the budget of the evaluation, a random sample of five households was selected within a 5 kilometer radius from each school in our sample. Basic information on both the household module respondent and household characteristics is reported in panel B of Table C.1.

The household module also collected information on parents' expectations and investment toward children's education as well as measures of homework supervision, interactions with community instructors, time spent on a number of school-related activities, and number of books at home. The parental expectation variable reported in panel A of Table 5 takes the value of one if the respondent expects her/his child to complete upper secondary education or higher, and zero otherwise. The parental investment index also reported in panel A of Table 5 is given by the latent factor of an exploratory factor analysis of five components: (i) an indicator variable for whether the caregiver helps with homework, (ii) participation in school fund-raising activities (school activities 1), (iii) participation in other school-related activities (school activities 2), (iv) the frequency of meetings with the child's instructor, and (v) whether the child participates in any academically-related activities outside the school hours. The individual components are reported in Table C.6. The factor is obtained with Bartlett's method, where the factor scores highly correlate with its own factor and not with others.

Finally, the household module collected several questions on both the quantity and the quality of parents' interactions with the mentors for those households that were assigned to either the API Standard group or the API Plus group. This information is used to construct the four variables reported in panel B of Table 5.

Classroom Observation Survey. We measure time use and different learning activities of community instructors as well as their ability to keep students engaged using an adapted version of Stallings classroom snapshot—a rubric for timed observations that has been previously used in Mexico (Bruns and Luque, 2015). An observer scores the instructor's effective use of 15 different activities over the course of a full one-hour lesson, with snapshots every three minutes. Each activity was scored between 1 and 4. In every snapshot the external observer reports whether the instructor is present in the classroom or not. Given the nature of the API intervention and the multi-grade context, the tool was adapted to capture the instructor's ability to use materials and keep the rhythm of the class.

The information included in this survey is used for the two outcome variables of the community instructors used in the last two columns of panel A in Table 5. The teaching score reported in Table C.5 is constructed as the sum of the individual scores in each three-minute snapshot for five key aspects of pedagogy: (i) reading, (ii) showing, (iii) answering questions, (iv) memorizing, and (v) giving homework. The material score also reported in Table C.5 is constructed as the sum of four indicator variables: (i) whether the instructor uses any book to explain a given topic, (ii) whether the instructor uses any material from the community to explain a given topic, (iii) whether drawings and other students' artworks are exposed in the classroom, and (iv) whether charts and maps are exposed in the classroom.

Instructors were also asked standard questions on their socio-demographic characteristics, education, experience and, if they were in the treatment group, their relationship with the mentors. Those are reported in panel A of Table C.1.

Mentors' Survey. Since the pedagogical mentors were not located in the communities on a continuous basis, the survey firm interviewed them by an end-of-year evaluation session. Their characteristics are reported in Table C.2 for the subset of the mentors who reported working in different schools from those they were initially assigned to.

A.3 Second follow-up survey in 2018

In the fall of 2018, we conducted a second round of data collection to evaluate the medium-term impacts of the intervention. As mentioned in Section 5, a significant number of schools in the original evaluation sample were closed during the time between the two data collections. Children's outcomes were measured in 186 schools out of the 224 original schools that were part of the evaluation. Using this information, we have computed the indicator function for whether the school is open that we used in Table 7 and Figure 2. Both the reading score and the math score were measured for children in grades three through six and they are reported in Figure C.2.

B Standard versus Plus: The Role of the Remedial Education Sessions

In this section, we provide more details on the interpretation of the results reported in Table 6. The goal is to decompose the heterogeneity of the effects above and below rank 7 into: (i) the direct effect of the two of API modalities for low-achieving and high-achieving children; and (ii) the effect of the remedial education sessions on children. For this reason, we first allow the direct effect of the API Standard and API Plus modalities to vary depending upon the children's rank in the class ($Below7_i$), independently of the remedial sessions. Second, we allow the effect of the remedial education sessions ($Remedial_i$) to vary between the two API modalities.

The full-interacted model takes the following form:

$$\begin{aligned}
Y_{i,j} =& \beta_{0,0}(1 - Below7_i) + \beta_{0,1} Below7_i + \\
&+ \beta_{1,0}(1 - Below7_i) * Standard_j + \beta_{2,0}(1 - Below7_i) * Plus_j + \\
&+ \beta_{1,1} Below7_i * Standard_j + \beta_{2,1} Below7_i * Plus_j + \\
&+ \beta_3 Remedial_i * Standard_j + \beta_4 Remedial_i * Plus_j + u_{i,j} \,,
\end{aligned} \tag{B.1}$$

where the coefficients ($\beta_{1,0}, \beta_{2,0}$) and ($\beta_{1,1}, \beta_{2,1}$) represent the effect of API Standard and API Plus for high-achieving and low-achieving children, respectively. The coefficients (β_3, β_4) represent the effect of the remedial education sessions for the API Standard and the API

Plus, respectively. These latter effects cannot be identified in our context as we don't have variation in $Remedial_i$ for the control group and the assignment to the remedial sessions was not randomized within the Standard and Plus groups. Still, we can decompose the differential impact of API Plus versus API Standard between children above or below rank 7 as follows:

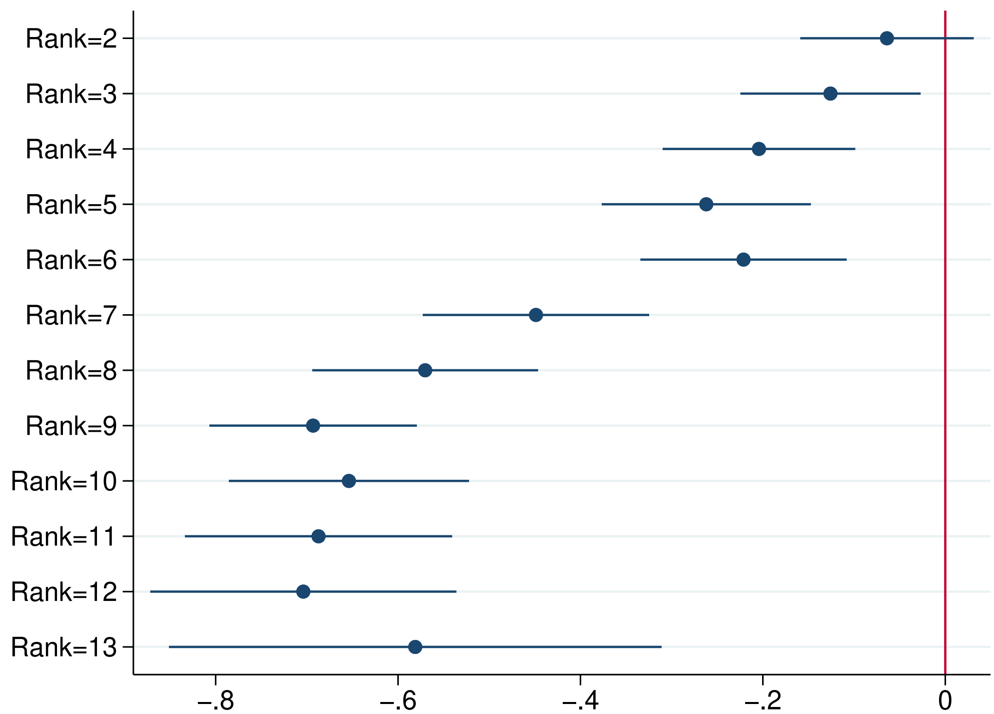
$$
\begin{aligned}
&(E[Y|Plus = 1, Below7 = 1] - E[Y|Standard = 1, Below7 = 1])-\\
&(E[Y|Plus = 1, Below7 = 0] - E[Y|Standard = 1, Below7 = 0]) =\\
&\underbrace{\underbrace{(\beta_{2,1} - \beta_{1,1})}_{\substack{\text{Relative Effect of Plus vs Standard}\\ \text{Below Rank 7}}} - \underbrace{(\beta_{2,0} - \beta_{1,0})}_{\substack{\text{Relative Effect of Plus vs Standard}\\ \text{Above Rank 7}}}}_{\geq 0} + \underbrace{\underbrace{(\beta_4 - \beta_3)}_{\substack{\text{Contribution of Remedial Sessions on}\\ \text{Plus vs Standard Effects}}}}_{\leq 0} \approx 0\,,
\end{aligned}
\tag{B.2}
$$

The main object of interest in this expression is $\beta_4 - \beta_3$, which represents the unobserved contribution of the remedial education sessions in explaining the observe difference in education outcomes between the effects of API Plus and API standard.

The estimates reported in Table 4 document that the differential impact of API Plus versus API standard decreases over the quantiles of the distribution of reading and math scores. Hence, we can bound the difference between the first two terms of equation (B.2) to be positive $((\beta_{2,1} - \beta_{1,1}) - (\beta_{2,0} - \beta_{1,0}) \geq 0)$. The overall term in (B.2) is assumed to be zero because of the estimated coefficients reported in Table 6. This implies that the remedial educational sessions cannot explain the differential impacts between the API Plus and the API Standard on a child's development $(\beta_4 - \beta_3 \leq 0)$.

C Additional Figures and Tables

Figure C.1: Marginal Effects on the Probability of Being in the Remedial Sessions by Inverted Achievement Ranks

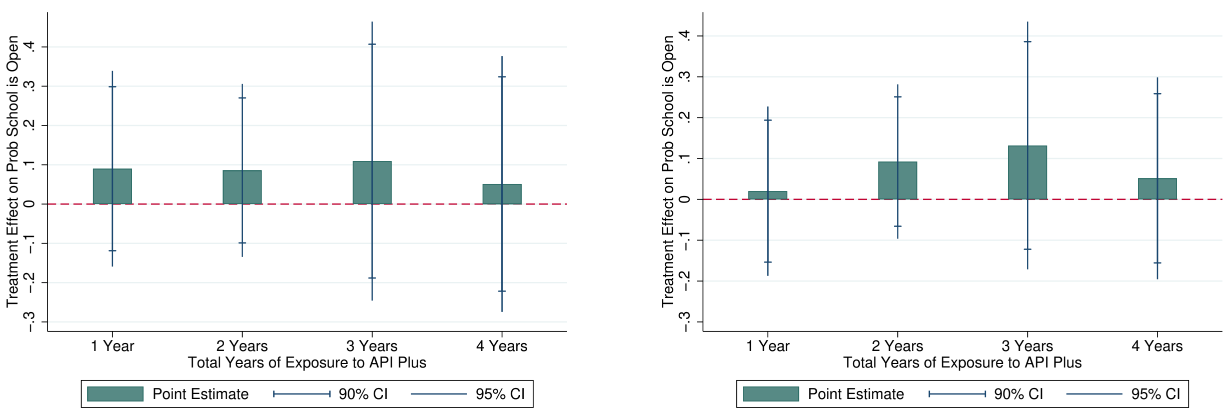


Notes: This figure shows the differential probability of students being assigned to the one-on-one remedial sessions as a function of their within-class relative rank in the diagnostic test. The omitted category is Rank = 1, which represents the least-performing child in the class.

Figure C.2: The Effects of the Length of Program Exposure: EGRA and EGMA

Panel A: Reading Score

Panel B: Math Score



Notes: The figure shows the differential effects of API exposure through the program national scale-up on reading (Panel A) and math scores (panel B). We divide the schools based on the API exposure from 2014 to 2018 into four categories: schools in the original control group with no exposure (omitted category), schools in the control group with 1 or 2 years of exposure, schools in either one of the two treatment arms with 2 years of exposure, and school in the original treatment arms with 3 or more years of exposure. For a description of the reading score and the math score collected in the follow-up survey, see Appendix A.3.

Table C.1: Survey Characteristics and Covariate Balance

Variable	Control Mean (SD)	API Standard Mean (SD)	API Plus Mean (SD)	Difference Std-Ctr (SE)	Difference Plus-Ctr (SE)
Panel A: Community Instructors Characteristics					
Lower than upper second.	0.067	0.062	0.066	-0.002	0.009
	(0.251)	(0.242)	(0.250)	(0.035)	(0.033)
Lower than higher ed.	0.918	0.901	0.908	-0.000	0.002
	(0.276)	(0.300)	(0.291)	(0.044)	(0.040)
Training weeks at baseline	4.515	4.704	4.500	0.128	-0.042
	(1.322)	(1.259)	(1.426)	(0.196)	(0.253)
3rd and 4th grade students	3.655	3.986	3.716	0.346	0.137
	(2.434)	(2.286)	(2.230)	(0.349)	(0.356)
5th and 6th grade students	3.517	3.838	3.507	0.325	0.054
	(2.408)	(2.507)	(2.298)	(0.354)	(0.352)
Panel B: Household Characteristics					
Indigenous Language	0.326	0.366	0.476	0.049	0.142
	(0.469)	(0.483)	(0.500)	(0.065)	(0.077)
Read	0.715	0.686	0.734	-0.031	0.022
	(0.452)	(0.465)	(0.443)	(0.041)	(0.042)
Less than Primary	0.615	0.587	0.584	-0.028	-0.030
	(0.487)	(0.493)	(0.494)	(0.043)	(0.041)
Upper Sec. or Higher	0.015	0.016	0.019	-0.001	0.003
	(0.123)	(0.124)	(0.135)	(0.009)	(0.009)
Oportunidades	0.813	0.807	0.829	-0.003	0.015
	(0.391)	(0.395)	(0.377)	(0.033)	(0.031)
Refrigerator	0.397	0.387	0.373	-0.010	-0.019
	(0.490)	(0.488)	(0.485)	(0.047)	(0.055)
Television	0.692	0.738	0.651	0.048	-0.040
	(0.462)	(0.440)	(0.478)	(0.047)	(0.051)
Car	0.084	0.081	0.063	-0.003	-0.019
	(0.277)	(0.273)	(0.244)	(0.027)	(0.024)
Sewage	0.254	0.253	0.320	-0.003	0.068
	(0.436)	(0.435)	(0.467)	(0.042)	(0.052)
Phone	0.220	0.233	0.204	0.014	-0.014
	(0.414)	(0.423)	(0.404)	(0.037)	(0.038)
Light	0.863	0.916	0.873	0.054	0.006
	(0.344)	(0.278)	(0.333)	(0.040)	(0.040)

Panel C: API Characteristics

Variable	API Standard Mean (SD)	API Plus Mean (SD)	Difference Plus-Std (SE)
Age	28.491	28.543	-0.135
	(3.760)	(3.075)	(0.650)
Male	0.566	0.587	-0.064
	(0.500)	(0.498)	(0.097)
High Edu Complete	0.887	0.891	0.014
	(0.320)	(0.315)	(0.066)
Previously Instructor	0.792	0.848	-0.079
	(0.409)	(0.363)	(0.072)
Previously Education Assistant	0.075	0.065	0.014
	(0.267)	(0.250)	(0.049)

Notes: The first three columns of the table reports mean and standard deviations in parenthesis for various characteristics collected in the 2016 follow-up survey. For a detailed descriptions of the variables used in this table, see Appendix A.2. The differences reported in the last two columns of the table are based on OLS regressions that control for stratification dummies. Standard errors are reported in parenthesis in the last two columns and they are clustered at school level for community instructors and household characteristics.

Table C.2: Characteristics of Dropout mentors

	Standard	Plus	Plus - Standard
Former CONAFE facilitator	0.689	0.703	0.012
	(0.468)	(0.463)	(0.102)
At least 5 days of training	0.467	0.514	0.061
	(0.505)	(0.507)	(0.111)
Sleeps in community (y/n)	0.711	0.757	0.052
	(0.458)	(0.435)	(0.097)
Number of nights in community last week	3.022	2.757	-0.301
	(2.061)	(1.978)	(0.442)
Number of students with personalized attention	6.049	5.767	-0.284
	(0.835)	(1.104)	(0.264)
Days spent in community during last month	10.220	10.200	0.063
	(4.613)	(4.715)	(1.148)
Insufficient students Level 2 according instructor	3.450	3.560	0.079
	(1.679)	(1.660)	(0.440)
Insufficient students Level 3 according instructor	2.727	2.731	-0.020
	(1.773)	(1.845)	(0.488)

Notes: This table shows the characteristics of the mentors who dropped out from the schools where they were originally assigned across API Standard and API Plus modalities. For a detailed descriptions of the survey variables used in this table, see Appendix A.2.

Table C.3: API and Educational Inequality

	Reading Score School-Level SD	Math Score School-Level SD	Socio-emotional School-Level SD
API Standard	-0.045	-0.004	0.059
	(0.047)	(0.040)	(0.060)
API Plus	-0.131	-0.098	0.118
	(0.053)	(0.045)	(0.069)
H0: Standard=Plus	0.126	0.067	0.430
Mean Dep. Var.	0.780	0.839	0.817
SD Dep. Var.	0.329	0.260	0.368
Observations	220	220	216

Notes: This table shows the estimates of the two API modalities on the school-level standard deviation in cognitive and socio-emotional skills. For a detailed descriptions of the reading, math and socio-emotional scores used in this table, see Appendix A.2. Standard errors in parentheses.

Table C.4: Treatment Assignment and School-level Student Composition

	Repeat	Attrition	Outside CONAFE in Previous Year	Same school in Previous Year
API Standard	-0.011	-0.018	-0.002	0.019
	(0.007)	(0.018)	(0.014)	(0.018)
API Plus	-0.010	-0.006	-0.003	0.011
	(0.007)	(0.018)	(0.016)	(0.019)
H0: Standard=Plus	0.834	0.491	0.911	0.620
Mean Control	0.011	0.061	0.045	0.934
SD Control	0.106	0.240	0.208	0.248
Observations	1019	1019	1019	1019
clusters	224	224	224	224

Notes: This table shows the estimates of the two API modalities on various measures of school-level changes in student composition. For a detailed descriptions of the survey variables used in this table, see Appendix A.2. Standard errors in parentheses are clustered at the school level.

Table C.5: Pedagogical Practices

	Teaching Score	Material Score
API Standard	0.012	-0.170
	(0.136)	(0.149)
API Plus	-0.002	0.014
	(0.154)	(0.158)
H0: Standard=Plus	0.933	0.247
Mean Dep. Var.	-0.005	0.056
Observations	259	259
Clusters	209	209

Notes: This table shows the estimates of the two API modalities on two measures of pedagogical practices: teaching score and material score. For a detailed descriptions of the outcome variables used in this table, see Appendix A.2. Standard errors in parentheses are clustered at the school level.

Table C.6: Parental Behaviors

	Helps with homework	School Activities 1	School Activities 2	Meeting with Teacher	Child Engaged in Extra-Activities
API Standard	0.051	-0.123	-0.168	0.046	0.072
	(0.034)	(0.085)	(0.097)	(0.075)	(0.042)
API Plus	0.082	0.152	0.080	0.195	0.112
	(0.042)	(0.087)	(0.097)	(0.081)	(0.045)
H0: Standard=Plus	0.464	0.007	0.036	0.113	0.409
Mean Control Group	0.479	-0.001	0.000	-0.006	0.636
SD Control Group	0.500	1.001	1.001	0.995	0.482
Observations	1043	1044	1044	973	1032
Clusters	224	224	224	223	224

Notes: This table shows the estimates for the behavioural responses in parental investments. The omitted category refers to the control group. For a detailed descriptions of the outcome variables used in this table, see Appendix A.2. Standard errors in parentheses are clustered at the school level.

Table C.7: Covariate Balance for Sub-Sample Below 7

Variable	(1) Control Mean (SE)	(2) API Standard Mean (SE)	(3) API Plus Mean (SE)	(4) Difference (2)-(1)	(3)-(1)
Panel A: Student					
Baseline Age (Months)	104.308	104.479	104.769	0.071	0.925
	(17.684)	(18.548)	(16.672)	(2.555)	(2.393)
Male	0.462	0.574	0.462	0.121	0.018
	(0.501)	(0.497)	(0.502)	(0.068)	(0.074)
Scholarship	0.778	0.755	0.782	-0.014	0.028
	(0.418)	(0.432)	(0.416)	(0.068)	(0.058)
Score Baseline Spanish. Test Conafe	8.085	8.019	8.096	-0.027	0.023
	(0.728)	(0.640)	(0.641)	(0.128)	(0.124)
Score Baseline Math Test Conafe	8.248	8.135	8.238	-0.084	-0.017
	(0.833)	(0.684)	(0.595)	(0.148)	(0.130)
Score Baseline Natural Science Test Conafe	8.105	8.061	8.105	-0.029	-0.006
	(0.715)	(0.561)	(0.608)	(0.122)	(0.125)
Score Baseline Social Sciences	7.872	7.894	7.965	0.057	0.085
	(0.723)	(0.644)	(0.624)	(0.129)	(0.130)
Repeater	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel B: Household Characteristics					
Indigenous Language	0.325	0.372	0.526	0.036	0.189
	(0.470)	(0.486)	(0.503)	(0.108)	(0.105)
Read	0.786	0.734	0.844	-0.043	0.058
	(0.412)	(0.444)	(0.365)	(0.065)	(0.059)
Less than Primary	0.624	0.479	0.462	-0.145	-0.166
	(0.486)	(0.502)	(0.502)	(0.084)	(0.070)
Upper Sec. or Higher	0.017	0.032	0.026	0.013	0.006
	(0.130)	(0.177)	(0.159)	(0.025)	(0.017)
Oportunidades	0.855	0.798	0.833	-0.065	-0.033
	(0.354)	(0.404)	(0.375)	(0.056)	(0.053)
Refrigerator	0.479	0.462	0.410	0.015	-0.046
	(0.502)	(0.501)	(0.495)	(0.078)	(0.089)
Television	0.795	0.806	0.718	0.021	-0.073
	(0.406)	(0.397)	(0.453)	(0.067)	(0.081)
Car	0.128	0.117	0.064	-0.015	-0.067
	(0.336)	(0.323)	(0.247)	(0.063)	(0.049)
Phone	0.256	0.298	0.244	0.057	-0.000
	(0.439)	(0.460)	(0.432)	(0.068)	(0.072)

Notes: School characteristics are based on the 2012 ENLACE and the 2013 Formato 911. Information on the characteristics of the community instructors draws on the 2016 follow-up survey, except for the number of training weeks that are reported in 2014 CONAFE administrative data. Student level information is based on the 2016 follow-up survey and the 2014 CONAFE administrative information on student diagnostics. Household level characteristics are obtained from the 2016 follow-up survey. The differences reported in the last two columns of the table are based on OLS regressions that control for stratification dummies. Standard errors for community instructors, student and household characteristics are clustered at school level.

Table C.8: Covariate Balance for Sub-Sample Above 7

Variable	(1) Control Mean (SE)	(2) API Standard Mean (SE)	(3) API Plus Mean (SE)	(4) Difference (2)-(1)	(3)-(1)
Panel A: Student Characteristics					
Baseline Age (Months)	105.049	104.536	105.834	-0.715	0.594
	(15.820)	(17.215)	(13.978)	(1.456)	(1.403)
Male	0.556	0.488	0.580	-0.066	0.029
	(0.498)	(0.501)	(0.495)	(0.041)	(0.048)
Scholarship	0.752	0.730	0.740	-0.022	-0.010
	(0.433)	(0.445)	(0.440)	(0.047)	(0.045)
Score Baseline Spanish. Test Conafe	7.275	7.463	7.309	0.193	0.026
	(0.839)	(0.848)	(0.710)	(0.119)	(0.100)
Score Baseline Math Test Conafe	7.397	7.535	7.349	0.146	-0.053
	(0.857)	(0.841)	(0.777)	(0.116)	(0.104)
Score Baseline Natural Science Test Conafe	7.378	7.553	7.357	0.180	-0.026
	(0.821)	(0.804)	(0.663)	(0.116)	(0.099)
Score Baseline Social Sciences	7.242	7.447	7.230	0.208	-0.021
	(0.843)	(0.800)	(0.690)	(0.118)	(0.105)
Repeater	0.033	0.005	0.017	-0.030	-0.019
	(0.178)	(0.069)	(0.128)	(0.015)	(0.018)
Panel B: Household Characteristics					
Indigenous Language	0.340	0.365	0.470	0.041	0.122
	(0.474)	(0.483)	(0.500)	(0.062)	(0.079)
Read	0.682	0.654	0.683	-0.031	0.002
	(0.466)	(0.477)	(0.466)	(0.050)	(0.052)
Less than Primary	0.621	0.649	0.641	0.025	0.014
	(0.486)	(0.478)	(0.481)	(0.049)	(0.049)
Upper Sec. or Higher	0.013	0.009	0.011	-0.004	-0.002
	(0.114)	(0.097)	(0.105)	(0.009)	(0.010)
Oportunidades	0.814	0.829	0.840	0.017	0.030
	(0.390)	(0.377)	(0.368)	(0.036)	(0.033)
Refrigerator	0.374	0.357	0.361	-0.015	-0.003
	(0.485)	(0.480)	(0.482)	(0.051)	(0.060)
Television	0.654	0.725	0.619	0.070	-0.031
	(0.477)	(0.448)	(0.487)	(0.052)	(0.058)
Car	0.069	0.066	0.061	-0.001	-0.006
	(0.253)	(0.249)	(0.240)	(0.023)	(0.024)
Phone	0.205	0.213	0.193	0.009	-0.003
	(0.404)	(0.411)	(0.396)	(0.042)	(0.041)

Notes: School characteristics are based on the 2012 ENLACE and the 2013 Formato 911. Information on the characteristics of the community instructors draws on the 2016 follow-up survey, except for the number of training weeks that are reported in 2014 CONAFE administrative data. Student level information is based on the 2016 follow-up survey and the 2014 CONAFE administrative information on student diagnostics. Household level characteristics are obtained from the 2016 follow-up survey. The differences reported in the last two columns of the table are based on OLS regressions that control for stratification dummies. Standard errors for community instructors, student and household characteristics are clustered at school level.

Table C.9: Total API Exposure at Scale and Original Treatment Assignment

Panel A: First Stage (Outcome: Transition to Secondary School Fall 2017)	
Original API Plus	1.96
	(0.10)
F-stat (Excluded Instrument)	414.94
Observations	625
Panel B: First Stage (Outcome: School is Open Fall 2018)	
Original API Plus	1.99
	(0.13)
F-stat (Excluded Instrument)	234.87
Observations	224

Notes: This table shows the first-stage estimates for the model in (2)-(3). The omitted category refers to the control group. In both panel A and panel B, the dependent variable in the first-stage regression represents the total years of API exposure. panel A shows the first-stage estimates when the second-stage repression outcome is transition to secondary school in the fall of 2017, while panel B shows the first-stage estimates when the second-stage repression outcome is whether schools are open in the fall of 2018.