WORKING PAPERS

N° 1196

May 2024

"Enhancing Human Capital in Children: A Case Study on Scaling"

Francesco Agostinelli, Ciro Avitabile and Matteo Bobba

Toulouse School of Economics

Enhancing Human Capital in Children:
A Case Study on Scaling*

Francesco Agostinelli† Ciro Avitabile‡ Matteo Bobba§

May 7, 2024

Abstract

This paper provides novel insights into the science of scaling by examining an educational mentoring program in Mexico. The empirical analysis encompasses two independent field experiments, and seizes a unique opportunity to learn from the government's implementation of the same intervention. While the program originally implemented at scale demonstrates limited effectiveness, the introduction of a new modality with enhanced mentor training significantly improves children's outcomes. Mentor-parent interactions are found to stimulate parental engagement at the community-school level. Our findings support the hypothesis that parents can play an important role in facilitating the scalability of educational programs.

*We are grateful to the *Consejo Nacional de Fomento Educativo* (CONAFE) for the collaboration throughout this project, and in particular to Carmen Gladys Barrios Veloso and Janet Venancio. We thank the Editor, John List, and the anonymous referees for their comments and feedback that greatly improved the paper. We also thank Jere Behrman, Jim Heckman, Giuseppe Sorrenti, and Stephane Straub for helpful comments and discussions. We are indebted to Alonso Sanchez for the contribution in the early stages of this project, and to Miguel Angel Monroy for excellent research assistance. Ciro Avitabile acknowledges financial support for data collection from the Strategic Impact Evaluation Fund (SIEF) of the World Bank and the *Consejo Nacional de Evaluación de la Politica de Desarollo Social* (CONEVAL). Matteo Bobba acknowledges financial support from the AFD, the H2020-MSCA-RISE project GEMCLIME-2020 GA No 681228, and the ANR under grant ANR-17-EURE-0010 (Investissements d'Avenir program). This study is registered in the AEA RCT Registry and the unique identifying number is AEARCTR-0001645.

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

‡The World Bank. E-mail: cavitabile@worldbank.org.

§Toulouse School of Economics, University of Toulouse Capitole & CEPR & IZA. E-mail: matteo.bobba@tse-fr.eu.

1 Introduction

A key challenge in using scientific insights to inform policy decisions arises during the implementation process, where small variations in the protocol of the intervention often translate into substantial differences in outcomes. Even when programs display large and significant effect sizes in randomized evaluations, their success in different situations is far from guaranteed (List, 2022). This is particularly evident when transitioning from a controlled research setting to real-world implementation by the government.

This paper contributes to a recent debate about the challenges to scale-up education interventions. In particular, we provide an empirical case study involving a mentoring program that deploys recent university graduates to remote and disadvantaged communities in Mexico. Mentors are engaged to a school community for a period of two years. Among other tasks, they help the local instructors, and encourage parental involvement in children's education. The mentoring intervention was initially launched at the national scale by the government without undergoing a rigorous evaluation. It featured a training module for mentors focused on curricular knowledge and pedagogical practices. However, subsequent evidence gathered through a randomized trial revealed null results of this program modality. The lack of effectiveness of the program served as a catalyst, prompting the need to improve the delivery of mentoring services in the targeted communities.

Our research team collaborated with the government—including accessing the existing infrastructure of the ongoing intervention—to design and implement an experimental evaluation of a new program modality that incorporated an enhanced training protocol for mentors. Among other changes in the training module, mentors attend training workshops and peer-to-peer meetings throughout the school year, during which they share effective practices on how to improve parenting skills and better interact with parents. The program innovations were largely influenced by the economic literature showing that gaps in family investment and parent/child interactions are behind the gaps in children's achievements among different socio-economic groups (Cunha et al., 2010; Heckman and Mosso, 2014; Attanasio et al., 2022b).

Science guides policy. Following the release of compelling evidence regarding the effectiveness of the new modality, which demonstrated significant positive effects on children's outcomes and increased parental investment, the government made the decision to adopt the program with the most effective approach. Our comprehensive analysis of the program's implemen-

tation in Chiapas, the poorest state in Mexico, provides strong support for the notion that the effectiveness of the new modality of the mentoring intervention was preserved at scale. Importantly, we find that parental engagement and attitudes toward schooling activities emerge as critical factors for the program's scalability.

Throughout the analysis, our empirical evidence draws from two independent field experiments and the subsequent government scale up of the effective program modality. The first experiment is directly carried out by the government immediately following the national implementation of the original mentoring program. Assignment to the program is randomized across 80 program-eligible primary schools. In the second experiment we randomly assign both the original and the new modality as well as a control group with no mentoring program across 230 primary schools. After two years of the mentoring program, the original modality displays relatively small and noisy effects on children's achievement outcomes when compared to the control group with no mentors. The new modality with enhanced training for the mentors delivers sizable and significant gains in children's reading scores (+0.32 standard deviations, p-value = 0.001), math scores (+0.24 standard deviations, p-value = 0.005), and socio-emotional scores (+0.20 standard deviations, p-value = 0.011) as well as a large effect on the probability of enrolling in seventh grade (+12.4 percentage points, p-value = 0.032), out of a basis of 62 percent enrollment in the control group. The effect of the mentoring intervention on educational outcomes is statistically different across program modalities.[1]

The government's decision to transition the program to a more effective modality provides us with a unique opportunity to investigate the factors and mechanisms influencing scaling. We integrate data from several administrative sources and leverage the early assignment of the program at scale across communities in Chiapas as a source of variation in exposure to mentoring activities. For our sample of 1,345 eligible schools, we demonstrate that, after considering the government's official criteria for program assignment, the 356 schools that received the 2-year mentoring program in our sample period exhibit no significant differences in observable characteristics and predetermined educational outcomes when compared to the remaining eligible schools. Our results show that the new variant of the mentoring intervention remained successful when implemented by the government. For the sub-sample of the 1,161 localities outside of the experimental sample, which had never received the *Plus* modality before and encompassed approximately 16,000 children enrolled in eligible

[1] All p-values reported in the text are adjusted for multiple hypotheses testing through the step-wise procedure described in Romano and Wolf (2005a,b, 2016). Alternative inference procedures, which are discussed in more detail in Section 3, deliver results that are broadly consistent with those reported here.

schools, the results show a positive effect on secondary school enrollment for schools that received the program, with an average program impact of 5.6 percentage points (p-value = 0.013). We further document positive and significant effects of the program on child literacy, which imply a reduction of illiteracy rates by approximately 20 percent with respect to the sample mean for the overall sample of schools. Regarding the remaining eligible localities that were previously included in the experimental evaluation, we observe comparable results. For instance, the average impact of receiving the new program modality on the fraction of children who enroll in lower-secondary education is +9.1 percentage points (p-value = 0.035).

The effectiveness of the new program when implemented by the government was not guaranteed *a priori*, despite the sizable and precise effect sizes observed in the field experiment. The existing literature underscores the significance of several "non-negotiable" aspects of program designs in the context of such changes in the situation (List, 2022). Neglecting to consider these critical elements during the widespread implementation of the intervention has the potential to not only reduce but also completely eliminate the program effects documented under experimental conditions (Al-Ubaydli et al., 2020; Caron et al., 2021; Al-Ubaydli et al., 2021). While we do observe minor changes in both the quantity and quality of mentors' activities during the scale-up phase, these differences are generally small in magnitude and lack statistical precision. We interpret these results as suggestive evidence that the mentoring program did not experience a substantial disruption during the transition between the two phases.

We argue that a potential source of "voltage drop" of the program results from the experimental design's trade-off between real-world applicability and the purity of the evaluation. While a significant challenge faced by educational programs in this context is the occurrence of frequent school closures, the intense monitoring during the field experiment from the research team had minimized the extent of this event in the period of the evaluation. The school closure rate is notably high, averaging 11 percent before the experimental evaluation. In contrast, only two schools closed during the randomized trial out of 230 schools. To the extent that the continuity of the schooling services is critical for ensuring the program's effectiveness, this particular difference in the implementation protocol poses a challenge for the ability of the field experiment to inform us about the scalability of the mentoring program.

We zoom into the relationship between exposure to the mentors and school closures in order to study the sources of scalability of the program. We first show evidence that the new program modality, unlike its predecessor, drastically reduces the occurrence of school clo-

sures. Within the community-based schooling system under investigation, parents emerge as pivotal actors, wielding influence through their decisions and votes within the parent association. Their choices and actions directly impact crucial aspects such as resource allocation, investments, and the ultimate determination of whether the school remains open or not. While the original modality of the mentoring program does not significantly affect parental investments, mentors with enhanced training are more effective in boosting parental engagement, both toward the school and directly with the child. Our measure of parenting practices increases by 0.36 standard deviations under the new program modality (p-value=0.002). We further show that mentors with enhanced training significantly increase both the quantity and the quality of their periodic interactions with parents, which in turn shaped parental attitudes and behaviors toward their children's education.

Taken together, the evidence on school closures and on parental responses provides suggestive evidence that parents can play an important role in the scalability of the program. Under the assumption that the treatments effects of the new modality of the mentoring program operate only through parental engagement, IV estimates document that an increase of 0.1 standard deviation in parental engagement reduces school closures by 2.2 percentage points (p-value = 0.021). The original modality, instead, displays null impacts on school closures in both experiments. This finding underscores the challenge faced by community-based educational programs in situations where parental engagement is lacking, making their success more difficult to achieve.

The implications of our results are pertinent to policy discussions on the design of educational interventions in disadvantaged contexts. While parents within local communities are readily available without supply-side constraints, it is crucial not to overlook their beliefs and attitudes toward schooling activities. For instance, August et al. (2006) find that as the situation shifted from their original field study to a larger scale implementation, there was a notable decline in family involvement in a program aimed at preventing conduct problems. This reduction in participation on a larger scale could significantly impact the effectiveness of broader implementations. The precise elements of the intervention (such as the training module in our study) and the consequent efficacy of mentor-parent interactions are key factors influencing how parents respond, thereby influencing the potential scalability of educational interventions.

In recent years, there has been increasing concern among scholars and policymakers regarding the effectiveness of field experiments in informing policy decisions. This concern stems from

recent and growing evidence on the challenges of replicating the effects observed in small-scale randomized trials when interventions are implemented at a larger scale (Bold et al., 2018; Cameron et al., 2019; Muralidharan and Singh, 2020; Bobba et al., 2023). Our field experiment has been designed and implemented during the roll-out of the original program, in close collaboration with the government agency responsible for the subsequent scale-up of the new modality. This collaborative approach guarantees the harmonization of the research design with the practical considerations and implementation realities of the policy under study (Banerjee et al., 2017; Muralidharan and Niehaus, 2017; Duflo et al., 2024; List, 2024).

We analyze two independent field experiments on different and representative samples as well as a larger, non-experimental sample of schools. Drawing joint inferences from these samples harnesses the statistical power of our findings (Maniadis et al., 2014; Allcott, 2015; Al-Ubaydli et al., 2020). Furthermore, the randomization was implemented at a relatively large unit level, encompassing schools and their surrounding communities. This design feature accounts for possible local spillover effects that often arise in the context of interventions evaluated at scale (Miguel and Kremer, 2004; Bobba and Gignoux, 2019; List et al., 2023).

Our findings build upon the theory of human capital investments (Becker, 1962) by highlighting that scaling educational interventions is inherently a socially-determined outcome. Previous literature has highlighted the role of parental investments and parent-mentor/home visitor interactions in boosting treatment effects of home visiting programs (Heckman and Zhou, 2021; Zhou et al., 2021; García and Heckman, 2023), and that parental choices are responsive to the environments that families face (Doepke and Zilibotti, 2017; Agostinelli, 2018; Agostinelli et al., 2020). The evidence presented here sheds light on how the scalability of educational programs critically depends upon the local engagement of parents in the schooling activities.

2 Context and Data

In this section, we discuss some relevant features of the mentoring program under study. We leverage two independent randomized experiments in conjunction with the conversion of the mentoring program into the new modality, serving as a compelling case study to uncover novel insights on the science of scaling. We draw on a rich combination of administrative and survey data sources, along with qualitative interviews with instructors and mentors (see Appendix A for more details).

2.1 The Mentoring Program

The *Consejo Nacional de Fomento Educativo* (CONAFE) is a government agency responsible for providing schooling services in rural and highly marginalized communities of Mexico with a population below 2,500 inhabitants. In 2013, these schools accounted for 10 percent of the roughly 99,000 primary schools across the 31 Mexican states. The largest presence of CONAFE schools is in Chiapas, the Mexican state with the highest incidence of poverty in the country (CONEVAL, 2018). CONAFE primary schools typically have a single multi-grade classroom with on average 15 students. For brevity, throughout the text we will refer to CONAFE primary schools as schools.

The local instructors predominantly consist of community residents aged between 15 and 29 years old, who typically have minimal to no formal training as teachers. 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. Schools frequently face closure due to similar challenges. In fact, the average yearly rate of school closures in Chiapas stands at 11 percent. Parents organize local associations aimed at promoting community education, to which they contribute by maintaining the school's facilities and contributing to the school's financial means. The parents' association also plays a vital role in the decision-making process to ensure the continuation of school operations.

In 2009, the government launched the "Mobile Mentors" (*Asesores Pedagogicos Itinerantes,* API henceforth) program as an attempt to improve the quality of education provision in basic education. Initially, the program was implemented in 11 states, but starting in 2012, it was extended to all 31 states in Mexico. The mentors are selected from recent university graduates (the program was advertised both during 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. Prior to start working as mentors, selected applicants receive a week-long training session focused on curricular knowledge and basic notions of pedagogy.

Mentors are assigned to work within a specific school community for the entire duration of the program, spanning two consecutive school years. In the event of a mentor's early departure from the community, the government endeavors to identify a replacement to ensure the uninterrupted continuation of program activities. The supply of available mentors only allows for accommodating a subset of the CONAFE schools in the country. Consequently,

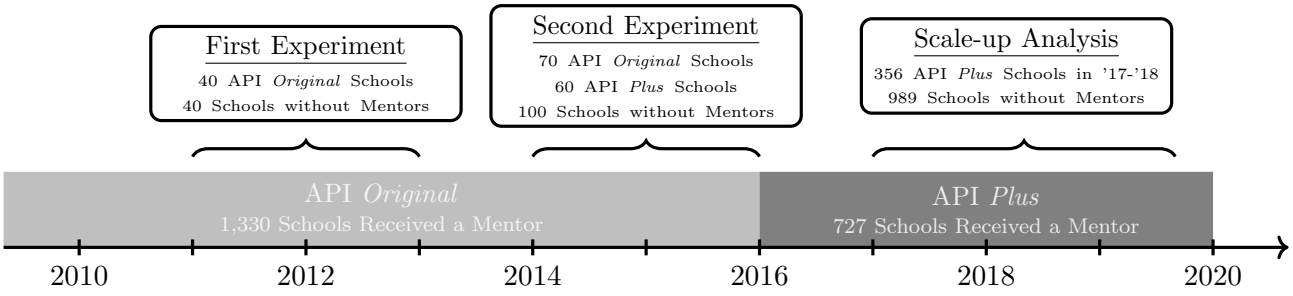
the allocation of the mentoring program across schools follows a priority-based mechanism based on four criteria: (i) at least 30 percent of the students are classified as "insufficient" in the national standardized achievement test; (ii) there are high, or very high, levels of poverty in the respective localities, as measured by a composite index of deprivation (CONEVAL, 2018); (iii) the municipality where the school communities are located is the recipient of a national anti-poverty program (the National Crusade Against Hunger); and (iv) the school has not received a mentor in previous academic cycles. A given school can receive the mentoring program over multiple (and even consecutive) two-year spells.

Mentors conduct periodic home visits as well as meetings on the school premises to update parents about their children's progress in school and encourage their active involvement in school activities. Each mentor is responsible for organizing individual remedial education sessions at school, which are held primarily after regular instructional hours. The tutoring sessions are offered to the six weakest students in the class, identified through a diagnostic evaluation conducted at the beginning of the school year and an additional exam administered by the mentor. During regular school hours, mentors are tasked with observing and possibly improve the teaching practices of community instructors. They also assist students with learning difficulties and provide support outside the classroom for those unable to attend the afternoon remedial sessions.[2] Finally, mentors hold periodic meetings with their own supervisors throughout the school year (every two months in two-day sessions), which are focused on enhancing mentors' pedagogical practices with the students. Henceforth, we will refer to this format of the mentoring program as the API *Original*.

Starting from 2016, the government adopted a new modality of the program, which we will refer to as the API *Plus* modality. The API *Plus* encompasses all the features of the API *Original* but with substantial changes in the training module. First and foremost, it extends the initial training period from one week to two. During this additional week, the focus shifts to practical, hands-on strategies designed to enhance students' reading and math skills. Secondly, the bimonthly meetings have undergone significant modifications. In contrast to the *Original* modality, each *Plus* modality meeting includes a training workshop to improve parenting skills (communication, learning activities, and managing transitions). Additionally, there is an extra day dedicated to peer-to-peer sessions. These sessions are intended to enable mentors to collaboratively devise strategies for more effective interaction

[2]Only the remedial tutoring activities are targeted toward the six weakest students in the class. The other tasks of the mentors (home visits and teachers' support) are directed toward all children in the school.

Figure 1: The Mentoring Program in Chiapas and the Different Study Samples



and engagement with parents.[3] The cost of the API *Plus* is US $332 per child, compared to US $285 per child for the API *Original*. These cost figures align closely with those of another recent government-run program in Colombia, which targets both children and parents (Attanasio et al., 2022a).

Figure 1 illustrates the timeline of the mentoring program in the State of Chiapas. Between 2009 and 2016, the API *Original* served over 1,300 schools and approximately 18,000 students. Following the transition to the API *Plus* between 2016 and 2020, around 700 schools and 10,000 students received mentoring support. Due to the program's two-year rotation cycle, different schools received the program over time, guided by the timeline and assignment criteria. CONAFE has the capacity to deploy approximately 360 mentors for each program's cycle in Chiapas, and this number has been reasonably stable since 2009. This reflects both the financial constraints of the program and a supply-side constraint related to the availability of highly-educated mentors in this context. For this reason, not all primary schools receive a mentor within a cycle. However, by the spring of 2016, almost the totality of schools in Chiapas received a mentor due to the reassignment of mentors across schools. When in the fall of 2016 the government converted the program in its *Plus* modality, the mentor assignments across schools started anew. This explains why the cumulative number of schools that received the API *Original* modality at least once during the 2009-2016 (seven school years) is approximately double the cumulative number of schools that received the

[3]The decision to innovate the program's modality along these lines was inspired by extensive economic literature, which suggests that successful mentoring programs in similarly disadvantaged contexts share a design feature of fostering parental engagement (Heckman and Mosso, 2014; Attanasio et al., 2022b; García and Heckman, 2023). Moreover, this decision was influenced by the feedback received from the mentors during the initial implementation of the *Original* modality. They reported that the interactions with parents were the most challenging aspect of their tasks in the local communities.

API *Plus* modality at least once during the 2016-2020 (four school years).[4]

2.2 Experimental Design and Scale-up Analysis

Two consecutive and independent randomized evaluations took place following the nation-wide implementation of the *Original* mentoring program. A first experiment was directly carried out by the government. We designed and implemented a second experiment in close collaboration with the government, leveraging the existing program's infrastructure. Figure 1 visually depicts the timelines for each pilot in comparison to the government's implementation at scale of the programs. After learning about the results of the second experiment (see Section 3), the government decided to discontinue the API *Original* program in the summer of 2016. All its primary schools, including those that were part of the evaluation samples of the two randomized trials, were deemed eligible to receive the API *Plus* program modality from the school year 2016-2017.

First Experiment. Eighty program-eligible primary schools are selected among those that never received the mentoring program before. Of those, 62 schools are located in the state of Chiapas and the remaining 18 schools are in the three States of Hidalgo, Queretaro, and Veracruz. Assignment to the mentoring program is randomized at the school level using a block design, with the strata represented by the Mexican states where schools are located. Forty schools are assigned to receive the API *Original* starting from the 2011–2012 school year while the remaining half of the schools are assigned to the control group without mentors.

We use administrative data on student-level test scores and a household survey that was collected by the government. Student outcomes are measured two school-years after the assignment of the API program through the performance in the national standardized test for students in grades three through six. A mid-line survey records parental behaviors and investments for 208 parents in 73 schools (the enumerators were not able to reach the parents in seven schools). Due to the incomplete take-up of the standardized achievement test—mainly due to the opposition from the teachers' unions in some states—we are able

[4]Out of the 1,523 schools in the program-eligible communities of Chiapas, 1,330 received mentoring at least once between 2009 and 2016. The final deployment of API-*Original* mentors took place in the 2015-2016 school year, coinciding with the second year of the program assignment for the second experiment (see Figure 1). At that point, the vast majority of the schools that did not receive mentoring were part of the experimental sample (100 schools in the control group and 60 schools in the API *Plus* group). As a result, the fourth criterion of the priority-based scheme as described in Section 2.1, which assessed whether communities had previously received a mentor, lost its significance in determining assignment priorities for the expansion of the *Plus* program.

to match 70 schools with 599 test score records out of the sub-sample of 73 schools with parental outcomes. Out of the ten schools that were part of the experimental sample and we are unable to match in our final sample, five schools are in the treatment group and five are in the control group. Appendix Table B-1 shows balance with respect to the assignment of the mentor for school and community characteristics measured in the year before the start of the first experiment.

Second Experiment. 230 program-eligible primary schools are selected in rural Chiapas among those that never received the mentoring program before. Assignment of the mentors is carried out using a randomized block design at the school level, with the strata represented by the deciles of the 2012 school-average in the national standardized achievement score in the Spanish test. As a result, 60 schools are assigned to receive API-*Plus* mentors starting from the 2014-2015 school year, another group of 70 schools are assigned to receive API-*Original* mentors over the same time period, and the remaining 100 schools are in the control group with no mentors.

The data collection occurred at the end of the second experimental school year. By that time, two of the original 230 schools in the sample had closed, and another four schools couldn't implement the program due to high political instability. Out of the six schools that dropped out of the sample, two schools are in the control group, two are in the *Original* group, and two in the *Plus* group. The number of schools part of the second experiment is 224. Appendix Table B-2 shows that a large array of pre-determined covariates of schools, teachers, children, households, and mentors is balanced with respect to the assignment of both API *Original* and API *Plus*. The household module of the survey is collected for a random sample of five households within a five kilometer radius from each school. The information is linked at the child-parent level through unique student identifiers. The final sample consists of 1,045 children.

We use separate administrative data on students' records to construct an indicator for enrollment in seventh grade, which is the first grade in lower secondary school. We link the seventh-grade enrollment data in Chiapas in the Fall of 2016 with the students in our sample who were in sixth grade during the spring of 2016 and were therefore making the decision of whether to enroll in lower-secondary school. The sample reduces to 468 sixth graders in 182 schools due to the variation in student composition across schools in later grades. The choice of this cohort of students is meant to maintain the same length of exposure to the

mentoring program of the sample of children in the survey.[5]

API *Plus* Scale-up Analysis. As indicated in Figure 1, we focus the analysis on the 356 schools that received a mentor during the 2017-2018 school year out of a total of 1,345 eligible schools (see Section 2.3). This sample selection allows for two complete school years between the arrival of the mentors in the communities and the data collection. At the same time, it enables us to assess the program's effectiveness under the *Plus* modality after sufficient time for program operations to fully adapt to the enhanced training module for mentors. Among the 356 schools that were assigned API-*Plus* mentors during the 2017-2018 school year, 270 were not part of our second experiment and 86 were part of the experimental sample. Within the experimental sample, the probability of receiving a mentor during the national scale-up of the *Plus* modality is constant with respect to the treatment arms of the second experiment after controlling for the program eligibility criteria—p-value($Original$) = 0.367, p-value($Plus$) = 0.660.

We employ administrative records detailing the program implementation in Chiapas under the API *Plus* modality and match this information with the quasi-universe of schools located in villages surveyed in the 2020 population census. We utilize two village-level educational outcomes from the Census data, which refer to the school year that started in the Fall of 2019: (i) the rate of lower-secondary enrollment among children between twelve and fourteen years old and (ii) the rate of child literacy for children between eight and fourteen years old. Unlike other school-survey-based or administrative test scores, these outcomes are not subject to any censoring due to school closures. This allows us to avoid the concerns about sample selection and survivorship bias, due to differential school closures induced by the API *Plus* program when implemented by the government (see Section 4.1).

2.3 Sample Representativeness

We focus our empirical analysis in the State of Chiapas, which hosts the majority of the schools that participated in the first randomized experiment, as well as all the schools involved in the second experiment. Research findings from field experiments may sometimes be difficult to generalize because, in the language of Al-Ubaydli et al. (2020), the properties

[5]The distribution of missing schools in the analysis of transition to secondary school is 18 schools in the control group, 14 in the API *Original* and 16 in the API *Plus*. Due to the different individual identifiers, we are not able to match this dataset to the survey data. The estimates reported in Appendix Table B-3 document no program effects on grade repetition and attrition, which suggest that conditioning on grade attainment is not problematic in our context.

of the study population may differ from the population of interest to policy makers.[6]

In Table 1 we compare means in observable characteristics between the overall population of schools in the state of Chiapas and both experimental samples. The students enrolled in the schools of the first experiment tend to perform worse in the national standardized tests (Spanish and Math) when compared to the overall population of students. Also, schools in the first experiment are located in larger localities in terms of population size. Instead, as shown in the fifth column of Table 1, we cannot reject equal means across the several variables assessed between the sample of schools of the second experiment and the overall population of schools in Chiapas. There is only a small imbalance in the number of enrolled students (see Panel A in Table 1).

This result underscores the representativeness of the study sample in the second experiment with respect to the population of interest. The fact that the sample of schools in the initial government-led experiment may not offer a comprehensive picture of the intervention's target population in Chiapas provides a further rationale for conducting a second field experiment to assess both mentoring program modalities, namely API *Original* and API *Plus*, within a larger and more representative sample of schools. In the scale-up analysis, we separate the experimental schools from the rest of the non-experimental schools in Chiapas in order to isolate any difference in the impact of the program across situations (field experiment Vs. government implementation of the API *Plus* program).

There are 1,523 schools in Chiapas that are eligible to receive the mentoring program.[7] Of those, we are able to match 1,345 schools (88 percent) with the 2020 population census containing village-level educational outcomes for the quasi-universe of the schools and the localities in Mexico.[8] The probability of being unmatched is orthogonal with respect to the probability of receiving an API-*Plus* mentor during the government's program implementation in Chiapas (p-value=0.634). The 1,345 schools in the matched sample serve approximately 19,000 students, with a total of 165,000 people living in the surrounding communities. Out of these 1,345 schools, 356 received a government mentor by the 2017-2018

[6] In seminal work, Heckman (1992) discusses selection into field experiments and finds that the characteristics of subjects who participate in a randomized job training program in the US can be distinctly different from those of subjects who do not participate. Recent studies document systematic evidence of positive selection of eligible participants in experimental evaluations (Allcott, 2015; Davis et al., 2021).

[7] Only schools with six or more enrolled students are eligible for the program.

[8] The match between the universe of schools and the localities of the population Census is one to one, as each village has at most only one primary school. For further details on the census sampling design, please refer to: https://www.inegi.org.mx/contenidos/productos/prod_serv/contenidos/espanol/bvinegi/productos/nueva_estruc/702825197629.pdf, accessed on October, 2023.

Table 1: Differences Across Populations

	Panel A: School Characteristics				
	All Chiapas Mean (SD)	First Experiment Mean (SD)	Second Experiment Mean (SD)	Chiapas vs. Experiment 1 Mean Difference [p-value]	Chiapas vs. Experiment 2 Mean Difference [p-value]
Average Test Score (Spanish)	424.503	399.116	431.340	-25.387	6.837
	(56.466)	(32.631)	(60.810)	[0.000]	[0.139]
Average Test Score (Math)	414.921	379.165	421.333	-35.756	6.412
	(75.300)	(45.339)	(80.895)	[0.000]	[0.297]
Number of Students	14.049	15.507	15.009	1.458	0.960
	(8.468)	(8.781)	(6.053)	[0.175]	[0.037]
Number of Teachers	1.231	1.333	1.217	0.102	-0.014
	(0.467)	(0.505)	(0.413)	[0.099]	[0.638]
Share Over-aged Students	0.349	0.230	0.324	-0.119	-0.025
	(0.797)	(0.552)	(0.659)	[0.088]	[0.610]
Number of Schools	1,523	80	230	1,603	1,753
	Panel B: Locality Characteristics				
	All Chiapas Mean (SD)	First Experiment Mean (SD)	Second Experiment Mean (SD)	Chiapas vs. Experiment 1 Mean Difference [p-value]	Chiapas vs. Experiment 2 Mean Difference [p-value]
Total Population	118.758	247.280	121.389	128.522	2.630
	(221.648)	(549.923)	(240.562)	[0.043]	[0.879]
Rate of Extreme Poverty	0.490	0.486	0.473	-0.004	-0.017
	(0.500)	(0.503)	(0.500)	[0.949]	[0.644]
Incidence of Social Conflicts	0.190	0.150	0.187	-0.040	-0.003
	(0.392)	(0.359)	(0.391)	[0.335]	[0.919]
Rate of Illiteracy	0.313	0.321	0.295	0.008	-0.018
	(0.160)	(0.157)	(0.153)	[0.662]	[0.127]
Labor Force Participation	0.297	0.289	0.303	-0.008	0.006
	(0.076)	(0.071)	(0.070)	[0.352]	[0.259]
Locality Access without Road	0.216	0.203	0.179	-0.013	-0.037
	(0.411)	(0.404)	(0.384)	[0.777]	[0.181]
Water Network (Y/N)	0.028	0.050	0.022	0.022	-0.006
	(0.164)	(0.219)	(0.146)	[0.365]	[0.578]
Sewage System (Y/N)	0.011	0.038	0.009	0.026	-0.002
	(0.105)	(0.191)	(0.093)	[0.219]	[0.712]
Garbage Collection (Y/N)	0.022	0.038	0.022	0.016	0.000
	(0.146)	(0.191)	(0.146)	[0.463]	[0.994]
Number of localities	1,523	80	230	1,603	1,753

Notes: Panel A shows school-level variables from the school census (2010) whereas Panel B displays community-level characteristics from the population census (2010). The first three columns show means and standard deviations in parentheses for various characteristics collected before the introduction of the API program. The last two columns show asymptotic p-values for mean differences between the overall population and the experimental samples after adjusting for Strata fixed effects. This adjustment accounts for the presence of 18 schools in the first experiment (out of a total of 80 schools) that are situated in different Mexican States other than Chiapas. See Appendix A.1 for more details on the data sources.

school year while the remaining 989 localities did not receive mentors and are used as a comparison group, based on the assignment's priority criteria (see Figure 1). This variation enables us to estimate the program's impact at scale by comparing educational outcomes from the 2019 school year, as recorded in the census, coinciding with the termination of the 2-year program cycle for those schools. The matched-census sample comprises 1,161 non-experimental schools and 184 schools previously engaged in the second experiment. These two sub-samples maintain their representativeness in terms of observable characteristics in relation to the overall targeted populations in Chiapas (see Appendix Table B-4).[9]

Program assignment based on specific priority-based rules often fails to adequately represent

[9]In our scale-up sample, forty-four schools part of the control group in the second experiment also did not receive a mentor during the national scale-up of the *Plus* modality.

the entire population of potential beneficiaries of the intervention. The assignment of the mentors across schools in our second experiment is randomized independently of the criteria that will later dictate program assignment priorities at a larger scale. The estimates of program impacts on student outcomes reported in Appendix Table B-5 do not exhibit patterns of heterogeneity based on the information underlying these official criteria. This evidence is consistent with the hypothesis that the estimated effect of the *Plus* intervention as implemented by the government, which are derived from the 2017-2018 assignment of the mentors across eligible communities, is representative of the impact under the full scale implementation of the program.

3 The Impact of the Mentoring Program on Children

In this section, we assess the impact of two different mentoring program modalities on various measures of children's academic outcomes. We provide empirical evidence supporting the ineffectiveness of the API *Original* by analyzing the results of two independent field experiments. Subsequently, we quantify the positive effects of the API *Plus* in the randomized evaluation as well as during the government's implementation of the mentoring intervention.

3.1 Econometric Model

We analyze the two experiments through linear regression models on the treatment assignment indicators for the API *Original* and the API *Plus* modality after two years of exposure to the mentoring program. An indicator for whether or not the child speaks an indigenous language is the only covariate that is not balanced across treatment arms in the second experiment (see Panel B in Appendix Table B-2). For this reason, we include the indicator for indigenous language in the regression analysis of the second experiment. All models further include the strata control variables that account for the block randomization designs, as well as student's age and gender, which are predictive of education outcomes. During the data collection in the second experiment, 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 is meant to control for the different timing of the survey in these communities. The error terms are clustered at the school level, which represents the unit of randomization in both field experiments.

To expand our analysis, we encompass the vast majority of program-eligible schools within the state of Chiapas (see Section 2.2). Our objective is to investigate whether the API *Plus* modality of the mentoring program, implemented on a larger scale by the government, has effectively enhanced educational opportunities for children in these disadvantaged communities. We leverage the variability in program assignments across communities, determined by the priority-based mechanism described in Section 2.1, to assess the impact of the API *Plus* on a large scale. To do this, we estimate the following linear regression model:

$$Y_j = \alpha_0 + \alpha_1 Plus_j + \boldsymbol{\delta'}\, \boldsymbol{X}_j + \epsilon_j \,, \tag{1}$$

where Y_j is a locality-level outcome on children's education attainment for locality j, while $Plus_j$ takes a value of one if the school in locality j receives a mentor in the school year 2017-2018, and zero otherwise. The vector $\boldsymbol{X}_j$ consists of indicator functions for the four criteria used to determine the differential priority across eligible localities/schools to receive the mentors (see Section 2.1). We also control for the number of hostile events related to land property, religion, elections, crime, or drug addiction as reported at the locality level in the population census (2010) as additional determinants of the assignment of the mentors across localities. Finally, we include in the vector $\boldsymbol{X}_j$ an indicator variable for prior exposure to the API *Original* modality during the period 2009-2015. The parameter of interest, α_1, represents the effect of exposure to the mentoring program during the government implementation on the outcome of interest.

The underlying identification assumption for unbiased and consistent estimation by OLS of α_1 in equation (1) is that the assignment of the program outside of the experiments is *conditionally* random, once we control for the criteria determining the priority of program assignments. In other words, after conditioning on the assignment criteria and the other covariates in equation (1), schools/localities that receive and do not receive the API *Plus* program are assumed to be similar in terms of unobserved characteristics. We provide two pieces of evidence that should bolster the credibility of this conditional independence assumption in our setting. First, we cannot reject the joint null hypothesis of no differences in observable characteristics at school and locality-level based on the school assignment of the mentoring program during the year 2017-2018 (p-value = 0.733), after conditioning on the priority criteria (see Appendix Table B-6).[10] Second, we run some placebo tests using

[10]The only covariate that shows a significant difference at 5% level is of whether the locality can be accessed with a road ("Locality Access without Road"). The inclusion of this extra covariate in our regression model (1) does not affect the estimated effect of the program on children's outcomes.

the school-level standardized achievement test scores collected before the conversion of the mentoring program under the *Plus* modality. Appendix Table B-7 displays the results. The assignment of the mentoring program outside of the experiments is not unconditionally random (odd columns of the table), as priority is given to more disadvantaged communities. Instead, when we control for the vector $\boldsymbol{X}_j$, the estimated coefficients displayed in the even columns of Table B-7 are very small and statistically insignificant.

To curb the possibility of detecting false positives, we go beyond the conventional asymptotic inference by employing three additional procedures. Firstly, we present p-values based on randomization inference, which are accurate irrespective on the number of sampling units or clusters. This approach is particularly relevant for the first experiment, where the number of schools per treatment arm was smaller compared to the second experiment and the scale-up sample. Secondly, given the extensive range of hypotheses explored throughout our analysis, we provide adjusted p-values that account for multiple hypothesis testing across various outcome families (List et al., 2019). Thirdly, building upon the insights in Maniadis et al. (2014); Al-Ubaydli et al. (2020), we leverage the value of replication by conducting two independent randomized trials within the same program environment (API *Original*) as well as by contrasting evidence on the impact of the API *Plus* modality under different situations (field experiment and government roll-out). For each program modality, we calculate p-values for joint null hypotheses across the different study samples used throughout the analysis.[11]

3.2 Evidence on API *Original*

Tables 2 and 3 display the impacts of the *Original* modality on children's schooling outcomes collected two years after the introduction of the mentoring program in each experiment, respectively. For the first experiment, the outcome variables shown in Table 2 are based on administrative records of third to sixth graders in a national standardized test of academic achievement. For the second experiment, we collect our own measures of cognitive and socio-emotional skills (first to fourth columns of Table 3), as well as a measure of educational

[11]To test hypotheses across study samples, we employ Fisher's combined probability test: $-2\sum_{i=1}^{k} log(p_i) \sim \chi^2_{2k}$, where $p_i \sim U[0,1]$ is the p-value for the i^{th} hypothesis test and k is the number of independent study replications being combined. This is akin to the joint statistical significance test commonly used in meta-analyses.

Table 2: Children's Achievement—First Experiment

	Reading Score	Math Score	Science Score	Overall Index
API *Original*	-0.053	0.083	-0.082	-0.022
	[0.737]	[0.655]	[0.585]	[0.902]
	{0.750}	{0.669}	{0.591}	{0.910}
	(0.779)	(0.739)	(0.717)	(0.878)
Number of Schools	70	70	70	70
Number of Observations	599	599	599	599

Notes: This table shows OLS estimates and the associated p-values on student outcomes measured after two years of exposure to the mentoring program under the first experiment run by the government. For detailed descriptions of the test scores used in this table, see Appendix A.1. The dependent variables are standardized with respect to their means and the standard deviations in the control group. p-values reported in brackets refer to the conventional asymptotic standard errors. p-values reported in braces are computed using randomization inference (randomization-t). p-values reported in parentheses are adjusted for testing the null impact of API Original across the four outcomes shown in the table through the step-wise procedure described in Romano and Wolf (2005a,b, 2016). All p-values account for clustering at the school level.

attainment (fifth column of Table 3).[12]

In spite of the differences in the measurement of children's academic achievement, the separate analyses of the two experiments show consistently inconclusive evidence regarding the effectiveness of the *Original* modality of the mentoring intervention. Depending on the outcome, the effect of the program on children in the first experiment ranges from positive to negative and is not statistically different from zero. The size of the estimated treatment effect on the overall index for academic achievement (column 4 of Table 2)—a Generalized Least Squares (GLS)-weighted average across the three subject tests that increases the power of the analysis (O'Brien, 1984)—is negative, small and imprecise.[13]

Effect sizes are positive and slightly more precise in the second experiment (see the first row of Table 3), although none of the estimated coefficients gets close to the conventional significance levels. The impact on the GLS-weighted overall index for student achievement across

[12]The national test that we employ in the first experiment (ENLACE) was administered to all Mexican students in grades three through six through the year 2013 (see Appendix A.1). The test was terminated in 2014, and so we cannot use it as a source of measurement for the academic achievement of the children that participated in the second experiment. Another national standardized test was administered by the National Institute for the Evaluation of Education (INEE) starting in 2015, the PLANEA National Plan for Learning Evaluation, although it was collected only in selected grades and in a random sample of students within schools.

[13]The GLS weighting procedure increases efficiency when compared to other summary indices by ensuring that outcomes that are highly correlated with each other receive less weight, while outcomes that are uncorrelated and thus represent new information receive more weight. This procedure is more powerful than other popular tests in the repeated-measures setting. Also, missing outcomes are ignored when creating the GLS-weighted score. Thus this procedure uses all the available data, but it weights outcomes with fewer missing values more heavily.

Table 3: Children's Achievement and Attainment—Second Experiment

	Survey-Based Test Scores				Admin Records	
	Reading	Math	Socio-emotional	Overall Index	Enroll Secondary	
API *Original*	0.126	0.056	0.071	0.126	0.073	0.081
	[0.104]	[0.455]	[0.418]	[0.182]	[0.255]	[0.519]
	{0.138}	{0.483}	{0.440}	{0.222}	{0.283}	{0.573}
	(0.147)	(0.554)	(0.554)	(0.240)	(0.312)	(0.469)
API *Plus*	0.315	0.237	0.199	0.368	0.124	0.298
	[0.001]	[0.008]	[0.022]	[0.001]	[0.074]	[0.030]
	{0.001}	{0.012}	{0.030}	{0.001}	{0.084}	{0.053}
	(0.001)	(0.005)	(0.011)	(0.001)	(0.032)	(0.032)
Original = *Plus*	[0.043]	[0.043]	[0.178]	[0.021]	[0.469]	[0.134]
	{0.086}	{0.115}	{0.225}	{0.024}	{0.570}	{0.229}
	(0.045)	(0.045)	(0.098)	(0.024)	(0.376)	(0.157)
Number of Schools	224	224	224	224	182	76
Number of Observations	1044	1044	1045	1045	468	106

Notes: This table shows OLS estimates and the associated p-values on student outcomes measured after two academic years of exposure to the API program under the second experiment designed and implemented by the authors in collaboration with the government. For detailed descriptions of the test scores used in this table, see Appendix A.2. The dependent variables in the first four columns are standardized with respect to their means and the standard deviations in the control group. The dependent variable in the last two columns is computed from administrative school records (see Appendix A.1). p-values reported in brackets refer to the conventional asymptotic standard errors. p-values reported in braces are computed using randomization inference (randomization-t). p-values reported in parentheses are adjusted for testing each null hypothesis (null impact of API Original, API Plus, and the comparison) for the each family of outcomes (survey-based and admin records) through the step-wise procedure described in Romano and Wolf (2005a,b, 2016). All p-values account for clustering at the school level.

the two cognitive measures and the socio-emotional score is 0.12 standard deviations—a non-negligible effect size that is nonetheless not statistically different from zero (p-value=0.23, after adjusting for multiple hypotheses testing). The effect of the *Original* modality of the mentoring program on the transition rates to lower secondary school are shown in the last two columns of Table 3. The estimated effect sizes are noisy, with an increase of 7-8 percentage points out of a basis of 62 percent enrollment rate in seventh grade in the control group.

The evidence consistently reveals that the API *Original* modality has not demonstrated substantial improvements in children's educational outcomes. The test statistic of the joint hypothesis of no effect across both experiments on schooling achievement has a p-value = 0.460. The lack of statistical significance of the effect of this specific mentoring approach among two independent and representative samples of schools may thus be indicative of a null result. These findings give rise to concerns about the potential impact and effectiveness of the mentoring program, which had already been implemented on a larger scale by the government.

3.3 Evidence on API *Plus*

The second row of Table 3 displays the estimated coefficients for the average impact of the *Plus* modality of the API program when compared to the control group. Children who are enrolled in schools that receive the *Plus* mentors increase their reading scores by 0.32 standard deviations (p-values ≤ 0.001). Quantitatively, the API *Plus* effect is approximately 2.5 times higher than the effect of the API *Original*. The difference between the two program effects is statistically different from zero after adjusting for multiple hypotheses testing (p-value=0.045).[14] We find similar patterns when we look at math scores (second column), which show a sizable and highly significant effect of the *Plus* modality with an estimated treatment effect of 0.24 standard deviations.

The API *Plus* program also generates a sizable improvement in the socio-emotional score of 0.2 standard deviations (third column). While the difference with respect to the *Original* modality is at the margin of statistical significance (p-value = 0.098), the larger effect of the *Plus* modality is consistent with qualitative evidence documenting that mentors with enhanced training acquired more effective skills to best deal with children's emotions during the bimonthly sessions.[15] The effect size of the *Plus* modality on the GLS-weighted index of achievement displayed in the fourth column of Table 3 is very large, 0.37 standard deviations—precisely estimated (p-values ≤ 0.001), and statistically different at the 95 percent level from the effect of the *Original* modality.[16]

The last two columns in Table 3 reports the estimated effects on the average transition rate to secondary school. Less than two-thirds of the sixth graders in the control group enroll in seventh grade, while the corresponding national average is 95 percent. The API *Plus* modality increases the probability of a child's enrolling in seventh grade by 12 percentage

[14]The Romano–Wolf correction (Romano and Wolf, 2005a,b, 2016) asymptotically controls the family-wise error rate, that is, the probability of rejecting at least one true null hypothesis among a family of hypotheses under test. This correction is considerably more powerful than earlier multiple-testing procedures, given that it takes into account the dependence structure of the test statistics by re-sampling from the original data.

[15]We conducted a series of in-depth interviews in the spring of 2022 for a small and representative sub-sample of 16 mentors and 12 community instructors who were part of our study. Appendix A.3 reports more details about these interviews. Tables A-1 and A-2 show that the characteristics of these survey respondents are broadly comparable to those of the mentors and the local instructors in the second experiment.

[16]In Appendix Table B-8 we report the results by sub-domains of the reading scores (panel A), math scores (panel B). While the estimates are erratic and not statistically significant for the *Original* modality, the *Plus* modality is shown to increase students' proficiency in reading across various domains (familiar-word reading, reading comprehension, and dictation). For math scores, the *Plus* modality seems particularly effective on numbers' identification and discrimination as well as additions. Similarly, in Appendix Table B-9 we report the effects of the two program modalities for each individual component of the socio-emotional score.

points. This effect on education attainment is precisely estimated (p-value=0.032, after accounting for multiple hypothesis testing) and quantitatively sizable, as it represents a 20 percent increase in the share of students who transit to secondary school relative to the mean in the control group. The size of the effect more than doubles when we focus on the sub-sample of over-aged sixth graders (13 years old or more, sixth column). Given recent longitudinal evidence on the labor market returns associated to the primary-to-secondary schooling transition in Mexico (Araujo and Macours, 2021), our estimated effect sizes on schooling attainment are particularly important in terms of life-cycle opportunities.

We finally investigate the extent to which the positive effects of the API *Plus* modality of the mentoring program on children's outcomes can be sustained at a larger scale. Secondary school is a critical period for the educational outcomes of the disadvantaged population under study, as more then a quarter of the children aged 12 to 14 in Chiapas are out of school. The first column of Table 4 shows that the program increases the fraction of children who enroll in secondary education by 5.6 percentage points (p-value = 0.013) for the sample of schools that did not previously participate in the second experiment. This represents an increase of 7.6 percent with respect to the sample mean. For the schools that were previously part of the experiment, the impact of receiving the program during the government implementation is larger (+9.1p.p., p-value = 0.035, see third column in Table 4). These effects of the mentoring intervention during the government's program implementation are statistically similar across the two school sub-samples and they are in line with the experimental findings of the API *Plus* modality on the enrollment in seventh grade documented in the fifth column of Table 3.[17]

The estimates of the impact of the government-run API *Plus* mentoring modality on child literacy are displayed in the even columns of Table 4. This is another relevant education outcome for the disadvantaged communities that are targeted by the intervention, which is akin to the achievement test scores reported in Tables 2 and 3. In our sample, 13 percent of school-aged children are still illiterate. After two years of exposure, we find that villages that received mentors display a 2.8 percentage points (p-value = 0.013) increase in child literacy rates when compared to villages without mentors. The magnitude of this effect implies a reduction of illiteracy rates by 21 percent with respect to the sample average. The estimated program effect for the sub-sample of experimental schools is quantitatively similar, although

[17]The census-based information reported in Table 4 represents the locality-level stock (rates) of children enrolled in secondary school in a given year, while our measure of enrollment in seventh grade (see Table 3) represents the flow of new students enrolling in any secondary schools.

Table 4: Children's Achievement and Attainment—API *Plus* Scale-up

	Non-Experimental Schools		Experimental Schools	
	Enroll Secondary	Child Literacy	Enroll Secondary	Child Literacy
API *Plus*	0.056	0.028	0.091	0.035
	[0.010]	[0.012]	[0.022]	[0.078]
	{0.013}	{0.013}	{0.022}	{0.068}
	(0.013)	(0.013)	(0.035)	(0.054)
Number of Schools	1161	1161	184	184

Notes: This table shows OLS estimates and the associated robust p-values on locality-level outcomes measured after two years of exposure to the API *Plus* modality of the mentoring program under the government implementation. For detailed descriptions of the outcome variables used in this table, see Appendix A.1. Control variables include indicators for the whether or not the locality satisfy the program assignment criteria, an indicator variable for prior exposure to the API *Original* modality, and the number of hostile event related to land property, religion, elections, crime, or drug addiction as reported at the locality level in the population census (2010). p-values reported in brackets refer to the conventional asymptotic standard errors. p-values reported in braces are computed using randomization inference (randomization-t). p-values reported in parentheses are adjusted for testing the null impact of API Plus for the two different sub-samples of schools (non-experimental and experimental) through the step-wise procedure described in Romano and Wolf (2005a,b, 2016).

a bit noisier (+3.5 percentage points, p-value = 0.054).

Both across different samples of schools under the same program modality and across different situations for the same sample, our findings overwhelmingly support the notion that the API *Plus* program has improved the education outcomes for children in these disadvantaged communities. This interpretation is corroborated by the test statistics for the joint hypothesis of no effect for this modality of the mentoring program on schooling achievement and attainment, which are highly significant (p-values = 0.0001 and 0.001, respectively). Furthermore, the program's impacts endure beyond the conclusion of the two-year intervention, enhancing its potential for scalability. Leveraging the initial randomization from the second experiment, Appendix Figure B-1 shows that the effect of the *Plus* program on secondary school enrollment continues beyond the two-year time frame of the program's cycle. Appendix Figure B-2 instead shows that extended exposure to the program beyond the initial two-year cycle further enhances the program's impact.

4 Challenges and Pathways to Scale

Despite the substantial impact of the mentoring program on supporting students and improving their educational outcomes, there are potential risks associated with the government's conversion of infrastructure for the large-scale implementation of the *Plus* modality. The

literature discusses various mechanisms that can cause a voltage drop in the new situation (Al-Ubaydli et al., 2020). In this section, we first outline specific aspects in the implementation protocol of the mentoring program that may have led to contrasting outcomes between the experimental phase and the subsequent government implementation. We next study the possible mechanisms behind the success of the *Plus* modality of the mentoring intervention using an array of survey modules collected during the two field experiments. This analysis aims to shed light on the pathways that likely facilitated the program's scalability.

4.1 Program Implementation Fidelity

The fidelity of the training and supervision might fall during the national program implementation even when scaling-up does not require hiring and training an increased number of service providers. There were two differences in terms of how mentors were recruited and assigned to communities within the randomized trial when compared to the on-going government intervention (API *Original*). First, the most important criterion for the assignment of the mentors was the ability to speak the main indigenous language in the community. Second, supervisors of the mentors received a salary increase in exchange for an obligatory increase in the frequency of their visits to the targeted communities. The extent to which these implementation changes were later adopted by government during the scale-up of the API *Plus* modality can potentially influence the effectiveness of the mentoring service.

We begin by examining the extent to which the population of mentors is similar between the experiment and the scale-up. To do so, we integrate the survey data on mentors from the second experiment with the program-roster data of mentors during the scale up.[18] Despite limited set of common variables across these two datasets, Appendix Table B-10 demonstrates that the observable traits of mentors in our experiment are similar to those of mentors in the program's scale-up. Gender, age, and the percentage of mentors who speak an indigenous language are evenly distributed across settings, which lends at least some empirical support to the notion that the recruitment practices used during the program's scale-up were consistent with those used in the experiment.

The presence of similar populations of mentors across different situations does not necessar-

[18]The survey data comprises a total of 139 mentors, while the program-register data includes 441 mentors. The number of mentors exceeds the number of schools because the survey included both mentors that were assigned to schools, as well as those who were awaiting a role. In the 2016 survey, for instance, the 139 mentors were either assigned to 107 unique schools included in the survey, or they were currently awaiting a role within the program.

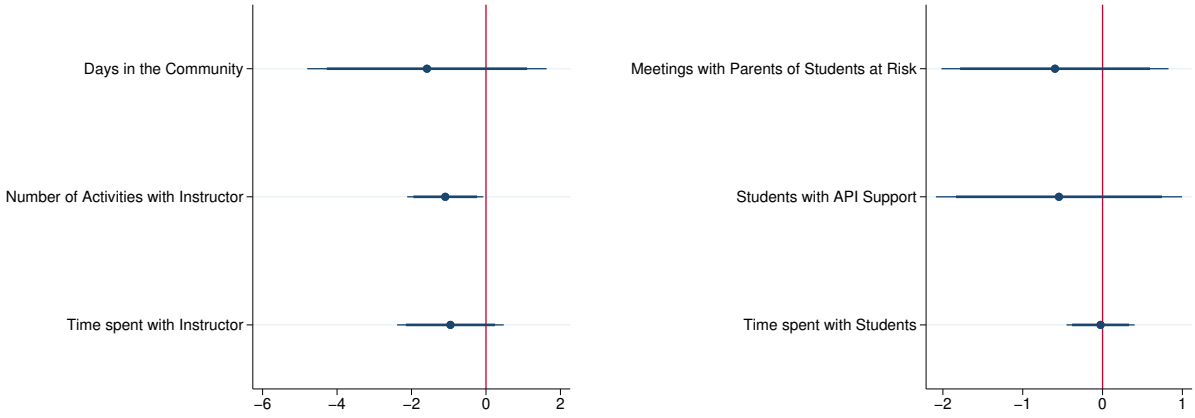
ily imply consistency in mentoring practices. Differences in incentive structures and training modules between the government implementation and the field experiment could potentially impact both the quantity and quality of the mentoring service. To examine this, we leverage survey data in our experimental schools on various topics related to the schooling environment, with a specific focus on the activities of mentors. We use data from two survey rounds that record instructor-reported measures of mentoring practices from 56 and 58 schools, respectively, that were part of the *Plus* program (see Appendix A.2 for further details on the surveys) in order to test the hypothesis that mentoring practices underwent significant changes during the government's scale-up.

The estimates displayed in Figure 2 represent the difference in means between the two survey periods, and relative inference, whereby the first period denotes the experimental setting and the second period denotes the scale-up regime. Panel A of the figure examines the quantity aspect of the mentoring service in more detail. Overall, the point estimates are negative, but generally small and noisy. The first variable shown in this panel is the number of days that mentors spent in the community during their last visit. The coefficient for this variable is -1.58, which indicates that, on average, during the government implementation mentors spent 1.5 fewer days in the communities (of the 14-day visit) compared to the experimental setting. The second variable of Panel A is the number of activities (ranging from zero to five) that the mentor carries out with the local instructor in the current school year.[19] We observe that mentors, in comparison to the field experiment, decrease the number of pedagogical training activities provided to teachers by approximately one in the current school year. The third variable indicates a decrease in the amount of time mentors spend with local instructors across the two scenarios. Specifically, mentors spend one minute less during their last visit to the community. In two out of three cases we cannot reject the null hypothesis of zero effect at conventional levels of significance.

In terms of the quality of the mentoring programs, our results also show a small and statistically insignificant reduction in our observed measures between the field experiment and the government setting. The estimates of the mean differences across situations are shown in Panel B of Figure 2. Both the number of meetings with parents of under-performing students (-0.60) and the number of students benefiting from the mentor support (-0.55) decreased dur-

[19]This measure represents the total number of activities that are completed by the mentor out of the following five: (i) talking with students about the school and their families; (ii) going over the diagnostic tests to students; (iii) explaining the pedagogical practices to the teachers; (iv) explaining to the teachers what to do to improve the performance of their classroom; and (v), supporting the teacher in the creation of the classroom materials.

Figure 2: Differences in the Mentoring Practices Between Experiment and Scale-up



Panel A: Quantity

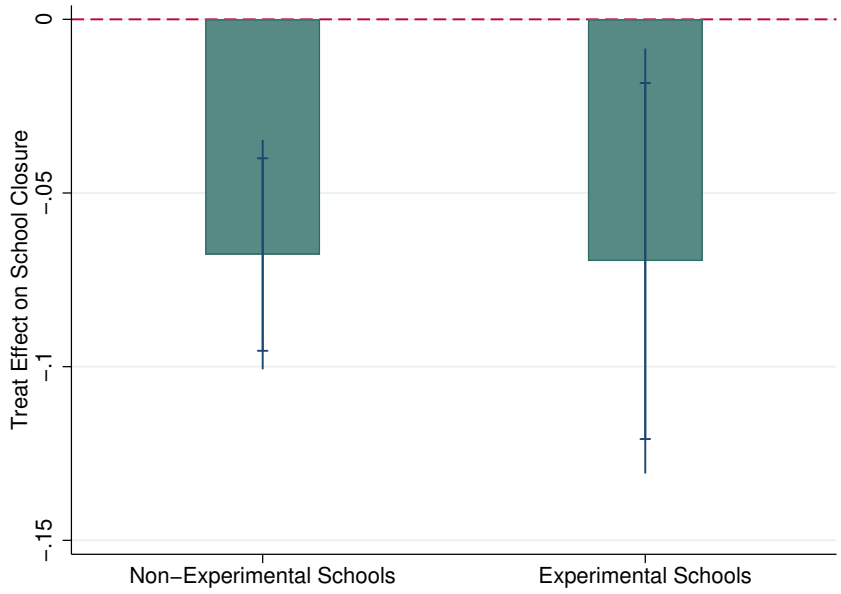
Panel B: Quality

Notes: The figure shows the comparison in the quantity and quality of API-*Plus* mentors between the second experiment and the government implementation. This information is collected during the surveys of the local instructors, in the school years 2015-2016 and 2018-2019. Each dot in the figure represents an OLS estimate for the difference in the mentoring services across the two situations, whereas the horizontal bars are the associated 90% and 95% confidence intervals. The associated table with the OLS estimates, p-values, and number of observations are also reported in Appendix Table B-11. All the regressions include the same set of controls as in Table 4.

ing the government scale-up of the API *Plus* intervention. Finally, when considering the time that mentors spent with children during their last visit in the communities, our results suggest no change in mentoring practices. Mentors spend the same amount of time (minutes) with students both in the field experiment compared with the scale-up regime.

The continuity of school services is vital for maintaining the program's effectiveness, as schools serve as the conduit for delivering the mentoring service. Consequently, the occurrence of school closures can significantly disrupt the program. Some institutional details make school closures more salient during the government implementation of the mentoring intervention when compared to the experimental situation. For example, the decision of closing schools is determined by the parent association with a vote. Whenever the number of students enrolled drops below six the school ceases to operate by default, unless the majority of parents oppose by vote. Schools in the second experiment were allowed to remain open if they had at least three enrolled students in either of the two school years of the study period. As a result, only two schools closed in the experimental sample of 230 schools, compared with a an average 11 percent school closing rate in the rest of Chiapas for the three years before the experiment, and with a 19 percent probability of school closures for schools with a size below the median.

Figure 3: The Impact of the API *Plus* Program on School Closures



Notes: The bars in the figure represents the OLS estimates of the assignment to the API program during the government implementation of the *Plus* modality (same as in Equation 1) on the rate of school closures as measured over the subsequent two years. Vertical lines overlaid on each bar display the 95 percent and 90 percent confidence intervals, respectively. Confidence intervals are based on asymptotic inference. The OLS estimates, p-values, and number of observations for the two subsamples of schools are also reported in Appendix Table B-12.

On one hand, it is conceivable that the program could fail when implemented on a larger scale due to the potential of school closures. On the other hand, if the *Plus* program successfully prevents the adverse event of school closures during the government implementation, it presents us with a valuable opportunity to gain insights into the mechanisms that enhance the scalability of this program modality when compared to the previous modality. To gain insights into this potential threat to scalability, we adopt the same regression model (1) and the same sample of schools previously used to evaluate the mentoring intervention on children's outcomes at scale (see Table 4). In this analysis, the outcome of interest is whether a school results permanently closed from the administrative school census during the fall of 2019.

Figure 3 shows that the government implementation of the *Plus* modality induces a significant and large effect, in percent terms, on school closures.[20] Both experimental schools and non-experimental schools in Chiapas exhibit similar patterns of school closures. When fo-

[20]For the overall sample of schools in the scale-up analysis (both experimental and non-experimental schools), only 1.6% of the schools with API-*Plus* mentors are permanently closed after two years of exposure to the government-run program against 9.1% of closures among schools without mentors.

cusing on the schools outside of the experimental sample in Chiapas (N=1,161), we observe a 6.8 percentage point reduction in the probability of school closures due to the program (p-value < 0.001). Schools that were previously part of the experimental sample (N=184) also experience a notable decrease in school closures during the government implementation of the API *Plus*, with an average impact of the mentoring program of -7.0 percentage points (p-value $= 0.026$).

The conversion of the program from a field experiment to government implementation has the potential to create significant disruptions. The evidence does not support the notion of a severe decline in the mentoring practices in the scale-up phase. Our previous findings on the impact of the *Plus* program on educational outcomes documented in Table 4 and the evidence on school closures discussed in this section are consistent with the hypothesis that the program's underlying effectiveness endures during the government implementation.

4.2 *Plus* vs. *Original*: Channels

In this sub-section, we study the possible mechanisms behind the success of the *Plus* modality of the mentoring intervention using an array of survey modules collected during the two field experiments.[21] Table 5 presents the average impact of the program on GLS-weighted indices of parental investment in their children's education (see Appendix A.2).[22] Panel A displays the estimates of the *Original* modality in the first experiment, while Panel B shows the corresponding figures for both the *Original* and *Plus* modality in the second experiment. Under the API *Original* program, consistently across experiments, the estimates are not statistically different from zero, with signs of the coefficients that range from positive to negative and effect sizes on the overall index of -0.03 and 0.1 standard deviations.

Parents appear to be systematically more invested in their children's education activities under the *Plus* modality of the mentoring program. The estimates reported in Panel B of Table 5 document that mentors with enhanced training are more effective in boosting

[21] As discussed in Section 2.3, the sample of schools of the second experiment is largely representative of the broader population of schools in the State of Chiapas in terms of observable characteristics (see Tables 1 and B-4) as well as in terms of program impacts (see Table 4 and Figure 3).

[22] We also estimate the impacts of both the *Original* and *Plus* modalities for each of the individual measures of the parental behavior collected in the survey that have been aggregated in the summary measures displayed in Table 5. Appendix Table B-13 reports the results, which are broadly comparable to the estimates discussed in the text. They show large and significant effects for the *Plus* modality on food donations to the instructors, the management of the school resources, help with homework, enrolling their children in extra-curricular activities, expecting their children to complete secondary education or more, and meet periodically with the instructor.

Table 5: Parental Investment

	Panel A: First Experiment			
	Engage at School	Manage School Resources	Engage With Child	Overall Index
API *Original*	0.198	-0.135	0.149	0.101
	[0.259]	[0.415]	[0.399]	[0.580]
	{0.261}	{0.422}	{0.399}	{0.578}
	(0.338)	(0.511)	(0.511)	(0.511)
Number of Schools	73	73	73	73
Number of Observations	208	208	208	208
	Panel B: Second Experiment			
	Engage at School	Manage School Resources	Engage With Child	Overall Index
API *Original*	-0.188	-0.124	0.167	-0.034
	[0.049]	[0.176]	[0.015]	[0.684]
	{0.058}	{0.197}	{0.015}	{0.630}
	(0.067)	(0.205)	(0.021)	(0.704)
API *Plus*	0.217	0.087	0.353	0.359
	[0.034]	[0.344]	[0.001]	[0.001]
	{0.037}	{0.247}	{0.001}	{0.001}
	(0.055)	(0.388)	(0.001)	(0.002)
Original = *Plus*	[0.001]	[0.056]	[0.029]	[0.001]
	{0.001}	{0.056}	{0.158}	{0.001}
	(0.002)	(0.036)	(0.036)	(0.001)
Number of Schools	224	224	224	224
Number of Observations	1045	1045	1045	1045

Notes: This table shows OLS estimates and the associated p-values on survey-based measures of parental behavior measured after two years of exposure to the API program. Panel A refers to the first experiment run by the government. Panel B refers to the second experiment designed and implemented by the authors in collaboration with the government. For detailed descriptions of the individual components of the summary measures of parental engagement used in this table, see Appendix A.2. p-values reported in brackets refer to the conventional asymptotic inference. p-values reported in braces are computed using randomization inference (randomization-t). p-values reported in parentheses are adjusted for testing each null hypothesis (null impact of API *Original*, API *Plus*, and the comparison) for the two different families of outcomes through the stepwise procedure described in Romano and Wolf (2005a,b, 2016). All p-values account for clustering at the school level.

parental engagement, both toward the school and directly with the child. The point estimates are positive throughout. After correcting inference for multiple hypotheses testing, two out of four coefficients are statistically significant at the 1 percent level, with a very large effect size for the overall index of parenting practices of 0.36 standard deviations. We can reject the null hypothesis of equal treatment effects on all four parental outcomes.

Home visits are a key component of the mentoring intervention under study. The goal of these visits as well as other encounters between mentors and parents in the school's premises is to increase parental awareness about their children's educational trajectories through periodic interactions. We study the role of the these interactions as a potential mechanism behind

the observed effect of the *Plus* modality on parental investment.

Panel A in Table 6 displays the estimated differences across the two API modalities on selected survey variables when parents were asked about the frequency and content of their interactions with the mentors over a period of two months prior to the survey—parents in the control group cannot be part of this analysis by design.[23] The evidence shows a clear pattern in spite of quite noisy estimates due to missing observations and the reduced sample size. Over a two-month period, mentors in the *Plus* modality met one time more with parents at school and 0.7 times more at home compared to those in the *Original* modality (sample means in the *Original* group are five and three, respectively). The GLS-weighted index shown in the third column documents that the quantity of parent-mentor interactions increased by 0.36 standard deviations under the *Plus* modality, which is significant at the 10 percent level. The last two columns of Panel A show marginally significant estimates on two measures of the quality of the interactions between parents and the mentors: (i) an indicator variable for whether the mentors have informed parents about their children's learning difficulties, (ii) and whether the mentors provide concrete advice to the parent on how to tackle these difficulties. The effect sizes are large for both outcomes, implying a 14 percent increase in the probability of informing parents relative to the respective sample means in the *Original* group (70 percent). The estimated coefficient for the GLS-weighted quality index is 0.25 standard deviations, which is significant at the 5–10 percent level depending on the inference procedure.

Panel B in Table 6 shows the effect of the API *Plus* on different competencies, or "parenting styles," that the mentors report to have promoted during their encounters with parents (see Appendix A.2).[24] Mentors with enhanced training are more inclined to foster attitudes that are centered on educative parenting styles, such as communicating with the child (first column), as well as learning activities (second column). The overall educative style GLS-weighted index (third column) shows a sizable and significant effect (across the three inference procedures) of the *Plus* modality, with an increase of 0.49 standard deviations in the promotion of educative parenting styles to parents during the home visits. The estimates in the last four columns of Panel B cover different aspects of the parent-child relationship, par-

[23]The number of observations varies across the columns in Panel A due to some of the 591 interviewed parents not responding to the survey questions. Missing values for each outcome are balanced with respect to the assignment of the API *Plus* (p-values = 0.746, 0.183, 0.442, 0.517, 0.539, and 0.575).

[24]Of a total of 126 schools that received mentors between the *Original* and *Plus* modalities, our survey enumerators were able to collect information for 107 schools. The attrition of survey participation is unrelated to the treatment assignment (p-value = 0.514).

Table 6: The Role of Mentors in Fostering Parental Attitudes—Second Experiment

	Panel A: Parents and Mentors Interactions (as reported by the parents)					
	Quantity (Last 60 Days)			Quality		
	Meetings	Visits	Index	Inform About Child	Advise About Child	Index
API *Plus*	1.039	0.726	0.362	0.102	0.100	0.251
	[0.147]	[0.125]	[0.062]	[0.057]	[0.034]	[0.040]
	{0.194}	{0.171}	{0.094}	{0.097}	{0.056}	{0.070}
	(0.194)	(0.194)	(0.100)	(0.078)	(0.078)	(0.078)
Number of Observations	482	491	504	354	353	357

	Panel B: Parenting Styles that Are Promoted by the Mentors (as reported by the mentors)						
	Educative Style			Emotional Style			
	Communication	Learning	Index	Share Feelings	Self-Knowledge	Manage Transitions	Index
API *Plus*	0.178	0.168	0.494	0.049	0.030	0.142	0.194
	[0.038]	[0.077]	[0.018]	[0.627]	[0.756]	[0.123]	[0.312]
	{0.043}	{0.091}	{0.029}	{0.635}	{0.753}	{0.134}	{0.321}
	(0.074)	(0.075)	(0.043)	(0.843)	(0.843)	(0.308)	(0.558)
Number of Observations	107	107	107	107	107	107	107

Notes: This table shows OLS estimates and the associated p-values of the API *Plus* modality on survey-based measures of interactions between parents and mentors (Panel A) and the different parenting styles that are promoted by the mentors during their interactions with the parents. For a detailed description of the outcome variables used in this table, see Appendix A.2. p-values reported in brackets refer to the conventional asymptotic inference. p-values reported in braces are computed using randomization inference (randomization-t). p-values reported in parentheses are adjusted for testing the effect of API *Plus* for the different families of outcomes (quantity and quality of interactions, parenting styles) through the stepwise procedure described in Romano and Wolf (2005a,b, 2016).

ticularly emotional practices. Our results show positive effects in this area of parent-mentor interactions, although these effects are statistically insignificant.

These findings point toward cross-modality variation in the quality of both the parent/mentor interactions and parent/child interactions as a potential mechanism behind the observed difference in parental investment as well as in children's outcomes. Although we are unable to precisely quantify the individual impact of each training module, it is probable that these effects can be attributed to the parenting skill training modules and the peer-to-peer sessions facilitated by mentors. Instead, the extra week of initial training is focused on pedagogical practices targeted to children at school. Qualitative evidence seems indeed to corroborate this hypothesis. We report here a few quotes from mentors who have participated to the training sessions of the *Plus* modality (see Appendix A.3 for more details):

- *"During the workshops I was told that I should be able to adapt to the context of the community and understand the local living arrangements in order to establish a dialog with the parents without modifying what they conceive as their environment."*

- *"It was recommended that we pay frequent home visits so as to establish a relationship with the parents and gain their trust."*

- *"[The workshops] exposed us to effective strategies of other mentors [for dealing with parents] that we could try and implement in our community."*

We evaluate the role of other possible channels related to the mentoring service that might partially account for the effectiveness of the *Plus* program compared to the *Original* modality. In particular, we focus on the main tasks of the mentors in the school communities beyond parental involvement: (i) remedial education sessions with students lagging behind; and (ii), pedagogical support to the local instructors. Although the design of the second experiment does not allow us to isolate the direct effect of the remedial education sessions within each API modality, we exploit the discontinuity in the eligibility of children for the remedial sessions (see Section 2.1 for details on the eligibility). The estimates displayed in Appendix Table B-14 suggest that there is no differential effect across achievement outcomes in the relative impact of the two training modalities between children who are more or less likely to be eligible for the remedial sessions (see also Appendix Figure B-3).

We next consider the role of the pedagogical practices of the community instructors. Because mentors provide help in improving their teaching habits, we test the hypothesis of whether this factor may partly explain the differential effect of the *Plus* modality on children's outcomes. Appendix Table B-15 reports estimates of the effect of the API *Original* and API *Plus* using data at the school-level on four summary measures of pedagogical practices based on GLS-weighted indices across an array of instructor-student interactions (for details, see Appendix A.2).[25] The results show erratic patterns of positive and negative signs with no statistically significant effects of either API modality.

In summary, differences in effectiveness across modalities of remedial education sessions or variations in pedagogical support for instructors are unlikely to account for the success of the *Plus* program. The available evidence suggests that a more active parental involvement, which was likely triggered by enhanced parent-mentor interactions, played a central role.

4.3 Parents as Potential Means of Scalability

Given the discussion in Sections 4.1 and 4.2, we next explore the link between school closures and the engagement of parents with the school community under the randomized program assignment. The functioning of the community-based schools under study is heavily reliant

[25]The sample average number of instructors per school is 1.2 in the school year prior to the start of the second experiment.

on the active involvement of parents through the local parental association. In particular, the association rules over the decision of whether or not to close the school, a situation that is automatically considered when the number of students enrolled in the school drops below six (see Section 2.1). Because school closures can undermine the success of the API *Plus* mentoring modality outside of the experimental conditions, this effectively implies that parents can play a crucial role in the scalability of the mentoring program.

We explore this possibility by examining whether or not the contrasting responses in parental investment across the two mentoring interventions, as shown in Table 5, are reflected in differential rates of school closures between the two program modalities. The first two columns of Table 7 show the reduced-form effects of the two randomized program modalities—in both the first experiment (first column) and the second experiment (second column)—on the probability that schools close two years after the program intervention. The *Original* modality displays small and noisy effects on school closures in both experiments, which are not statistically different from zero. This finding supports the notion that situations characterized by a lack of parental engagement—as indicated by our previous results—are not conducive to the effectiveness of community-based educational programs.

The second column of Table 7 shows that the *Plus* modality, which substantially boosts parental engagement during the experiment, has a large and significant impact on school closures two years after the *Plus* modality was adopted by the government. Schools are 8.3 percentage points less likely to close (p-value=0.030). This result echoes previous evidence on the relationship between the probability of closures for schools that receive a mentor during the government implementation of the *Plus* modality, which is shown in Figure 3.

The IV estimates shown in the third column of Table 7 go a step further and quantify the causal effect of parental engagement on the probability of school closures. An increase of 0.1 of a standard deviation in the overall parental engagement index is associated with a reduction of 2.2 percentage points in the probability that their children experience a school closure (p-value=0.021). We propose three main reasons why it seems plausible to assume that parents are the primary channel through which the *Plus* modality of the API program influences school closures. First, contextual information points to the role of the parental association in deciding school closures. The role of parents in ensuring continuity in schooling

Table 7: School Closures and Parental Engagement

	Outcome: School Closures		
	First Experiment	Second Experiment	Second Experiment, IV
API *Original*	0.063	-0.031	-0.031
	[0.225]	[0.396]	[0.410]
API *Plus*		-0.083	
		[0.030]	
Overall Parental Engagement			-0.217
			[0.021]
Observations	73	224	1045
Clusters	.	.	224
F-Stat (Excl. Instrument)			13.833

Notes: This table reports the estimates for the reduced-form effects of the API modalities during the two experiments (columns 1 and 2) on the probability of school closures, as well as the instrumental variable estimates of the impact of parental engagement on school closures. In the third column, the randomized API *Plus* modality during the second experiment is used as an instrumental variable, while the randomized API *Original* modality is included as a control variable. The dependent variable is an indicator variable for whether the school is closed in the fall of 2014 (column 1) or in the fall of 2018 (columns 2 and 3). The variable "Overall Parental Engagement" is the same variable used in the last column of Table 5. *p*-values reported in brackets refer to the robust asymptotic inference.

activities clearly emerges in the qualitative evidence.[26] Second, our findings reported in Section 4.2 reject the hypothesis that other behavioral responses by teachers and students may mediate the effect of the API-*Plus* program on school closures. Third, the absence of any impact from the *Original* mentoring program in both independent field experiments on parental investments and school closures (see Tables 5 and 7) serves as further corroboration that when parental investments are not boosted by the intervention, the underlying impact on school closures is muted.

Taken together, the evidence presented in this subsection is consistent with the hypothesis that the effectiveness of the mentoring intervention during the large-scale implementation of the program likely depends on the active involvement of parents in educational activities. Within the new program modality, parents not only increased their interactions and investment with children—a shared result among past successful interventions (Heckman and

[26] As reported by the local instructors, engaged parents may have more at stake in keeping the schools open as they invest in durable goods for the local school: *"[Parents] help manage the school and contribute by improving the fencing, painting the walls, fixing the toilets, as well as buying school materials." "[Parents] serve the needs of the school with construction works and they provide food to the local instructor."* As reported by the mentors, parents follow up with their children on homework and other pedagogical material whenever the mentor is busy attending tasks outside of the community: "*Parents used to provide support with homework whenever mentors are visiting other communities ensuring pedagogical support, so that upon the return of the mentors the children are able to make progress in the schooling activities without setbacks.*"

Mosso, 2014; Zhou et al., 2021; García and Heckman, 2023)—but also they intensified their engagement at the school and community level. Parental responses are shown to prevent schools from closing, which would otherwise pose a threat to the scalability of the program during government implementation.

5 Conclusion

This paper seizes a unique opportunity to investigate the challenges and determinants of scaling when transitioning an educational intervention from a field experiment to government implementation. In the context of a school mentoring program in the state of Chiapas, Mexico, we show that relatively minor variations in the training content of mentors can lead to large changes in schooling outcomes. While the government's original implementation of the program proves to be largely ineffective, an alternative approach that prioritizes mentors' ability to effectively interact with and engage parents was successful in enhancing test scores and improving educational attainment for the students in our sample. The magnitudes of the estimated impacts are comparable across situations (field experiment versus government implementation) as well as across study samples (experimental schools versus the rest of the schools in Chiapas).

We acknowledge the limitations of the empirical analysis in addressing the "vertical" aspect of scaling, as outlined in List (2022), which would involve supply-side considerations for the implementation of the program at a larger scale. One possible contextual shortcoming is that the intervention under study relies on university graduates as mentors. This feature may hinder the extent to which the program can be scaled up in settings where human resources with relatively high levels of human capital are scarce. Finally, while we underscore the pivotal role that local communities and parents play in promoting the success of education interventions, our evidence is merely suggestive on the channels through which this particular program remains successful when implemented at scale. More research is needed to further explore the complex social dynamics triggered by large-scale interventions.

References

Agostinelli, Francesco, "Investing in Children's Skills: An Equilibrium Analysis of Social Interactions and Parental Investments," 2018.

_ **, Matthias Doepke, Giuseppe Sorrenti, and Fabrizio Zilibotti**, "It Takes a Village: The Economics of Parenting with Neighborhood and Peer Effects," Working Paper 27050, National Bureau of Economic Research April 2020.

Al-Ubaydli, Omar, John A. List, and Dana Suskind, "2017 Klein Lecture: The Science of Using Science: Toward an Understanding of the Threats to Scalability," *International Economic Review*, 2020, *61* (4), 1387–1409.

_ **, Min Sok Lee, John A. List, Claire Mackevicius, and Dana Suskind**, "How can experiments play a greater role in public policy? Twelve proposals from an economic model of scaling," *Behavioural Public Policy*, 2021, *5* (1), 2–49.

Allcott, Hunt, "Site Selection Bias in Program Evaluation," *The Quarterly Journal of Economics*, 2015, *130* (3), 1117–1165.

Araujo, Maria Caridad and Karen Macours, "Education, Income and Mobility: Experimental Impacts of Childhood Exposure to Progresa after 20 Years," PSE Working Papers halshs-03364972, HAL October 2021.

Attanasio, Orazio, Helen Baker-Henningham, Raquel Bernal, Costas Meghir, Diana Pineda, and Marta Rubio-Codina, "Early Stimulation and Nutrition: The Impacts of a Scalable Intervention," *Journal of the European Economic Association*, 01 2022.

_ **, Sarah Cattan, and Costas Meghir**, "Early Childhood Development, Human Capital, and Poverty," *Annual Review of Economics*, 2022, *14* (1).

August, Gerald, Michael Bloomquist, Susanne Lee, George Realmuto, and Joel Hektner, "Can Evidence-Based Prevention Programs be Sustained in Community Practice Settings? The Early Risers? Advanced-Stage Effectiveness Trial," *Prevention science : the official journal of the Society for Prevention Research*, 07 2006, *7*, 151–65.

Banerjee, Abhijit V., Rukmini Banerji, James Berry, Esther Duflo, Harini Kannan, Shobhini Mukerji, Marc Shotland, and Michael Walton, "From Proof of

Concept to Scalable Policies: Challenges and Solutions, with an Application," *Journal of Economic Perspectives*, November 2017, *31* (4), 73–102.

Becker, Gary S., "Investment in Human Capital: A Theoretical Analysis," *Journal of Political Economy*, 1962, *70* (5), 9–49.

Bobba, Matteo and Jérémie Gignoux, "Neighborhood Effects in Integrated Social Policies," *World Bank Economic Review*, 2019, *33* (1), 116–139.

_ , Veronica Frisancho, and Marco Pariguana, "Perceived Ability and School Choices: Experimental Evidence and Scale-up Effects," IZA Discussion Papers 16168, Institute of Labor Economics (IZA) May 2023.

Bold, Tessa, Mwangi Kimenyi, Germano Mwabu, Alice Ng'ang'a, and Justin Sandefur, "Experimental Evidence on Scaling Up Education Reforms in Kenya," *Journal of Public Economics*, 2018, *168* (C), 1–20.

Bruns, Barbara and Javier Luque, *Great Teachers : How to Raise Student Learning in Latin America and the Caribbean*, Washington, DC: World Bank, 2015.

Cameron, Lisa, Susan Olivia, and Manisha Shah, "Scaling Up Sanitation: Evidence from an RCT in Indonesia," *Journal of Development Economics*, 2019, *138*, 1–16.

Caron, EB, Kristin Bernard, and Allison Metz, "Fidelity and Properties of the Situation, Challenges and Recommendations," in "The Scale-up Effect in Early Childhood and Public Policy. Edited by John A. List, Dana Suskind, and Lauren H. Supplee," Routledge, 2021.

CONEVAL, "Medición de Pobreza 2008-2018, Estados Unidos Mexicanos," 2018.

Cunha, Flavio, James J. Heckman, and Susanne M. Schennach, "Estimating the Technology of Cognitive and Noncognitive Skill Formation," *Econometrica*, 2010, *78* (3), 883–931.

Davis, Jonathan, Jonathan Guryan, Kelly Hallberg, and Jens Ludwig, "Studying Properties of the Population: Designing Studies that Mirror Real World Scenarios," in "The Scale-up Effect in Early Childhood and Public Policy. Edited by John A. List, Dana Suskind, and Lauren H. Supplee," Routledge, 2021.

Doepke, Matthias and Fabrizio Zilibotti, "Parenting With Style: Altruism and Paternalism in Intergenerational Preference Transmission," *Econometrica*, September 2017, *85*, 1331–1371.

Dubeck, Margaret M. and Amber Gove, "The Early Grade Reading Assessment (EGRA): Its Theoretical Foundation, Purpose, and Limitations," *International Journal of Educational Development*, 2015, *40*, 315–322.

Duflo, Annie, Jessica Kiessel, and Adrienne M Lucas, "Experimental Evidence on Four Policies to Increase Learning at Scale," *The Economic Journal*, 02 2024, p. ueae003.

García, Jorge Luis and James J. Heckman, "Parenting Promotes Social Mobility Within and Across Generations," *Annual Review of Economics*, 2023, *15* (1), null.

Heckman, James, "Randomization and Social Policy Evaluation," in "Evaluating Welfare and Training Programs. Edited by C. F. Manski and I. Garfinkel," Harvard University Press, 1992.

_ **and Jin Zhou**, "Interactions as Investments: The Microdynamics and Measurement of Early Childhood Learning," Working Paper, Center for the Economics of Human Development, University of Chicago 2021.

Heckman, James J. and Stefano Mosso, "The Economics of Human Development and Social Mobility," *Annual Review of Economics*, 2014, *6*, 689–733.

List, John A., *The Voltage Effect: How to Make Good Ideas Great and Great Ideas Scale*, Penguin Books, 2022.

List, John A, "Optimally generate policy-based evidence before scaling," *Nature*, 2024, *626* (7999), 491–499.

List, John A., Azeem M. Shaikh, and Yang Xu, "Multiple Hypothesis Testing in Experimental Economics," *Experimental Economics*, December 2019, *22* (4), 773–793.

List, John, Fatemeh Momeni, Michael Vlassopoulos, and Yves Zenou, "Neighborhood Spillover Effects of Early Childhood Interventions," CEPR Discussion Papers 18134, C.E.P.R. Discussion Papers May 2023.

Maniadis, Zacharias, Fabio Tufano, and John A. List, "One Swallow Doesn't Make a Summer: New Evidence on Anchoring Effects," *American Economic Review*, January 2014, *104* (1), 277–90.

Miguel, Edward and Michael Kremer, "Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities," *Econometrica*, January 2004, *72* (1), 159–217.

Muralidharan, Karthik and Abhijeet Singh, "Improving Public Sector Management at Scale? Experimental Evidence on School Governance India," Working Paper 28129, National Bureau of Economic Research November 2020.

_ **and Paul Niehaus**, "Experimentation at Scale," *Journal of Economic Perspectives*, 2017, *31* (4), 103–124.

O'Brien, Peter C., "Procedures for Comparing Samples with Multiple Endpoints," *Biometrics*, 1984, *40* (4), 1079–1087.

Platas, Linda M., Leanne R. Ketterlin-Geller, and Yasmin Sitabkhan, "Using an Assessment of Early Mathematical Knowledge and Skills to Inform Policy and Practice: Examples from the Early Grade Mathematics Assessment," *International Journal of Education in Mathematics, Science and Technology*, 2016, *4(3)*, 163–173.

Romano, Joseph P. and Michael Wolf, "Exact and Approximate Stepdown Methods for Multiple Hypothesis Testing," *Journal of the American Statistical Association*, March 2005, *100*, 94–108.

_ **and** _ , "Stepwise Multiple Testing as Formalized Data Snooping," *Econometrica*, July 2005, *73* (4), 1237–1282.

_ **and** _ , "Efficient Computation of Adjusted p-Values for Resampling-Based Stepdown Multiple Testing," *Statistics & Probability Letters*, 2016, *113* (C), 38–40.

Zhou, Jin, Alison Baulos, James J. Heckman, and Bei Liu, "The Economics of Child Development with an Application to Home Visiting at Scale," in "The Scale-up Effect in Early Childhood and Public Policy. Edited by John A. List, Dana Suskind, and Lauren H. Supplee," Routledge, 2021.

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. 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 Table B-1 and in Panel A of Table B-2. School census data for the years 2015–2020 are used to track the school closures during the government implementation of both the API *Original* and *Plus* modalities, as shown in Table 7 and Figure 3.

Locality-level Population census: The National Institute of Statistics and Geography (INEGI) is in charge of compiling a population count with detailed information on socio-demographics, poverty, and education, among other information every decade. Census data are made available at the individual level for a small random sample of the population, as well as at the locality-level for the universe of localities in Mexico. We use the locality-level information collected in the census rounds of 2010 and 2020 for our analysis. In particular, we use information from the 2010 population census in Tables 1, B-1 and B-4. We leverage information on schooling outcomes in the 2020 population census for all the localities in the state of Chiapas (including those that were part of the experimental sample), which is shown in Table 4.

Standardized test scores. Between 2007 and 2013, all Mexican students in third grades through ninth grade 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, or geography. We use the school-level average of the Spanish scores in 2012 to construct the strata for the school-level randomization of the second experiment. In the first experiment, we use individual scores in each pedagogical area in 2013 as our main

measures of academic achievement. The *Overall Score* displayed in Table 2 is computed using GLS-weighted score over the three scores (O'Brien, 1984). Last, we use the 2013 ENLACE scores at the school-level for the placebo tests displayed in Table B-7.

Transitions to Secondary Schools. We link the enrollment records of the sixth graders in the sample of the second experiment across the population of seventh graders in Chiapas during the following academic year. Individual transitions computed in the school year 2016–2017 (i.e., by the end of the second experiment) are reported in Table 3, while transitions computed in the school year 2017–2018 (i.e., after the first year of the government implementation of the API *Plus* modality) are reported in Figure B-2.

Other administrative records. All students in Chiapas schools, irrespective of whether they received the API program, must 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 subject ranges between 5 and 10. We use the individual-level average across the three subjects in the diagnostic tests at the beginning of the 2014–2015 school year to construct the within-school student rankings displayed in Figure B-3 and Table B-14, which proxy for the individual eligibility for the one-on-one remedial education sessions.

We use student-level longitudinal information for the population of primary schools to construct various measures of school-level changes in student composition reported in Table B-3: whether the student must repeat a grade in school year 2015–2016, attrition from the 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.

A.2 Survey Data

Measures of Children's Achievement. We use 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 student assessments that have been conducted in more than 40 countries and in a variety of languages (Dubeck and Gove, 2015; Platas et al., 2016). While these instruments are typically applied to students in first, second, or third grade, we administer them to third through six grade students to account for the large learning gaps of the children in our sample. The school-average standardized scores in math and Spanish as measured in the school year prior to the introduction of the second experiment are, respectively, 0.5 and 0.7 standard deviations

below the national averages.[27] The reading scores reported in Tables 3 and B-14 are 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) listening, 8) dictation. The math scores reported in Tables 3 and B-14 are 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. An orthogonal rotation is applied before standardizing each factor with respect to the mean and the standard deviation in the control group. The individual components of the math and reading scores are reported in Table B-8.

To measure the impact of the intervention on socio-emotional skills, we consider a collection of thirty-two behavioral issues as reported by a caregiver, which resembles the questionnaire in the Children section of the National Longitudinal Study of Youth (CNLSY-79), such as antisocial behavior, anxiety/depression, headstrongness, hyperactivity and peer conflicts (for details, see Appendix A.2). The resulting behavioral problem index is re-scaled in such a way that higher values are associated with fewer behavioral issues (socio-emotional score). The survey also contains a module on instructors' characteristics as well as pedagogical practices collected through an adapted version of the Stallings Classroom Snapshot (Bruns and Luque, 2015), a module on parental attitudes and investment toward children's education, as well as information about the mentors' activities in the communities, among others. To better interpret our results, we standardize most of the survey-based outcome variables using the mean and the standard deviation observed in the control group. The socio-emotional scores reported in Tables 3 and B-14 are the sum of the following thirty-two items on how often the child displays a given emotion/behavior: 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 scared or anxious, 6) talks and argues too much, 7) has difficulty focusing on a specific activity for an extended amount of time, 8) gets easily confused, 9) has his/her head is in the clouds, 10) threatens or is mean with other children, 11) tends to challenge parental authority, 12) does not feel guilty after a bad deed, 13) does not get along with other children, 14) is impulsive or acts "fast" without thinking, 15) has inferiority issues, 16) has no friends, 17)

[27] Only 5 percent of the children in our sample score at the maximum of the scale in two or more subdomains of the reading score (out of eight subdomains) and in three or more subdomains of the math score (out of a total of seven subdomains). Unlike the first experiment, we cannot leverage the national standardized test scores for the second experiment since the test ceased to be universal during the period of interest (after 2014).

has difficulty letting go of certain thoughts, 18) is hyper active, 19) has a bad temper or is irascible, 20) easily loses his/her temper, 21) feels unhappy, sad, or depressed, 22) is shy, does not socialize with others, 23) breaks objects on purpose, 24) is too attached to adults, 25) cries too much, 26) demands a lot of attention, 27) is too much dependent on others, 28) is afraid of other people's judgment, 29) tends to be in bad company; 30) reserved, keeps things for himself/herself, 31) worries about everything, 32) misbehaves at school and does not respect the instructor.

The *Overall Score* of students' achievement displayed in Table 3 is computed using GLS-weighted averages over the two cognitive measures and the socio-emotional score.

Parenting Practices. The household survey collects information on parents' behavior and investment in their children's education. The same information was collected during the mid-line survey of the first experiment. The parental engagement outcomes reported in Table 5 are computed using GLS-weighted averages over different indicators of parental behavior. For *Engage at School*: whether or not parents (i) volunteer at the school, (ii) donate money to the school, (iii) donate in kind to the school, and (iv) offer food to the instructor. For *Manage School Resources*: whether or not parents (i) directly manage the school budget, (ii) propose some materials to the school, (iii) decide to use some materials for the school, and (iv) decide on how to allocate money for some school activities, and (v) define the pedagogical targets of the school. For *Engage with Child*: whether (i) parents help with their child's homework, (ii) meet with the instructor, (iii) expect their child to complete secondary education or more, and (iv) children participate in other academically-related activities outside the school hours. The *Engagement Index* is the same GLS-weighted average over each of the individual components described above, which are reported in Table B-13.

Mentor Characteristics. As part of the data collection activities, we have collected basic socio-demographic information on the mentors who served in the schools of the second experiment. Those are reported in Panel C of Table B-2 and in the second column Table B-10. For the other schools in Chiapas that were not part of the experimental sample, we rely on administrative rosters about mentors' characteristics from the program. Those are reported in the first column of Table B-10.

Parent-Mentor Interactions. The household module collects 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 *Original* group or the API *Plus* group. This information

is used to construct the four variables reported in Panel A of Table 6. Basic information on both the household module respondent and household characteristics is reported in Panel B of Table B-2.

Parenting Styles. The mentors' questionnaire included a battery of questions on the specific competencies they promote during their interactions with parents. The indicator variables for each competency are used as outcomes variables in Panel B of Table 6.

Teaching Practices. Local instructors were asked standard questions on their socio-demographic characteristics, education and experience. Those are reported in Panel A of Table B-2. 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, which is a rubric for timed observations that has been used previously 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. 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 module is used to construct GLS-weighted averages over the different types of teacher behavior, which are displayed in Table B-15. *Learning Activities* is the sum of the amount of time children spend on (i) reading aloud alone, (ii) reading aloud in a group, (iii) questions and answers, (iv) memorizing, (vi) individual homework, and (viii) verbal tasks. *Engage with Students* is the sum of the amount of time the instructor spends on (i) elaborating on a given concept, (ii) students were not involved, and (iii) keeping discipline. *Manage Time* is the amount of time the instructor spends (i) out of the classroom, (ii) effectively administering some tasks in the classroom, (iii) whether or not the instructor complies with the start and end time of each classroom, (iv) whether or not the instructor keeps the rhythm of the class as well as of the individual students according to their age and their mother-tongue, and (v) whether or not the students were grouped according to their respective academic levels. *Use of Material* is 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 displayed in the classroom, and (iv) whether charts and maps are displayed in the classroom. The *Overall Index* is the same

GLS-weighted average of the individual components of teacher behavior described above.

Quantity and Quality of Mentoring Services. Local instructors were asked about mentors' practices and activities within the local communities at two specific points in time: during the end-line survey of the experiment (Spring 2016) and in an additional follow-up survey module conducted in the fall of 2018 among the schools that were previously involved in the second experiment. The end-line survey was conducted in 57 out of a total of 58 schools that received the API *Plus* during the experiment. The follow-up survey was conducted in 93 out of a total of 103 schools that received the API *Plus* program during the government's program implementation. We obtained information about mentors from the responses collected by local instructors for 56 schools in the end-line survey and 58 schools in the follow-up survey. The corresponding measures are presented in Figure 2 and Table B-11.

A.3 In-Depth Interviews

In the spring of 2022 we implemented a series of semi-structured phone interviews with a small sample of local instructors and mentors who participated in the program. In total, we were able to locate and contact 104 local instructors and 68 mentors. Of those, 12 instructors and 16 mentors agreed to complete the phone interview. More than half of the survey respondents continued working as mentors after the 2016 government implementation of the *Plus* modality. The characteristics of the survey respondents in comparison with the overall sample are shown in Tables A-1 and A-2.

The survey contains a series of open questions related to the experiences of the mentors/local instructors with the parents in the communities. Below, we report the original quotes in Spanish that we refer to in the main body of the paper (authors' translation from Spanish). In particular, these quotes from the mentors about the peer-to-peer sessions of the training are reported in Section 4.2:

> "*Fue un momento de la capacitación en donde me dijeron que debía adaptarme al contexto de su centro del trabajo, de comprender las necesidades y de entender situaciones que se vivían en la misma comunidad, para poder dialogar con los padres y atender a los niños sin afectar o modificar lo que ellos conciben como su medio.*"

> "*Recomendaban hacer las visitas domiciliarias con frecuencia y ayudarle en*

algo a los papás o salían con ellos a visitas y les daba más confianza."

"*[Las sesiones de orientacion me permitieron] escuchar las diferentes estrategias que ellos tenían para poder probarlas e implementarlas.*"

These quotes from the local instructors about the role of parents in the day-by-day routine of the school are reported in Section 4.3.

"*La gestión dela escuela y se le hicieron mejoras de cercado, pintaron la escuela arreglaron los baños y se compraron materiales.*"

"*Eran participativos, estaban pendientes del bienestar de la escuela por ejemplo la construcción, de materiales e incluso de los desayunos y alimentación del instructor.*"

"*Los padres apoyaban en el seguimiento al bloc de tareas y trabajaban en equipo cuando los API que no podían estar presentes por apoyar a otra comunidad, los mantenían al corriente o, incluso un poco más avanzados, por lo que cuando los APIs regresaban podían dar continuidad a sus clases sin ningún atraso.*"

Table A-1: Characteristics of Mentors—Sample vs Phone Survey

	Original Sample	2022 Survey	Difference
Age	28.443	27.556	0.888
	(3.260)	(3.941)	(1.150)
Male	0.585	0.778	-0.193
	(0.495)	(0.441)	(0.171)
High School Completed	0.868	1.000	-0.132
	(0.340)	(0.000)	(0.114)
Training Weeks	2.858	2.667	0.192
	(2.035)	(1.871)	(0.703)
Experience as Api	21.274	13.444	7.829
	(10.058)	(6.803)	(3.425)
Previously Local Instructor	0.840	0.778	0.062
	(0.369)	(0.441)	(0.130)
Previously Education Assistant	0.085	0.000	0.085
	(0.280)	(0.000)	(0.094)
Days Spent in the Community	13.528	13.556	-0.027
	(5.331)	(4.876)	(1.840)
Students Lagging Behind	5.698	5.889	-0.191
	(1.657)	(3.018)	(0.621)

Notes: This table reports means and standard deviations for the characteristics of the mentors in the main sample of the analysis and those of the mentors who participated in the in-depth phone interviews (2022). The differences reported in the last column of the table are based on OLS estimates of the regression models that control for stratification dummies. Standard errors of the mean differences for the student characteristics are reported in parentheses in the last column and they are clustered at school level. For detailed descriptions of the survey variables used in this table, see Appendix A.2.

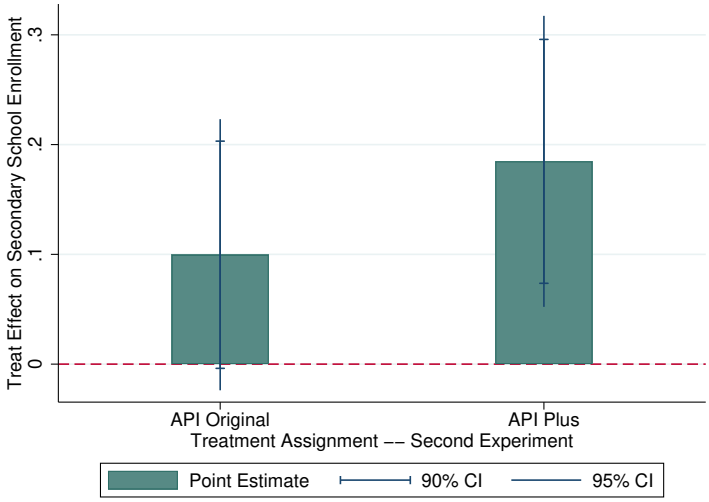
Table A-2: Characteristics of Local Instructors—Sample vs. Phone Survey

	Original Sample	2022 Survey	Difference
Age	21.284	21.157	0.127
	(2.585)	(2.034)	(0.702)
Male	0.560	0.786	-0.226
	(0.497)	(0.426)	(0.135)
Lower than Upper Second	0.062	0.071	-0.010
	(0.241)	(0.267)	(0.066)
Upper Second Complete	0.800	0.643	0.157
	(0.401)	(0.497)	(0.111)
Above Upper Second	0.138	0.286	-0.148
	(0.346)	(0.469)	(0.097)
Experience in Months	13.545	13.429	0.117
	(9.408)	(9.362)	(2.577)
Training Weeks at Baseline	4.768	5.500	-0.732
	(4.114)	(5.019)	(1.140)
Time spent in the School	9.509	9.071	0.438
	(4.220)	(3.269)	(1.146)
Sleeps in the Community	0.651	0.857	-0.206
	(0.478)	(0.363)	(0.130)
Nights spent in the Community	3.204	3.071	0.132
	(2.065)	(2.093)	(0.566)

Notes: This table reports means and standard deviations for the characteristics of the mentors in the main sample of the analysis and those of the mentors who participated in the in-depth phone interviews (2022). The differences reported in the last column of the table are based on OLS estimates of the regression models that control for stratification dummies. Standard errors of the mean differences for the student characteristics are reported in parentheses in the last column and they are clustered at the school level. For detailed descriptions of the survey variables used in this table, see Appendix A.2.

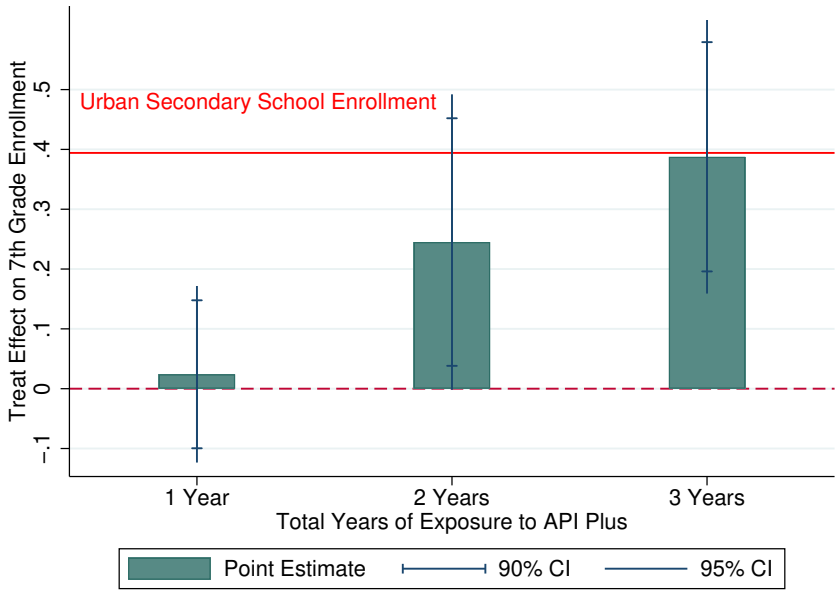
B Additional Figures and Tables

Figure B-1: Treatment Effects on Secondary School Enrollment During the Transition Between the Second Experiment and the Government Implementation



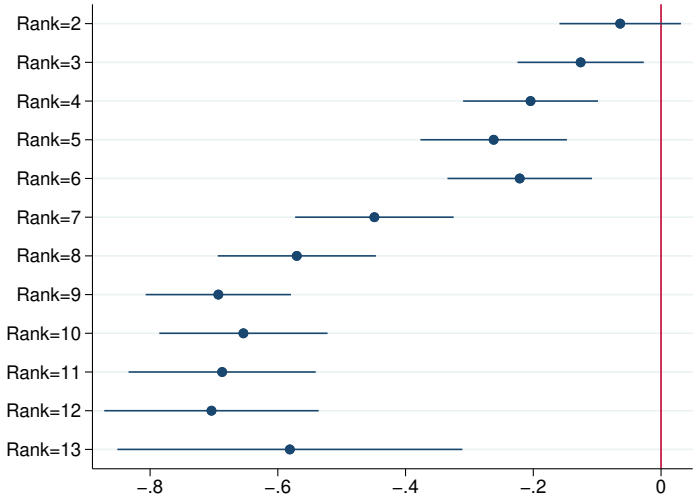
Notes: The bars depicted in this figure show the OLS estimates of the treatment assignments in the second experiment on the probability of enrolling in seventh grade in the year after the end of the second experiment (2017). The vertical lines overlaid on the bars represent asymptotic confidence intervals at the 90 percent and the 95 percent confidence levels. Confidence intervals are based on asymptotic inference. The sample includes 207 schools of the 224 that were part of the experiment. Beyond a school that permanently closed, the sample attrition is caused by schools not having sixth graders during that school year. Attrition is balanced among schools that were part of the two treatment arms (p-values = 0.914, and 0.768).

Figure B-2: The Cumulative Effect of API *Plus* in the Experimental Sample of Schools



Notes: This figure shows OLS estimates of the years of exposure to the mentoring program on the probability of enrolling in seventh grade during the transition from the second experiment to the government implementation of the API *Plus* modality. Vertical lines overlaid on each bar display the 95 percent and 90 percent confidence intervals, respectively. Confidence intervals are based on asymptotic inference. The sample includes 207 schools of the 224 that were part of the second experiment. Beyond a school that permanently closed, the sample attrition is caused by schools not having sixth graders during that school year. Attrition is balanced with respect to the indicator variables for the years of exposure to API *Plus* (p-value[1 year]=0.467, p-value[2 years]=0.812, and p-value[3 years]=0.568, the reference category is zero years of exposure).

Figure B-3: Probability of Being in Remedial Sessions by Inverted Achievement Rank



Notes: The dots in this figure are estimated marginal effects from Probit regression models of indicator variables for the inverted within-school student rank based on the average score on the diagnostic tests in math, Spanish, and natural science on the probability of participating in the one-on-one remedial education sessions with the mentors. The indicator variable for whether the student is ranked first (i.e., the worst-performing student in the class) is the omitted category. The horizontal lines around each dot represent 90 percent confidence intervals. Confidence intervals are based on asymptotic inference.

Table B-1: Baseline Characteristics and Covariate Balance – First Experiment

	API Original		Control		Diff
	Mean	Std. Dev.	Mean	Std. Dev.	p-value
	Panel A: Schools in Mid-Line 2012 Survey of Parents				
Average Test Score (Spanish)	401.971	38.973	399.036	28.974	0.703
Average Test Score (Math)	377.916	43.159	388.422	51.038	0.351
Number of Students	15.917	8.334	14.917	7.987	0.597
Number of Teachers	1.389	0.549	1.417	0.604	0.827
Share Over-aged Students	2.134	7.225	1.961	4.094	0.900
Total Population	217.054	597.061	234.778	506.694	0.888
Labor Force Participation	0.286	0.064	0.276	0.069	0.553
Water Network (Y/N)	0.027	0.164	0.056	0.232	0.547
Sewer System (Y/N)	0.027	0.164	0.028	0.167	0.990
Rate of Illiteracy	0.321	0.170	0.333	0.173	0.745
Garbage Collection (Y/N)	0.027	0.164	0.056	0.232	0.551
Number of Schools/Localities	37		36		
	Panel B: Schools with Individual Test Score 2013 Data				
Average Test Score (Spanish)	401.869	40.034	399.206	29.378	0.748
Average Test Score (Math)	377.168	44.284	390.561	50.120	0.242
Number of Students	15.971	8.449	14.743	8.034	0.527
Number of Teachers	1.400	0.553	1.400	0.604	1.000
Share Over-aged Students	2.195	7.321	2.017	4.140	0.900
Total Population	225.857	612.996	227.543	512.201	0.990
Labor Force Participation	0.287	0.065	0.278	0.069	0.579
Water Network (Y/N)	0.029	0.169	0.057	0.236	0.568
Sewer System (Y/N)	0.029	0.169	0.029	0.169	1.000
Rate of Illiteracy	0.327	0.165	0.335	0.175	0.823
Garbage Collection (Y/N)	0.029	0.169	0.057	0.236	0.566
Number of Schools/Localities	35		35		

Notes: This table shows means and standard deviations for community and school characteristics collected in the population census (2010) and the school census (2010). See Appendix A.1 for more details on these data sources. The fifth column reports the associated p-values of the differences in means between the treatment and the control group.

Table B-2: Baseline Characteristics and Covariate Balance – Second Experiment

Sample Statistic	Control Mean (SD)	API Original Mean (SD)	API Plus Mean (SD)	Original-Control Mean Difference (p-value)	Plus-Control Mean Difference (p-value)
Panel A: School and Teacher Characteristics					
Average Test Score (Spanish)	429.389	432.326	430.573	0.846	0.743
	(60.477)	(67.579)	(67.463)	(0.738)	(0.792)
Average Test Score (Math)	453.090	455.820	451.627	0.156	-2.057
	(78.436)	(84.546)	(82.461)	(0.978)	(0.778)
Average Test Score (Science)	438.349	441.259	442.856	1.435	3.866
	(50.264)	(49.323)	(50.492)	(0.735)	(0.390)
Number of Teachers	1.224	1.309	1.207	0.086	-0.016
	(0.419)	(0.465)	(0.409)	(0.213)	(0.820)
Number of Students	15.296	15.441	14.379	0.161	-0.953
	(5.819)	(5.655)	(5.824)	(0.857)	(0.320)
Teacher with Secundary Education	0.763	0.794	0.833	0.031	0.072
	(0.389)	(0.398)	(0.358)	(0.628)	(0.241)
Years of Experience as Teacher	0.737	0.706	0.693	-0.034	-0.042
	(0.872)	(0.802)	(1.085)	(0.802)	(0.797)
Months of Teacher Working in the School	9.531	9.309	9.281	-0.229	-0.249
	(3.947)	(4.925)	(3.266)	(0.751)	(0.676)
Observations	98	68	58	166	156
Panel B: Child and Household Characteristics					
Age in Months at Baseline (September 2014)	104.993	104.289	105.539	-0.818	0.647
	(16.384)	(17.532)	(14.924)	(0.485)	(0.605)
Male (Y/N)	0.532	0.519	0.543	-0.011	0.013
	(0.500)	(0.500)	(0.499)	(0.734)	(0.772)
Indigenous Language (Y/N)	0.302	0.307	0.461	0.012	0.155
	(0.460)	(0.462)	(0.499)	(0.855)	(0.032)
Scholarship (Y/N)	0.746	0.733	0.747	-0.013	0.005
	(0.436)	(0.443)	(0.435)	(0.763)	(0.903)
Parent Can Read	0.715	0.686	0.734	-0.030	0.023
	(0.452)	(0.465)	(0.443)	(0.465)	(0.590)
Parent with Less than Primary	0.614	0.587	0.584	-0.027	-0.029
	(0.487)	(0.493)	(0.494)	(0.526)	(0.483)
Household Receives Oportunidades CCT	0.812	0.807	0.829	-0.003	0.016
	(0.391)	(0.395)	(0.377)	(0.929)	(0.614)
Observations	453	322	269	775	722
Panel C: Mentor Characteristics					
Age in Years		28.386	28.400		0.242
		(3.678)	(3.057)		(0.705)
Male		0.579	0.620		0.051
		(0.498)	(0.490)		(0.597)
High Edu Complete		0.877	0.880		0.006
		(0.331)	(0.328)		(0.926)
Months of Experience as Mentor		22.298	20.040		-2.218
		(10.997)	(8.755)		(0.260)
Observations		57	50		107

Notes: The first three columns of the table report mean and standard deviations in parentheses 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 and from the 2013 school census. The other characteristics reported in Panels B-D are collected in the survey data. The differences reported in the last two columns of the table are based on OLS estimates of the regression models that control for stratification dummies. p-values for the null hypothesis of equal mean differences are reported in parentheses in the last two columns. See Appendix A for more details on the data sources.

Table B-3: Treatment Assignment and School-Level Student Composition

	Repeat	Attrition	Outside CONAFE in $t-1$	Same school in $t-1$
API *Original*	-0.011	-0.018	-0.002	0.019
	[0.116]	[0.322]	[0.895]	[0.295]
API *Plus*	-0.010	-0.006	-0.003	0.011
	[0.153]	[0.751]	[0.861]	[0.574]
H0: *Original* = *Plus*	[0.834]	[0.491]	[0.911]	[0.620]
Number of Schools	224	224	224	224
Number of Observations	1019	1019	1019	1019

Notes: This table shows the estimates of the two API modalities on various measures of school-level changes in student composition. The number of observations drops from 1045 to 1019 due to incomplete school identifiers (CURP) for 26 students. Asymptotic p-values reported in brackets are clustered at school level. For a detailed descriptions of the schooling records used in this table, see Appendix A.1.

Table B-4: Differences Between Overall Samples and Matched-census Samples

	All Chiapas			Second Experiment		
	All Sample Mean (SD)	Census Sample Mean (SD)	Mean Difference (p-value)	All Sample Mean (SD)	Census Sample Mean (SD)	Mean Difference (p-value)
			Panel A: School Characteristics			
Average test score (Spanish)	424.503	422.903	1.600	431.340	433.855	-2.515
	(56.466)	(54.786)	(0.522)	(60.810)	(63.370)	(0.705)
Average test score (Math)	414.921	413.736	1.184	421.333	424.043	-2.710
	(75.300)	(74.699)	(0.725)	(80.895)	(84.848)	(0.760)
Number of students	14.049	13.974	0.075	15.009	15.158	-0.149
	(8.468)	(8.865)	(0.834)	(6.053)	(5.794)	(0.799)
Number of Teachers	1.231	1.240	-0.008	1.217	1.217	-0.000
	(0.467)	(0.480)	(0.671)	(0.413)	(0.414)	(1.000)
Share Over-aged Students	0.349	0.348	0.001	0.324	0.290	0.034
	(0.797)	(0.818)	(0.971)	(0.659)	(0.615)	(0.589)
			Panel B: Locality Characteristics			
Total Population	118.758	121.170	-2.412	121.389	158.276	-36.887
	(221.648)	(208.666)	(0.775)	(240.562)	(337.620)	(0.219)
Share of High-Poverty Villages	0.490	0.489	0.001	0.473	0.453	0.020
	(0.500)	(0.500)	(0.945)	(0.500)	(0.499)	(0.702)
Incidence of Social Conflict (Y/N)	0.190	0.204	-0.014	0.187	0.201	-0.014
	(0.392)	(0.403)	(0.355)	(0.391)	(0.402)	(0.719)
Share of Illiterate Adults	0.313	0.315	-0.002	0.295	0.292	0.003
	(0.160)	(0.159)	(0.703)	(0.153)	(0.150)	(0.860)
Share of Adults in the Labor Force	0.297	0.296	0.002	0.303	0.301	0.002
	(0.076)	(0.077)	(0.575)	(0.070)	(0.067)	(0.765)
Locality Access without Road	0.216	0.224	-0.008	0.179	0.149	0.029
	(0.411)	(0.417)	(0.609)	(0.384)	(0.357)	(0.426)
Water Network (Y/N)	0.028	0.028	0.000	0.022	0.038	-0.016
	(0.164)	(0.164)	(0.998)	(0.146)	(0.192)	(0.341)
Sewage System (Y/N)	0.011	0.012	-0.001	0.009	0.016	-0.008
	(0.105)	(0.109)	(0.830)	(0.093)	(0.127)	(0.497)
Garbage Collection (Y/N)	0.022	0.023	-0.002	0.022	0.027	-0.005
	(0.146)	(0.151)	(0.784)	(0.146)	(0.163)	(0.724)
Observations	1,523	1,161	3,046	230	184	414

Notes: Means and standard deviations in parentheses for various characteristics collected before the introduction of the API program. The last column shows asymptotic p-values for mean differences between the overall population and the experimental sample. Panel A shows community-level characteristics from the population census (2010), whereas Panel B displays school-level variables from the school census (2010). See Appendix A.1 for more details on the data sources.

Table B-5: Heterogeneity in the Impact of the Program by Eligibility Criteria

	Achievement Index	Enrolled Secondary
API Original	0.095	0.134
	[0.382]	[0.209]
API Original× Poverty	0.069	-0.098
	[0.712]	[0.441]
API Original× Welfare	0.097	-0.035
	[0.464]	[0.754]
API Original× Achievement Score	-0.036	0.003
	[0.689]	[0.964]
API Plus	0.329	0.276
	[0.039]	[0.022]
API Plus× Poverty	0.080	0.014
	[0.682]	[0.909]
API Plus× Welfare	-0.001	-0.220
	[0.997]	[0.124]
API Plus× Achievement Score	-0.058	-0.018
	[0.569]	[0.797]
API Original(Poverty)=API Original(Welfare)=API Original(Score)	[0.732]	[0.753]
API Plus(Poverty)=API Plus(Welfare)=API Plus(Score)	[0.774]	[0.450]
Observations	1045	468
Clusters	224	182

Notes: This table shows OLS estimates and the associated asymptotic p-values (in brackets) on student outcomes measured after two academic years of exposure to the API program under the second experiment designed and implemented by the authors in collaboration with the government. For a detailed descriptions of the test score index used in this table, see Appendix A.2. The dependent variables in the first column is standardized with respect to its mean and the standard deviation in the control group. The dependent variable in the second column is computed from administrative school records (see Appendix A.1). All p-values account for clustering at the school level.

Table B-6: Program Assignment at Scale (2017-2018) and Observable Characteristics

	OLS Estimates [p-value]
Panel A: School Characteristics	
Average test score (Spanish)	-3.635
	[0.395]
Average test score (Math)	-7.707
	[0.184]
Number of students	0.644
	[0.264]
Number of Teachers	0.038
	[0.263]
Share Over-aged Students	0.020
	[0.690]
Panel B: Locality Characteristics	
Total Population	8.847
	[0.515]
Share of High-Poverty Villages	-0.035
	[0.296]
Incidence of Social Conflict (Y/N)	0.022
	[0.100]
Share of Illiterate Adults	-0.015
	[0.121]
Share of Adults in the Labor Force	-0.004
	[0.472]
Locality Access without Road	-0.061
	[0.026]
Water Network (Y/N)	0.007
	[0.538]
Sewage System (Y/N)	0.012
	[0.137]
Garbage Collection (Y/N)	-0.002
	[0.841]
F-Statistic for Joint Hypothesis of no Differences	0.76
	[0.733]
Observations	1,345

Notes: This table shows OLS estimates and asymptotic p-values (in brackets) for the indicator of the API *Plus* assignment during the 2017-2018 school cycle after controlling for the assignment criteria (Section 2.1), an indicator variable for prior exposure to the API Original modality, and the number of hostile event related to land property, religion, elections, crime, or drug addiction as reported at the locality level in the population census (2010). Panel A shows community-level characteristics from the population census (2010), whereas Panel B displays school-level variables from the school census (2010). See Appendix A.1 for more details on the data sources.

Table B-7: Placebo Test for API Plus Assignment During Program Scale-up

	Spanish		Math		Science	
API Plus	-0.104	0.003	-0.093	-0.001	-0.062	0.027
	[0.062]	[0.954]	[0.099]	[0.989]	[0.268]	[0.621]
	{0.062}	{0.949}	{0.112}	{0.993}	{0.277}	{0.643}
	(0.107)	(0.996)	(0.146)	(0.996)	(0.260)	(0.866)
Controls for Criteria	No	Yes	No	Yes	No	Yes
Observations	1183	1183	1183	1183	1183	1183

Notes: This table shows OLS estimates and the associated p-values of the assignment API *Plus* in the fall of 2017. For detailed descriptions of the 2013 school-average test scores used in this table as outcome variables, see Appendix A.1. Control variables include indicator functions for the four criteria used to determine the differential priority across eligible schools to receive the mentors (see Section 2.1) as well as an indicator function for prior exposure to the mentoring program and the number of hostile event related to land property, religion, elections, crime, or drug addiction as reported at the locality level in the population census (2010). p-values reported in brackets refer to the conventional asymptotic standard errors. p-values reported in braces are computed using randomization inference (randomization-t). p-values reported in parentheses are adjusted for testing the null impact of API Plus across the two specifications considered (without and with controls) through the step-wise procedure described in Romano and Wolf (2005a,b, 2016).

Table B-8: Average Program Impacts by Subdomains of the Reading and the Math Scores

	Panel A: Share of Correct Reading Answers by Subdomain							
	Letter Name	Initial Name	Initial Sound	Word Recogn.	Word Reading	Read Comprehen.	Listening	Dictation
API Original	0.103	0.006	0.122	0.129	0.075	0.118	-0.004	0.129
	[0.232]	[0.941]	[0.156]	[0.091]	[0.300]	[0.107]	[0.963]	[0.120]
	{0.285}	{0.949}	{0.194}	{0.124}	{0.341}	{0.138}	{0.968}	{0.173}
	(0.449)	(0.996)	(0.365)	(0.255)	(0.510)	(0.290)	(0.996)	(0.314)
API Plus	0.240	-0.019	0.042	0.318	0.197	0.321	0.123	0.378
	[0.005]	[0.816]	[0.565]	[0.000]	[0.014]	[0.000]	[0.145]	[0.000]
	{0.010}	{0.824}	{0.584}	{0.000}	{0.026}	{0.001}	{0.185}	{0.000}
	(0.005)	(0.789)	(0.728)	(0.000)	(0.021)	(0.000)	(0.226)	(0.000)
API Original = API Plus	[0.180]	[0.771]	[0.343]	[0.039]	[0.183]	[0.023]	[0.094]	[0.005]
	{0.174}	{0.799}	{0.479}	{0.062}	{0.229}	{0.059}	{0.220}	{0.003}
	(0.328)	(0.727)	(0.421)	(0.077)	(0.328)	(0.045)	(0.194)	(0.010)
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 Original	0.094	0.036	0.099	0.011	0.061	-0.051	0.022
	[0.252]	[0.661]	[0.192]	[0.874]	[0.402]	[0.481]	[0.789]
	{0.301}	{0.681}	{0.226}	{0.882}	{0.447}	{0.511}	{0.800}
	(0.576)	(0.919)	(0.483)	(0.923)	(0.789)	(0.817)	(0.923)
API Plus	0.259	0.201	0.204	0.215	0.111	0.116	0.099
	[0.005]	[0.026]	[0.022]	[0.003]	[0.103]	[0.156]	[0.316]
	{0.011}	{0.036}	{0.035}	{0.008}	{0.130}	{0.200}	{0.365}
	(0.007)	(0.033)	(0.033)	(0.007)	(0.137)	(0.163)	(0.247)
API Original = API Plus	[0.095]	[0.103]	[0.218]	[0.008]	[0.500]	[0.046]	[0.396]
	{0.163}	{0.129}	{0.420}	{0.020}	{0.514}	{0.080}	{0.550}
	(0.191)	(0.191)	(0.361)	(0.008)	(0.516)	(0.090)	(0.516)
Observations	1044	1044	1044	1044	1044	1044	1044
Clusters	224	224	224	224	224	224	224

Notes: This table shows OLS estimates and the associated p-values of the two API modalities: API *Original* and API *Plus* for 1,044 students enrolled in third to sixth grade by the end of the second school year since treatment assignment. For detailed descriptions of the sub-components of the reading and math scores used in this table, see Appendix A.2. The outcome variables are standardized with respect to their means and the standard deviations in the control group. The inference procedures take into account clustering of the error terms at the school level and the block randomization design at the strata level. p-values reported in brackets refer to the conventional asymptotic inference. p-values reported in braces are computed using randomization inference (randomization-t). All p-values account for clustering at the school level. p-values reported in parentheses are adjusted for testing each null hypothesis (null impact of API *Original*, API *Plus*, and the comparison) on multiple outcomes through the step-wise procedure described in Romano and Wolf (2005a,b, 2016).

Table B-9: Average Program Impacts by the Individual Components of the Socio-Emotional Score

Panel A: First 16 Components

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
API Original	0.040	-0.068	0.074	0.003	-0.008	0.026	0.072	-0.009	0.006	0.015	0.017	0.042	-0.013	-0.024	0.030	-0.020
	[0.293]	[0.041]	[0.049]	[0.943]	[0.835]	[0.477]	[0.047]	[0.818]	[0.863]	[0.679]	[0.646]	[0.205]	[0.737]	[0.410]	[0.348]	[0.563]
	{0.340}	{0.052}	{0.065}	{0.945}	{0.849}	{0.507}	{0.062}	{0.826}	{0.868}	{0.700}	{0.654}	{0.246}	{0.748}	{0.447}	{0.386}	{0.588}
	(0.989)	(0.370)	(0.409)	(1.000)	(1.000)	(0.999)	(0.393)	(1.000)	(1.000)	(1.000)	(1.000)	(0.934)	(1.000)	(0.997)	(0.994)	(0.999)
API Plus	0.125	0.058	0.057	-0.012	-0.014	0.038	0.096	-0.023	0.021	-0.007	0.055	0.056	0.047	0.061	0.040	0.003
	[0.001]	[0.136]	[0.158]	[0.773]	[0.720]	[0.317]	[0.019]	[0.584]	[0.510]	[0.870]	[0.150]	[0.113]	[0.205]	[0.057]	[0.216]	[0.937]
	{0.002}	{0.168}	{0.204}	{0.798}	{0.748}	{0.352}	{0.035}	{0.607}	{0.533}	{0.889}	{0.173}	{0.149}	{0.249}	{0.078}	{0.251}	{0.939}
	(0.010)	(0.775)	(0.813)	(0.999)	(0.999)	(0.972)	(0.157)	(0.997)	(0.995)	(0.999)	(0.809)	(0.710)	(0.901)	(0.421)	(0.908)	(0.999)
API Original = API Plus	[0.044]	[0.002]	[0.690]	[0.721]	[0.863]	[0.777]	[0.560]	[0.739]	[0.696]	[0.595]	[0.380]	[0.706]	[0.141]	[0.014]	[0.759]	[0.532]
	{0.073}	{0.003}	{0.641}	{0.758}	{0.894}	{0.812}	{0.772}	{0.795}	{0.680}	{0.637}	{0.413}	{0.796}	{0.174}	{0.024}	{0.789}	{0.580}
	(0.367)	(0.013)	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)	(0.998)	(1.000)	(0.843)	(0.119)	(1.000)	(0.999)
Observations	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045
Clusters	224	224	224	224	224	224	224	224	224	224	224	224	224	224	224	224

Panel B: Second 16 Components

	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)
API Original	-0.005	-0.050	0.015	-0.030	0.044	-0.034	0.085	-0.026	0.040	0.026	0.060	0.010	0.075	0.002	0.024	0.033
	[0.882]	[0.138]	[0.677]	[0.405]	[0.178]	[0.116]	[0.020]	[0.450]	[0.328]	[0.519]	[0.054]	[0.720]	[0.044]	[0.956]	[0.553]	[0.301]
	{0.894}	{0.159}	{0.707}	{0.448}	{0.192}	{0.143}	{0.038}	{0.491}	{0.370}	{0.564}	{0.076}	{0.730}	{0.067}	{0.967}	{0.564}	{0.345}
	(1.000)	(0.823)	(1.000)	(0.997)	(0.905)	(0.757)	(0.189)	(0.998)	(0.991)	(0.999)	(0.436)	(1.000)	(0.381)	(1.000)	(0.999)	(0.989)
API Plus	0.073	-0.009	0.091	0.021	0.040	-0.013	0.077	0.071	0.045	0.037	0.100	0.053	0.020	0.036	0.037	0.007
	[0.018]	[0.807]	[0.014]	[0.559]	[0.214]	[0.547]	[0.031]	[0.048]	[0.305]	[0.336]	[0.005]	[0.049]	[0.613]	[0.344]	[0.327]	[0.838]
	{0.028}	{0.817}	{0.028}	{0.586}	{0.245}	{0.608}	{0.045}	{0.065}	{0.353}	{0.379}	{0.009}	{0.071}	{0.647}	{0.366}	{0.383}	{0.846}
	(0.154)	(0.999)	(0.117)	(0.997)	(0.908)	(0.997)	(0.258)	(0.371)	(0.972)	(0.972)	(0.037)	(0.379)	(0.997)	(0.972)	(0.972)	(0.999)
API Original = API Plus	[0.018]	[0.246]	[0.055]	[0.191]	[0.923]	[0.350]	[0.848]	[0.012]	[0.925]	[0.796]	[0.301]	[0.193]	[0.203]	[0.422]	[0.735]	[0.494]
	{0.037}	{0.298}	{0.092}	{0.233}	{0.933}	{0.408}	{0.896}	{0.027}	{0.960}	{0.775}	{0.444}	{0.175}	{0.210}	{0.463}	{0.742}	{0.493}
	(0.146)	(0.966)	(0.432)	(0.935)	(1.000)	(0.996)	(1.000)	(0.102)	(1.000)	(1.000)	(0.989)	(0.935)	(0.937)	(0.998)	(1.000)	(0.999)
Observations	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1045	1044
Clusters	224	224	224	224	224	224	224	224	224	224	224	224	224	224	224	224

Notes: This table shows OLS estimates and the associated p-values of the two API modalities: API *Original* and API *Plus* for 1,044 students enrolled in third to sixth grade by the end of the second school year since treatment assignment. The individual components of the socio-emotional score are indicator variables for whether the child displays one of the following emotions/behaviors: 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 scared or anxious, 6) talks and argues too much, 7) has difficulty in focusing on a specific activity for an extended amount of time, 8) gets easily confused, 9) it seems that his/her head is in the clouds, 10) threatens or is mean with other children, 11) tends to challenge parental authority, 12) does not feel guilty after a bad deed, 13) does not get along with other children, 14) is impulsive or acts "fast" without thinking, 15) feels has inferiority issues, 16) has no friends, 17) has difficulty letting go certain thoughts, 18) is hyper-active, 19) has a bad temper, or is irascible, 20) looses easily his/her temper, 21) feels unhappy, sad, or depressed, 22) is shy, does not socialize with others, 23) breaks objects on purpose, 24) is too attached to the adults, 25) cries too much, 26) demands a lot of attention, 27) is too much dependent on others, 28) is afraid of other people's judgement, 29) Tends to be in bad company; 30) is reserved, keeps things for himself/herself, 31) worries about every thing, 32) misbehaves at school and does not respect the instructor (see Appendix A.2). The inference procedures take into account clustering of the error terms at the school level and the block randomization design at the strata level. p-values reported in brackets refer to the conventional asymptotic inference. p-values reported in braces are computed using randomization inference (randomization-t). All p-values account for clustering at the school level. p-values reported in parentheses are adjusted for testing each null hypothesis (null impact of API *Original*, API *Plus*, and the comparison) on multiple outcomes through the stepwise procedure described in Romano and Wolf (2005a,b, 2016).

Table B-10: Comparison of Mentors' Characteristics Across Situations

Variable	(1) Chiapas sample	(2) Experiment 2	(3) Chiapas vs Experiment 2
Male	0.571	0.604	0.033
	(0.495)	(0.491)	[0.492]
Age	28.460	28.266	-0.194
	(3.780)	(3.287)	[0.558]
Speaks Indigenous Language	0.295	0.374	0.079
	(0.457)	(0.486)	[0.101]
Observations	441	139	580

Notes: This table shows the comparison of mentors' characteristics between the second experiment and the scale-up of the *Plus* program. The first two columns show mean and standard deviations (in parentheses) for both samples. The third column shows the difference and the associated p-values (in brackets) of the null hypothesis of no difference across samples.

Table B-11: Change in Situation and Impacts on Quality and Quantity of Mentoring Program

	Quantity			Quality		
	Days in Community	Number Activities with Instructor	Time Spent with Instructor	Meetings with Parents of Students at Risk	Students with API Support	Time spent with Students
Change in Situation	-1.585	-1.093	-0.954	-0.596	-0.546	-0.025
	[0.330]	[0.037]	[0.189]	[0.407]	[0.483]	[0.908]
Observations	114	113	114	109	96	110
Observations Survey 2016	56	55	56	51	54	52
Observations Survey 2018	58	58	58	58	42	58

Notes: This table shows the comparison in the quantity and quality of API *Plus* program between the second experiment and the government implementation. This information is collected during the surveys of the local instructors, in the school years 2015-2016 and 2018-2019. Each estimate in each column represents an OLS estimate for the difference in the mentoring services across the two situations. The asymptotic p-values are reported in square brackets. All the regressions include the same set of controls as in Table 4.

Table B-12: The Impact of the API Plus Program on School Closures

	Non-Experimental Schools	Experimental Schools
API Plus	-0.068	-0.070
	[0.000]	[0.026]
Observations	1161	184

Notes: This table shows the OLS estimates of the assignment to the API program during the government implementation of the Plus modality on the rate of school closures as measured over the subsequent two years. p-values reported in brackets are based on asymptotic inference. All the regressions include the same set of controls as in Table 4.

Table B-13: Average Program Impacts by the Individual Components of Parental Investments

	Engage with School				Manage School Resources					Engage with Child			
	Volunteering	Donate Cash	Donate In-Kind	Food Instructor	Manage School Resources	Propose School Material	Decide School Material	Decide Money Allocation	Evaluate School Targets	Help With Homework	Extra-Academic Activities	Meeting Teachers	Expect Upper Secondary
					Panel A: First Experiment								
API Original	0.042	0.118	0.063	0.046	-0.042	0.026	-0.009	0.002	-0.040	0.210	0.055	0.203	0.025
	[0.417]	[0.126]	[0.478]	[0.560]	[0.579]	[0.726]	[0.912]	[0.974]	[0.487]	[0.358]	[0.528]	[0.291]	[0.608]
	{0.435}	{0.147}	{0.494}	{0.566}	{0.597}	{0.734}	{0.916}	{0.971}	{0.512}	{0.382}	{0.524}	{0.322}	{0.626}
	(0.955)	(0.475)	(0.969)	(0.969)	(0.969)	(0.969)	(0.983)	(0.983)	(0.969)	(0.928)	(0.969)	(0.872)	(0.969)
Number of clusters	73	73	73	73	73	73	73	73	73	73	73	73	73
Observations	208	208	207	208	208	208	208	208	208	208	207	208	199
					Panel B: Second Experiment								
API Original	-0.031	-0.004	-0.058	-0.058	-0.029	-0.070	-0.062	-0.010	-0.027	0.222	0.074	0.043	0.010
	[0.356]	[0.894]	[0.130]	[0.042]	[0.471]	[0.095]	[0.122]	[0.772]	[0.389]	[0.027]	[0.082]	[0.568]	[0.781]
	{0.884}	{0.981}	{0.452}	{0.194}	{0.917}	{0.369}	{0.452}	{0.981}	{0.888}	{0.137}	{0.350}	{0.942}	{0.981}
	(0.377)	(0.902)	(0.155)	(0.057)	(0.488)	(0.123)	(0.153)	(0.783)	(0.422)	(0.048)	(0.117)	(0.598)	(0.791)
API Plus	0.036	0.018	0.044	0.071	0.069	0.001	0.006	0.010	0.018	0.221	0.108	0.192	0.094
	[0.289]	[0.625]	[0.329]	[0.013]	[0.095]	[0.978]	[0.890]	[0.776]	[0.570]	[0.066]	[0.015]	[0.020]	[0.019]
	{0.765}	{0.953}	{0.778}	{0.062}	{0.323}	{0.977}	{0.977}	{0.977}	{0.953}	{0.245}	{0.063}	{0.072}	{0.072}
	(0.341)	(0.666)	(0.364)	(0.024)	(0.128)	(0.977)	(0.901)	(0.791)	(0.598)	(0.105)	(0.025)	(0.037)	(0.034)
Clusters	224	224	224	224	224	224	224	223	224	224	224	223	224
Observations	1042	1042	1039	1042	1033	1036	1027	1031	1029	1044	1033	974	1017

Notes: This table shows OLS estimates and the associated p-values of the two API modalities: API *Original* and API *Plus* for 1,044 students enrolled in third to sixth grade by the end of the second school year since treatment assignment. For a detailed descriptions of the sub-components of the reading and math scores used in this table, see Appendix A.2. The outcome variables are standardized with respect to their means and the standard deviations in the control group. The inference procedures take into account clustering of the error terms at the school level and the block randomization design at the strata level. p-values reported in brackets refer to the conventional asymptotic inference. p-values reported in braces are computed using randomization inference (randomization-t). All p-values account for clustering at the school level. p-values reported in parentheses are adjusted for testing each null hypothesis (null impact of API *Original*, API *Plus*, and the comparison) on multiple outcomes through the stepwise procedure described in Romano and Wolf (2005a,b, 2016).

Table B-14: Remedial Education Sessions

	Reading Score	Math Score	Socio-Emotional Score	Overall Index
API *Original*× Rank≥7	0.193	0.023	0.147	0.192
	[0.105]	[0.844]	[0.313]	[0.177]
API *Plus*× Rank≥7	0.423	0.274	0.206	0.430
	[0.001]	[0.055]	[0.140]	[0.003]
API *Original*× Rank<7	0.078	0.045	0.034	0.074
	[0.431]	[0.641]	[0.728]	[0.487]
API *Plus*× Rank<7	0.261	0.224	0.183	0.327
	[0.011]	[0.042]	[0.082]	[0.003]
H0: *Original* = *Plus* (<7)	[0.104]	[0.095]	[0.192]	[0.039]
H0: *Original* = *Plus* (≥7)	[0.072]	[0.081]	[0.721]	[0.144]
H0: [*Original-Plus* (<7)]=[*Original-Plus* (≥7)]	[0.766]	[0.675]	[0.639]	[0.937]
Number of Schools	224	224	224	224
Number of Observations	1044	1044	1045	1045

Notes: This table shows the estimates for the API program 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 Figure B-3). 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. Asymptotic p-values reported in brackets are clustered at the school level.

Table B-15: Teacher Pedagogical Practices

	Learning Activities	Engage With Students	Manage Time	Use of Material	Overall Index
API *Original*	0.048	-0.066	0.093	-0.125	-0.037
	[0.718]	[0.678]	[0.645]	[0.473]	[0.795]
	{0.711}	{0.692}	{0.658}	{0.458}	{0.797}
	(0.973)	(0.973)	(0.973)	(0.926)	(0.973)
API *Plus*	-0.072	0.050	-0.086	0.025	-0.203
	[0.619]	[0.738]	[0.620]	[0.871]	[0.150]
	{0.620}	{0.752}	{0.598}	{0.884}	{0.141}
	(0.956)	(0.956)	(0.956)	(0.956)	(0.322)
Original = *Plus*	[0.462]	[0.488]	[0.358]	[0.428]	[0.274]
	{0.432}	{0.497}	{0.385}	{0.448}	{0.281}
	(0.709)	(0.709)	(0.699)	(0.709)	(0.580)
Number of Observations	209	209	209	209	209

Notes: This table shows OLS estimates and the associated p-values of the API *Original* and the API *Plus* modalities on teachers' pedagogical practices (Stallings Classroom Snapshot). The outcome variables are standardized with respect to their means and the standard deviations in the control group. The inference procedures take into account the block randomization design at the strata level. p-values reported in brackets refer to the conventional (robust) asymptotic inference. p-values reported in braces are computed using randomization inference (randomization-t). p-values reported in parentheses are adjusted for testing each null hypothesis (null impact of API *Original*, API *Plus*, and the comparison) on multiple outcomes through the stepwise procedure described in Romano and Wolf (2005a,b, 2016).