Improving Math and Science Skills in Contexts of Low Teacher Capacity: Experimental Evidence from India*

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Abstract

We present experimental evidence on a program in India that recruited college students ("fellows") in math and science fields to teach in primary schools for a year. Fellows were younger, less educated, and less experienced than teachers, but they outperformed them by 1.4σ on a test of content knowledge and pedagogy. They received a brief training, lesson scripts, and instructional coaches. During unannounced visits, fellows were no more likely to go to work or arrive early than control teachers. Yet, during announced observations, they fared 0.73σ better on an index of positive instructional practices. After a year, their students scored 0.34σ higher in math, 0.22σ in science, and 0.15σ in language than those taught by regular teachers. By the start of the next year, they still scored 0.36σ, 0.14σ, and 0.08σ higher in these subjects, respectively. They did not, however, differ on attitudes towards math and science, intelligence and math mindsets, or aspirations to pursue related careers. Evidence of compensatory behavior (more resources) in control classes suggests our estimates may understate true effects.

JEL codes: C93, I21, I22, I25

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1 Introduction

There is a growing evidence base suggesting that individuals who develop math and science skills are more likely to be employed and earn higher wages once they enter the labor market. In the United States, a standard deviation (SD) in math test scores at the end of secondary school is associated with 12% higher earnings during adulthood (Mulligan, 1999; Lazear, 2003). A third to a half of those gains are explained by higher achievement (Murnane et al., 2000).[1] The returns to such skills may be even larger in low- and middle-income countries (LMICs). For example, in India, studying math and science in secondary school is related to more years of education, completing a professional degree, returns to entrepreneurship, working in the public sector, and 22% greater earnings than either business or humanities (Jain et al., 2022).[2] Math and science scores on international assessments predict earnings (Hanushek and Zhang, 2009; Hanushek et al., 2017) and economic growth (Lee and Lee, 1995; Hanushek and Kimko, 2000; Barro, 2001; Woessmann, 2003; Jamison et al., 2007; Hanushek and Woessmann, 2008).

Despite their importance, teacher capacity in these subjects is low in much of the world. Secondary-school students hoping to become teachers score below the national mean in math on global tests, and a fourth to a full SD below aspiring engineers (Bruns and Luque, 2014). Future teachers in top-performing school systems like South Korea scored a fourth of an SD above all other participating countries on a math test (Schmidt et al., 2007; Tatto et al., 2012). Gaps in teachers' content and pedagogical knowledge are even more pronounced in LMICs. For example, a study in Sub-Saharan Africa found two thirds of math teachers could not solve an algebra problem and nine in ten could not prepare a lesson using a text (Bold et al., 2017). These gaps in teacher preparation matter. In Peru, each SD on a test of teachers' content knowledge increased student achievement by about 0.1 SDs (Metzler and Woessmann, 2012).[3]

We conducted a randomized evaluation of an intervention designed to address this problem. It recruits college students ("fellows") in math and science fields to teach alongside existing teachers for a year and offers them pedagogical support while they earn a teaching certificate. We evaluated this intervention in the city of Pune, the second-largest in Maharashtra, India. We randomly assigned 48 public and *Vidya Niketan* (ViNi) primary schools (which are also publicly funded and managed, but have more autonomy on school-management decisions) to receive the intervention in grade 5 or grade 6. The 26 grade 5 and 25 grade 6 classes that received the intervention made up the treatment group and the rest served as controls.[4] Fellows were selected using content-knowledge tests, demonstration lessons, and interviews.

[1] See also Bishop (1989); O'Neill (1990); Grogger and Eide (1995); Murnane et al. (1995); Neal and Johnson (1996); Mulligan (1999); Altonji and Pierret (2001); Murnane et al. (2001) or Hanushek (2002) for a review.

[2] See also Boissiere et al. (1985); Alderman et al. (1996); Glewwe (1996); Angrist and Lavy (1997); Jolliffe (1998); Moll (1998); Behrman et al. (2008) or Hanushek and Woessmann (2008) for a review.

[3] This finding is consistent with correlational evidence across 31 countries (Hanushek et al., 2019).

[4] This randomization strategy, pioneered by Banerjee et al. (2007), ensures that all schools have an incentive to participate in all data-collection rounds. We discuss it in greater detail in section 2.3.

They received a brief training, scripts for every lesson, and support from instructional coaches. They taught math and science six hours per week while attending afternoon/evening lessons to earn their teaching certificate. They received a small stipend of about USD 4 per lesson.

We report six main sets of results. First, we document how fellows differed from teachers in the schools where they were placed to explain how they shifted the composition of instructors.[5] They were less likely to be female (63% v. 76%) and nearly half their age (21 v. 42 years old). They were also less likely to have a bachelor's degree (35% v. 84%) and had less teaching experience (1.2 v. 18 years). Yet, due to frequent transfers across schools, regular teachers on average only had about 2 years teaching math or science at their school, so the intervention did not sharply reduce the teaching experience of the average instructor in these subjects. Further, fellows scored 1.4 SDs above regular teachers in a written assessment of math and science knowledge, instructional practices, and understanding of students' misconceptions.

Second, the intervention increased students' opportunity to interact with their instructors. Control teachers already went to work frequently: 84% were found during unannounced visits, compared to 72% of fellows, and the difference between them was not statistically significant. Control teachers were also more likely than fellows to arrive on time (74% v. 44%, $p < 0.01$). Conditional on attending, however, fellows were far more likely than control teachers to be found in their classroom (56% v. 4.8%, $p < 0.01$), as opposed to somewhere else in the school.

Third, the intervention did not increase the share of lesson time devoted to instruction. Control teachers already spent 78% of such time "on task" during announced observations, compared to 75% for fellows, and the difference between them was not statistically significant. The introduction of fellows led treatment teachers to spend far less time teaching (70 pp. less than their control peers, $p < 0.01$) and more on class management (21% of time; 6.9 pp. more than controls, $p < 0.1$) and being "off task" (69% of time, 61 pp. above controls, $p < 0.01$). Thus, we interpret the effects that follow as what happens when we replace existing instructors with less trained and experienced ones with more subject-matter knowledge and scaffolding.

Fourth, fellows differed considerably from teachers in control classes in how they taught. During announced observations, they were much more likely than control teachers to ask both closed and open questions (16 pp., $p < 0.05$), ask students to explain their answers (29 pp., $p < 0.01$), allow them to ask questions (20 pp., $p < 0.05$), assign them homework (16 pp., $p < 0.1$), and praise or encourage them (20 pp., $p < 0.01$). In fact, fellows performed 0.73 SDs ($p < 0.01$) above control teachers on a composite index of these and other positive practices.

Fifth, the intervention had moderate-to-large positive impacts on student achievement. After accounting for baseline test scores, treatment students outperformed their control peers by 0.34 SDs in math and 0.22 SDs in science ($p < 0.01$ for both). Even if fellows only taught

[5]Throughout this paper, we use the term "teacher" to refer to regular teachers in public and charter schools, the term "fellows" to refer to individuals participating in the intervention that we evaluate, and "instructors" as the overarching term that includes both of these groups.

these subjects, they also increased student achievement in language by 0.15 SDs ($p < 0.01$).[6] During the study, we found that some items from the baseline test were used to coach students, but we present evidence that test-score impacts cannot be explained solely by such coaching. Treatment students still outperformed their control peers on items first introduced at endline.[7] They also fared better on contemporaneous tests drawing from a different source of items, although the material in these tests may have been better aligned with instruction in science.[8] Further, they outperformed control counterparts at the start of the next school year by 0.36 SDs in math ($p < 0.01$), 0.14 SDs in science, and 0.08 SDs in language ($p < 0.05$ for both). Both the end-of-year and next-year gains in student achievement were broad-based, with the treatment distributions stochastically dominating the control distributions for both rounds.[9]

Lastly, despite these gains, the intervention did not change students' attitudes towards math and science, intelligence and math mindsets, or aspirations to pursue related careers.[10] The null effects on these indicators suggest that the intervention did not raise achievement by boosting students' motivation or their perceived payoff from exerting effort in these subjects.

We also present three sets of robustness checks. We first show that the intervention did not increase students' attendance to school, ruling out the possibility that it improved achievement by motivating students to go to school more regularly. We then show that it did not increase students' propensity to attend private tuition in math and science or time spent on tuition, ruling out students seeking outside help on these subjects as a primary mechanism of impact. We also find that control classes were more likely to have learning and teaching materials than treatment classes, suggesting that principals may have tried to compensate the former. If correct, our estimates would actually understate the true impact of the intervention.

Our first contribution is to global evidence on what teachers know and are able to do. Internationally comparable data have traditionally tracked teachers' education and experience (OECD, 2022; UNESCO, 2022; World Bank, 2023), but such metrics do a poor job predicting teaching effectiveness (Rockoff, 2004; Rivkin et al., 2005; Kane et al., 2008; Kane and Staiger, 2008; Rockoff et al., 2011; Kane and Staiger, 2012; Kane et al., 2013; Araujo et al., 2016). Studies in LMICs have drawn attention to the high teacher absence rates in these settings (Kremer et al., 2005; Chaudhury et al., 2006; Muralidharan et al., 2017), and more recently, to

[6] Scaling results to account for differences in item characteristics yielded nearly identical results. Impacts are equivalent to 6.7 pp. in math, 3.8 pp. in science, and 3.5 pp. in language in percentage-correct scores.

[7] After accounting for baseline, treatment students outperformed controls in non-repeated items by 4.1 pp. in math ($p < 0.01$), 1.7 pp. in science ($p < 0.05$), and 3.4 pp. in language ($p < 0.01$).

[8] After accounting for baseline, treatment students outperformed control peers by 0.09 SDs in math (not significant) and 0.40 SDs in science ($p < 0.01$).

[9] We found no heterogeneous effects by students' sex or caste. We found higher impacts for students with higher socio-economic status, but they mostly become statistically insignificant once we account for baseline.

[10] Most students enjoyed studying these subjects, but expressed some performance anxiety. About half believed intelligence and math skills cannot be developed, and almost as many thought boys were smarter and better at math than girls. Three-fourths wanted to study math or science in high school, more than two-thirds wanted to continue studying after high school, and a third aspired to a job involving math or science.

how lesson time is allocated (Abadzi, 2009; Bruns and Luque, 2014; Stallings et al., 2014). Yet, we still have a relatively narrow understanding of teachers' competence in these contexts.[11] Our study presents a rare comprehensive account of teachers' work in a developing setting, drawing on surveys to describe their background and credentials, assessments to measure their content and pedagogical knowledge, surprise school visits to track their attendance, and class observations to monitor time allocation and identify the prevalence of instructional practices.[12]

Grounded in this understanding, we also advance causal research on how to improve math and science instruction in contexts of low teacher capacity. Prior work largely takes gaps in the preparation of the current stock of teachers as given and focuses on mitigating its effects, automating parts of their work—e.g., using pre-recorded lessons (Naslund-Hadley et al., 2014) and lesson segments (Beg et al., 2022; de Barros, 2022)—or asking students to learn on their own—e.g., with inquiry-based (Beuermann et al., 2013; Bando et al., 2019) and peer-to-peer learning (Wachanga and Mwangi, 2004; Ajaja and Eravwoke, 2010; Berlinski and Busso, 2017). Our study shows that it is possible to raise the capacity of the teaching labor force by recruiting individuals with subject-matter expertise and providing them with pedagogical support.[13]

Finally, and relatedly, we contribute to the literature on scripted lessons in LMICs. Previous studies have shown that, in contexts of extremely low teacher capacity (e.g., little or no post-secondary education), scripts that tell teachers what to do at each stage of a lesson can ensure a minimum "floor" of instructional quality (Piper and Korda, 2010; Tilson et al., 2013; Piper et al., 2018; Albornoz et al., 2020; Romero et al., 2020; Gray-Lobe et al., 2022). In fact, structured pedagogy more broadly was recently identified as one of the most cost-effective interventions to improve learning outcomes in LMICs (Akyeampong et al., 2023). Our study illustrates how such scripts can also be provide scaffolding to instructors who have high levels of subject-matter expertise but low levels of pedagogical training and experience. It is also one of the first to document the extent to which instructors adhere to the different components of scripts and to examine which types of teachers are more likely to use them.

The rest of the paper is structured as follows. Section 2 describes the context, intervention, sampling, and randomization. Section 3 presents the data. Section 4 discusses the empirical strategy. Section 5 reports the results. Section 6 discusses implications for policy and research.

[11] International surveys of teachers (e.g., the International Evaluation Association's Teacher Education and Development Study in Mathematics [TEDS-M] or the Organization for Economic Cooperation and Development's Teaching and Learning International Survey [TALIS]) offer rich descriptions of teachers' work, but only a handful of LMICs have ever participated in them (Tatto et al., 2012; OECD, 2019).

[12] For similar work, see Bhattacharjea et al. (2011); Bold et al. (2017); World Bank (2017).

[13] To our knowledge, there is only one other study on a similar approach in a LMIC, but it is not causal (it uses propensity-score matching), it was conducted in a country subsequently categorized as high income (Chile), and it evaluates an initiative that does not intend to keep recruits in teaching (Alfonso et al., 2010).

2 Experiment

2.1 Context

We conducted our study in the city of Pune, the second-largest city in the Indian state of Maharashtra (after its capital city, Mumbai). According to the latest census of India (in 2011), there are 9.4 million people in Pune (NIEPA, 2017), rendering its population size comparable to that of countries such as Belarus, Honduras, and the United Arab Emirates (UN, 2019). Its school system is run by the Pune School Board (*Shikshan Mandal* or PSB) under the Pune Municipal Corporation (PMC). On the latest school year with available data, there were 3,473 primary-only schools (grades 1-5) and 1,941 additional primary schools with upper primary (grades 6-8), with 834,354 students enrolled in both types of primary schools (NIEPA, 2017). For reference, if Pune were a school system in the U.S., it would rank between the top two largest districts in number of students: New York City and Los Angeles Unified (NCES, 2018).

Nearly all primary-school aged children in Pune are enrolled in school: the gross enrollment rates are 110% in primary and 109% in upper primary.[14] Marathi is the language of instruction in 76% of primary-only schools and 55% of those with upper primary; English-medium schools account for 23% and 41% of these types of schools, respectively.[15] About 85% of primary-only schools and 63% of primary schools with upper primary are "government" (i.e., public) schools. There are, on average, 24 students per teacher in primary-only schools and 32 in primary schools with upper primary, and these numbers closely track class sizes (NIEPA, 2017).

The public sector employs most primary-school teachers: 61% of teachers in primary-only and 47% of those in schools with upper primary work in government schools. The vast majority of teachers in these types of schools are "regular" (i.e., tenured): 93% and 84%, respectively; the rest are hired on a renewable contract basis (NIEPA, 2017).

Most primary-school children in Pune lack basic math and science skills. According to a nationally representative student assessment administered by the central government, only 30% of fifth-graders in Pune could use arithmetic for daily situations, 38% could identify equivalent fractions, and 34% could estimate a volume. Results for science were equally discouraging: 23% could identify linkages between terrain, climate, and resources; 29% could group objects, materials, and activities according to properties such as shape, color, and sound; and 45% could estimate spatial quantities in simple standard units (NCERT, 2018).

[14] The gross enrollment rate indicates the number of children enrolled at a given education level *irrespective of age*, divided by the number of children *of age* to attend this level and multiplied by 100. Gross enrollment rates often exceed 100% because the denominator includes both younger and older children. The net enrollment rate (the number of children enrolled at a given level who are of age to attend such level, divided by the total number of children of age for that level) is only reported for upper primary and it is 91% (NIEPA, 2017).

[15] According to anecdotal evidence, a non-trivial share of instruction in these schools is also in Marathi.

2.2 Sample

We selected 48 public and *Vidya Niketan* primary schools for this study.[16] We started with all 286 PSB-run primary schools. We excluded 118 schools away from the city center (because their location would have limited the capacity of the non-profit running the intervention to monitor its implementation), 30 Urdu-medium schools (because most fellows did not speak Urdu),[17] 59 schools where the PSB or other non-profits were conducting other programs (because we wanted to estimate of the effects of the intervention on its own), 20 schools with low enrollment (to minimize sampling error), and nine schools that already participated in the intervention (because we wanted to estimate the effects of the first year of the intervention). Our data-analytic sample includes 46 of the 48 sampled schools. Shortly after baseline, we had to drop two schools that could not be matched to any fellows based on their preferences.

2.3 Randomization

We randomly assigned the 48 sampled schools to receive the intervention in grades 5 or 6. This process resulted in 26 grade 5 and 25 grade 6 treatment classrooms and 26 grade 5 and 25 grade 6 control classrooms, such that all schools had at least one classroom with a fellow.[18] This randomization strategy, pioneered by Banerjee et al. (2007), seeks to minimize the risk of differential attrition (i.e., schools without any intervention dropping before the endline).

We also randomly assigned fellows to schools, conditional on their preference set. First, we grouped fellows based on their preferred neighborhood, school shift (morning or afternoon), and medium of instruction (English or Marathi). Then, we ran 17 lotteries—one per preference set (e.g., one lottery for neighborhood 1, morning shift, English medium schools).

Table 1 presents summary statistics on students and compares the characteristics and achievement of students between experimental groups. The mean control-group student was 11 years old, which is expected given that the sample is split between grades 5 and 6. Most control students (69%) speak Marathi at home, some (17%) speak Hindi, and few (1%) speak English. Less than two-thirds of them have mothers who completed primary school and more than three-fourths have fathers who reached this level. Nearly all of them (90%) have a TV, but fewer have Internet (43%), a desk (28%), a computer (20%), or their own room (17%),

[16] These schools are publicly funded and managed like regular public schools, but they have more autonomy over school-management decisions.

[17] Note, however, that Urdu-medium schools account for about 1% of primary-only schools and 3% of schools with upper primary in Pune (NIEPA, 2017).

[18] Two schools had two grade 5 and two grade 6 classrooms and two other schools had *either* two grade 5 *or* two grade 6 classrooms. In both cases, we assigned all classrooms within the same grade to the same experimental group to prevent contamination, which is particularly likely to occur within the same grade (e.g., a grade 5 fellow in a treatment classroom sharing materials with the grade 5 teacher in a control classroom). All other schools had one treatment and one control classroom (either grade 5 or grade 6).

suggesting that the schools where SEI places its fellows serve relatively low-income families. Yet, two in three of these students attends tuition in math and one in four does so in science.[19]

We find no systematic differences in the characteristics or achievement of students across experimental groups. By chance, treatment students had lower achievement in math ($p < 0.1$), so we estimate the effect of the program with and without accounting for baseline achievement.

2.4 Intervention

The intervention we evaluated was the Science Education Initiative's Fellowship Program.[20] SEI is a Pune-based non-profit organization dedicated to improving math and science learning and the fellowship was its flagship program. Since 2014, it has placed 200 fellows in 110 classes. It recruits college students ("fellows") majoring in science, technology, engineering, and math (STEM) to teach math and science in schools serving disadvantaged students for one year.[21]

Fellows are selected through a competitive four-stage process. In stage 1, they take a test of content knowledge based on the math and science curricula for grades 5 to 10.[22] In stage 2, they deliver a brief (5- to 7-minute) demonstration lesson on a topic of their choice. In stage 3, they participate in an interview to assess their scientific aptitude, leadership qualities, and motivation to teach. In stage 4, they deliver a longer (15- to 20-minute) demonstration lesson on a topic chosen by SEI. Those who succeed in all four stages are provisionally selected, but their appointment as fellows is not finalized until they complete pre-service training.[23]

All admits complete a three-week pre-service training. This training focuses on pedagogy, classroom management, and math and science knowledge. Once they start teaching, fellows also complete a bachelor's degree in education (paid for by SEI) at a partner teacher-training college, so all fellows become certified teachers by the end of their one-year appointment.

SEI fellows differ from PMC teachers in both their expected workload and remuneration. PMC teachers teach either math or science during 30-minute lessons and they may teach two or more lessons of the same subject on the same day. At the time of the study, the median stipend for a fellow was INR 50,000 (USD 601) per month. SEI fellows teach math and science during 120-minute lessons to minimize the number of times per week they travel to the school. They are expected to teach three lessons per week for one year and they receive a stipend of INR 250 per lesson or INR 3,000 (USD 47) per month.

SEI fellows also differ from PMC teachers in their background (Table 2). First, they are less likely to be female (63% v. 76%) and, on average, nearly half their age (21 v. 42 years old).

[19] Private tuition is common in urban India, even among low-income families (Berry and Mukherjee, 2019).

[20] After our study, SEI changed its name to Science for All.

[21] SEI also has a fellowship for college graduates, which we did not evaluate in the present study.

[22] These curricula are jointly determined by the National Council of Educational Research and Training (NCERT) and the Board of Education of Maharashtra.

[23] There are no language requirements, but applicants proficient in Marathi are prioritized, given that most primary schools in Pune are Marathi-medium schools.

Second, they have fewer years of education. Only one in three fellows has a bachelor's degree (most are still pursuing their first university degree), compared to 84% of teachers. Third, fellows are less experienced. They only complete one year of teaching, whereas the average teacher had accrued 18 years of teaching experience, five of which were at their current school, and two of which focused on math or science. Lastly, fellows outperform teachers by 14 pp. on a test that we developed (described in section 3) and their scores vary less (see Figure A.1 in appendix A). In fact, they fare better in all domains of the test, including content knowledge (by 10 pp.), instructional practices (by 20 pp.), and student misconceptions (by 17 pp.)

Fellows were expected to teach using lesson scripts, which specified in considerable detail the topics to be taught on each day, the teaching and learning materials to be used, the words to be written in the blackboard, the activities to complete, and the questions to ask students. During announced classroom observations, we tracked whether fellows followed the scripts. The vast majority of fellows adhered to recommendations on what to write on the board and ask students, but only half used the suggested materials and activities, and a fifth to a third deviated in some way (making additions, changes, or exclusions; see Table A.1 in appendix A). In general, fellows who scored above the median in the test of content knowledge, instructional practices, and understanding of students' misconceptions were more likely to follow the scripts.

The total cost of running the fellowship in 2017-2018 was INR 3,662,000 (USD 56,867). It accounted for almost half of SEI's budget for that year.[24] With 60 fellows and 1,920 students that year, it cost INR 61,033 (USD 948) per fellow and INR 1,907 (USD 30) per student.[25]

3 Data

As Table 3 shows, we conducted four rounds of data collection, including: (a) student surveys and assessments at baseline (to check the comparability of experimental groups, increase the precision of our estimates, and test for heterogeneous effects); (b) unannounced school visits (to estimate the impact of the intervention on instructor attendance and punctuality) and announced classroom observations (to estimate effects on lesson-time allocation and pedagogical practices) during the school year; (c) student surveys and assessments (to estimate impacts on achievement, attitudes, mindsets, and aspirations) and instructor surveys and assessments (to compare PMC teachers with SEI fellows) at endline; and (d) student assessments at follow-up (to check whether impacts on achievement persisted over time).

[24]The other half was spent on organization building (29%), the graduate fellowship (12%), training (8.5%), research and innovation (2.6%), and technology (1.2%).

[25]Of those 60 fellows, only 48 are part of the present study.

3.1 Student surveys

We administered a short student survey at baseline focusing on background characteristics (e.g., sex and socio-economic status) to test for heterogeneous effects, and a longer one at endline measuring constructs that may be affected by the intervention (e.g., attitudes towards math and science, intelligence and math mindsets, and educational and career aspirations).

3.2 Student assessments

We administered student assessments of math and science (the two subjects targeted by the intervention) and language (to test for spillover effects) at baseline, endline, and follow-up. All assessments evaluated what students know and are able to do based on global standards.[26] Each test had 30 multiple-choice items.[27] We included items from a wide range of difficulty levels to reduce the possibility of "floor" and "ceiling" effects (i.e., students answering no or all questions correctly, respectively).[28] We present the results in three ways: percentage-correct scores (i.e., the percentage of items answered correctly), standardized scores (at baseline, with respect to the overall distribution; at endline and follow-up, with respect to the control group), and Item-Response Theory (IRT) scores to account for differences in students' ability and item characteristics (i.e., difficulty and differentiation between students of similar ability).[29]

Right before the endline, we noticed that SEI's instructional coaches had used items from the baseline assessments in practice tests. We had asked the organization not to read or keep assessments, but internal miscommunication resulted in coaches making copies of the tests. This was problematic because it raised the possibility that any effects observed at endline may be solely due to coaching on repeated items (which are needed to link results across rounds). We test for this possibility in three ways. First, we estimate endline effects separately for repeated items (which were first administered at baseline and were thus subject to coaching) and non-repeated items (which were introduced at endline and thus not subject to coaching). Then, we estimate endline effects on "audit" assessments of math and science, which drew on different items from a concurrent evaluation (Gray-Lobe et al., 2022), to a subset of students. Lastly, we also estimate effects on follow-up assessments of math, science, and language at the start of the next school year to all students in our sample.

[26] The math and science tests were based on the 2019 Trends in International Math and Science Study assessment framework (IEA, 2017). The math test covered three content domains (numbers, geometry and measurement, and data display) and three cognitive domains (knowing, applying, and reasoning). The science test covered three content domains (life, physical, and earth science) and the same cognitive domains as the math test. The language test was based on the 2016 Program for International Reading Study framework (IEA, 2015). It covered three content domains (vocabulary, grammar, and reading) and three cognitive domains (retrieving explicit information, making inferences, and interpreting and integrating ideas and information).

[27] At baseline and follow-up, we created two versions of each assessment to prevent cheating.

[28] We used items from international assessments, domestic assessments, and impact evaluations in India. Figures A.2 and A.3 shows the distributions of proportion-correct and IRT-scaled scores, respectively.

[29] We used a two-parameter logistic IRT model (Yen and Fitzpatrick, 2006).

3.3 Unannounced school visits

We conducted unannounced visits to school during the school year to estimate the impact of the intervention on instructor attendance and punctuality by comparing SEI fellows to PMC teachers in control and treatment classes. We did not announce these visits to minimize the chances that instructors would attend school, or would do so earlier than usual, because of us. We also collected administrative data and counted the number of students in the classroom to estimate the impact of the intervention on student attendance and punctuality.[30]

3.4 Announced classroom observations

We conducted announced classroom observations during the school year to estimate the impact of the intervention on instructor lesson-time allocation by comparing SEI fellows to PMC teachers in control and treatment classes.[31] We announced our observations because we were interested in how teachers used lesson time when they attended. We also collected data on whether fellows and control teachers engaged in certain practices during the lesson.

3.5 Instructor surveys

We administered a survey of instructors at endline to compare SEI fellows to PMC teachers on background characteristics (e.g., sex, education, training, and experience) to understand how the intervention had changed the composition of instructors to which students were exposed.

3.6 Instructor assessments

We administered instructor assessments at endline to compare SEI fellows to PMC teachers on content knowledge, instructional practices, and understanding of students' misconceptions. The test had 36 multiple-choice items.[32] The items on content knowledge were sampled from the student assessments. Those on instructional practices presented objectives for hypothetical lessons and asked respondents to choose their preferred approach to pursue those goals. Those on student misconceptions presented mistakes that students made and asked respondents to identify the most likely underlying reason for students' misunderstanding.

[30] We supplemented these measures with survey-based measures from endline.

[31] We adapted a classroom-observation protocol that has been widely used in LMICs, including India (Stallings, 1977; Bruns and Luque, 2014; Sankar and Linden, 2014; Stallings et al., 2014; World Bank, 2017).

[32] We drew on items from international assessments, domestic assessments, and previous assessments of teacher knowledge and skills conducted by domestic organizations (e.g., Pratham, Educational Initiatives) and international organizations (e.g., Educational Testing Service, Bridge International Academies).

4 Empirical strategy

We estimate the intent-to-treat effect of the offer of the intervention by fitting the model:

$$Y_{igs}^{t} = \alpha_{r(g)} + Y_{igs}^{t=0}\gamma + T_g'\beta + \epsilon_{igs} \quad (1)$$

where Y_{igs}^{t} is the outcome of interest for student i in grade g and school s at endline ($t = 1$) or follow-up ($t = 2$); $Y_{igs}^{t=0}$ is a measure of that outcome at baseline (when available); $r(g)$ is the randomization stratum of grade g and $\alpha_{r(g)}$ is the corresponding stratum fixed effect; T_g is an indicator variable for random assignment to the intervention; and ϵ_{igs} is an error term. The parameter of interest is β, which captures the causal effect of the offer of the intervention. We estimate equation (1) by ordinary least-squares regression. We use cluster-robust standard errors to account for within-school correlations across students in outcomes.

We also fit variations of this model in which outcomes are measured at the instructor level (to estimate the impact of the intervention on instructors) and models that interact the intervention indicators with student and teacher covariates (to test for heterogeneous effects).

5 Results

5.1 Instructor attendance and punctuality

Previous research has found that teachers in India are often absent to school (see, e.g., Kremer et al., 2005; Muralidharan et al., 2017). Yet, most of these studies focused on public-school (rather than charter-school) teachers in rural (rather than in urban) areas. Therefore, while we believed that the SEI fellowship could increase students' exposure to instructors, we did not know what business-as-usual absence and punctuality rates would be in our context.

We found that SEI fellows were no more likely than PMC teachers in control classes to go to work or arrive on time—partly, because the latter were already doing so at fairly high rates. As Table 4 shows, 84% of control teachers were present during unannounced visits, compared to 72% of fellows, and this difference was not statistically significant. In fact, fellows were 30 pp. *less* likely to arrive on time than control teachers ($p < 0.01$; panel A, cols. 1, 4, and 5).

Prior research also suggested that the introduction of fellows could affect the attendance and punctuality of treatment teachers, but the direction of the expected effect was not clear. On the one hand, teachers hired on a contract basis have previously led to *reductions* in the attendance and punctuality of civil-service teachers (Muralidharan and Sundararaman, 2013; Duflo et al., 2015). Yet, both the fellows and charter-school teachers in our context differ considerably from the contract and public-school teachers in those studies. On the other hand, the hiring of extra workers in pre-school centers in India *increased* the attendance and

punctuality of main workers (Ganimian et al., 2023). Yet, the main workers in this particular context had to open the centers for the extra workers. It was not clear that any of these studies shed light on what would happen in our setting.

We found that the introduction of fellows did not increase the attendance or punctuality of teachers in treatment classes. Treatment teachers were slightly more likely than their control counterparts to be present (88% v. 84%) and less likely to arrive on time (71% v. 74%) during the unannounced visits, but neither difference was statistically significant (cols. 1-3).

If we had only measured attendance and punctuality, as most prior studies have done, we would have concluded that the intervention had no effect on students' exposure to instructors. Fortunately, however, we also tracked where fellows and teachers were during each school visit. We found that, conditional on being at school, fellows were far more likely to be in their class. Only 4.8% of control teachers were in their classroom, compared to 56% of fellows, a difference of almost 51 pp. ($p < 0.01$; panel B, cols 1, 4, and 5). Fellows did not impact the likelihood of treatment teachers of being in their classroom, which was nearly identical (4.7%; cols. 2-3).

In short, the intervention seems to have increased students' opportunities to interact with their instructors not by reducing instructor absence and tardiness, but rather by increasing the probability that the instructor would be in the classroom when they are at school.

5.2 Instructor lesson-time allocation

Several studies in India, across pre-primary, primary, and secondary education, have found that when teachers are at school, they devote most of their lesson time to instruction (instead of managing student behavior or other tasks; see Bhattacharjea et al., 2011; Sankar and Linden, 2014; World Bank, 2018; Ganimian et al., 2023). Thus, it seemed unlikely that the intervention would increase the share of instructional time.

We found that SEI fellows did not devote a larger proportion of their lessons to instruction than PMC teachers—largely, because the latter were already teaching for most of their lessons. As Table 5 shows, the average control teacher devoted 78% of their lesson to instruction during announced classroom observations, compared to 75% for the average fellow, and the difference between these groups was not statistically significant (panel A, cols. 1, 4, and 5).[33]

As mentioned in section 5.1, contract teachers have been found to reduce the effort of regular teachers (Muralidharan and Sundararaman, 2013; Duflo et al., 2015). Therefore, even if treatment teachers did not increase their absence or tardiness as a result of the intervention, we were interested in whether they could reduce their effort in other ways. To explore this possibility, we tracked how treatment teachers allocated their time while fellows were teaching.

[33]As discussed in section 2.4, while PMC teachers teach either math or science in 30-minute lessons, SEI fellows teach both math and science combined in 120-minute lessons. Therefore, while we compare these groups focusing on the proportion of lesson time devoted to each type of activity, we also report the number of minutes devoted to each category in panel B of Table 5.

These teachers were supposed to remain in the classroom during these lessons and to leverage their education, training, and experience to support their junior peers.

We found that treatment teachers rarely engaged with the fellows while they were teaching. The typical teacher in this group spent 8% of a fellow's lesson teaching, 21% managing the class, and 69% being off task—61 pp. more than the typical control peer ($p < 0.01$; cols. 1-3).

As discussed in section 2.4, fellows were expected to use scripts that specified what and how they are supposed to teach each lesson and the vast majority of fellows adhered to them. Therefore, we examined whether they allocated their lesson time differently from teachers.

We found that fellows allocated their lesson time similarly to teachers in control classes. Control teachers spent most of their lesson time lecturing and explaining (34%), asking and answering questions (18%), and assigning students classwork (13%; Table A.2 in appendix A). The corresponding figures for fellows were nearly identical (34%, 15%, and 16%, respectively). In fact, fellows also resembled teachers in their use of class management and off task time (Tables A.3-A.4), suggesting that time allocation was not a primary mechanism of impact.

5.3 Instructor pedagogical practices

In recent years, several initiatives have adapted classroom-observation protocols originally developed for high-income countries to fit the realities of low- and middle-income contexts (see, e.g., Bruns and Luque, 2014; Stallings et al., 2014; De Gregorio et al., 2016; Wolf et al., 2018; Molina et al., 2020). Despite these valuable efforts, we still know relatively little about the efficacy of specific pedagogical practices in these settings. We believed that the intervention could potentially reduce the frequency of negative practices (e.g., shouting at or hitting students) and/or increase the frequency of positive practices (e.g., praising or encouraging students), so we measured both types of practices (see section 3.4).[34]

We found that the intervention did not reduce the frequency of negative teaching practices —largely, because these practices were quite rare. As Table 6 shows, in control classes, only 19% of teachers taught from the same spot and 5% or fewer remained sitting down, used their phone, got upset at incorrect answers, or was aggressive towards students (panel A, col. 1). Fellows were 13 pp. less likely than control teachers to teach from the same spot ($p < 0.05$), but they were also 12 pp. more likely to get upset at incorrect answers ($p < 0.1$), and they did not differ statistically significantly on a composite index of all negative practices (cols. 4-5). Fellows did not reduce treatment teachers' engagement in these practices either (cols. 2-3).

Instead, fellows engaged more frequently in positive practices that were already happening. Many control teachers used closed- and open-ended questions (63%), asked students to explain their answers (47%), corrected wrong answers (72%), allowed students to ask questions (39%),

[34]As discussed in section 3.4, we only collected these data for teachers in control classes and fellows in treatment classrooms (i.e., not teachers in treatment classes), so our discussion here focuses on those groups.

provided individual help (68%), assigned homework (48%), and praised or encouraged students (76%; panel B, col. 1). Fellows were even more prone to pursue these practices. In fact, they scored 0.73 SDs higher on a composite index of all positive practices ($p < 0.01$; cols. 4-5).

5.4 Student achievement

Given the intervention's focus on math and science, we expected it to mainly impact students' test scores in these two subjects. We thought it may also impact their test scores in language if, during their lessons, fellows defined difficult words, offered opportunities to practice reading, and/or gave students feedback on their writing. These spillovers are not uncommon in contexts in which there is ample room for improvement and scarce opportunities for remedial support (see, e.g., He et al., 2008; Lai et al., 2013).

After one school year, the intervention improved student achievement in math and science. As Table 7 shows, the endline standardized test scores of treatment students were 0.247 SDs higher in math and 0.207 SDs higher in science than those of their control peers ($p < 0.01$; panel A, cols. 1 and 3). If we account for baseline performance, estimates are slightly higher ($p < 0.01$; 0.340 SDs and 0.216 SDs, respectively; cols. 2 and 4). We obtain nearly identical results if we scale the endline results using a two-parameter logistic IRT model to account for differences in students' latent ability and item characteristics (Table A.6). These effects are equivalent to 4.9 pp. in percent-correct scores in math and 3.8 pp. in science (Table A.7).

The intervention also had spillover effects on language. Treatment students outperformed their control counterparts by 0.132 SDs or 0.151 SDs once we account for baseline performance ($p < 0.05$ and $p < 0.01$, respectively, cols. 5-6), even if fellows did not teach this subject. IRT-scaled impacts are very close (Table A.6). This effect translates into 3.1 pp. (Table A.7).

As discussed in section 3.2, right before the endline, we noticed that the nonprofit that was implementing the intervention had coached students on items from our baseline assessments. To check whether differences in test scores across experimental groups were due to coaching, we first estimated the effect of the intervention separately for endline items that had been used at baseline ("repeated" items) and for those that were introduced at endline ("non-repeated").

This approach suggests that endline impacts are not entirely explained by item coaching. As Table 8 shows, treatment students outperformed their control peers by a larger margin on repeated items than on non-repeated items (panels B and C).[35] In fact, in most specifications, we can reject the null hypothesis that effects on these two sets of items are equal (last row). Yet, treatment students still fared better than their control peers on the non-repeated items

[35]In this table, we report effects in proportion-correct scores because the mean score for control students differs across these two sets of items. However, to help the reader understand the magnitude of the difference between these two sets of impacts, we include the effects on total proportion-correect scores from Table A.7. In the notes, we also report standardized effects using the control means for repeated and non-repeated items.

by 2.8 pp. in math ($p < 0.01$), 1.8 pp. in science ($p < 0.05$), and 3 pp. in language ($p < 0.05$; panel C, cols. 1, 3, and 5). Effects are larger if we account for baseline (cols. 2, 4, and 6).

During the endline, we also administered "audit" assessments of math and science, which drew on a different set of items from a concurrent impact evaluation (Gray-Lobe et al., 2022). These assessments offer further evidence that coaching does not fully account for the impacts. As Table 7 shows, treatment students also outperformed control students in the endline audit test by 0.046 SDs in math and 0.367 SDs in science (panel B, cols. 1-4). Only the latter are statistically significant ($p < 0.01$), but this may be because the items on these tests may have overlapped more with the material taught in that subject.

We also administered "follow-up" assessments on all subjects at the start of the next year. The purpose of these assessments was twofold: we wanted to check that differences between groups were due to learning instead of coaching, and if so, to explore whether they persisted. These assessments offer more proof that the intervention impacted learning beyond coaching. Treatment students scored 0.283 SDs higher than control students in math ($p < 0.01$) and 0.120 SDs in science ($p < 0.05$; panel C, cols. 1 and 3) or 0.356 SDs and 0.142 SDs accounting for baseline (cols. 2 and 4). They also did better in language (by 0.059 SDs; col. 5), but the effect is only statistically significant if we account for baseline (0.081 SDs, $p < 0.05$; col. 6).

The intervention improved test scores across all levels of the achievement distribution. Quantile treatment effect plots show that the treatment distributions first-order stochastically dominate the control distributions on the endline, audit, and follow-up tests, suggesting that the intervention led to broad-based gains (Figure A.4). Non-parametric estimates of average treatment effects at each percentile of the baseline scores also show large positive impacts for all three rounds of data collection across the full range of baseline achievement (Figure A.5). Consistent with these results, we find no heterogeneous effects by baseline scores (Table A.8).

We find some evidence that the intervention was more effective for students with higher socio-economic status. In our estimation of endline effects, the interaction term between the treatment and the first principal component of a principal-component analysis of home assets (SES index) is positive and statistically significant for all subjects, and the p-value of the sum of the main and interaction effects of this index is statistically significant for math and science (Table A.9, panel A, cols. 1, 3, and 5). Once we account for baseline performance, however, the p-value of the sum is no longer statistically significant for any subject (cols. 2, 4, and 6). Further, we do not see any evidence of heterogeneity by SES in the endline audit (panel B). The interaction between the treatment and SES index is positive and statistically significant for the follow-up (panel C, cols. 1, 3, and 5), but this interaction and the p-value of the sum only remain statistically significant for science after we control for baseline (cols. 2, 4, and 6).

We do not find evidence of heterogeneous effects by other student or teacher characteristics. The interaction between treatment and an indicator for female students is positive for all

subjects at endline, but it is statistically significant only for math once we account for baseline (Table A.10, panel A) and statistically insignificant in the other two rounds (panels B and C). The interaction between treatment and an indicator for students from scheduled castes or tribes is negative but statistically insignificant for nearly all subjects and rounds (Table A.11). Lastly, students do not benefit more from the intervention when they have an instructor of the same sex (Table A.12) or one who scores higher on the written assessment (Table A.13).[36]

5.5 Student attitudes, mindsets, and aspirations

We considered whether the intervention changed students' attitudes towards math and science. Specifically, we wanted to understand whether, due to their higher subject-matter expertise, fellows could reduce students' anxiety towards or increase their enjoyment of these subjects.

Our data reveal that most control students already enjoy these subjects, but some also find them challenging and anxiety producing, and the intervention did not change this pattern. About eight out of ten control students liked learning math and science and almost as many found them useful, but a fourth felt nervous about them, a third wished they did not have to study these topics, and a fifth gave up when the material is difficult. Treatment students did not differ statistically significantly on a composite of these measures (Table A.15, panel A).

We also explored whether the intervention changed students' intelligence or math mindsets. Several studies have found that students in LMICs often hold "fixed" mindsets in these domains (i.e., believing that they are largely inherited and that they cannot be developed), which in turn correlate negatively with their achievement (see, e.g., Claro et al., 2016; Alan et al., 2019; Outes et al., 2017; Ganimian, 2020; Dweck and Yeager, 2021; OECD, 2021).

Fixed mindsets were common among control students and fellows did not dispel them. About half of control students believed intelligence and math skills cannot be developed, and four in ten thought boys are both more intelligent and skilled at math. The intervention had a negative but statistically insignificant effect on all of these beliefs (Table A.15, panel B).

Lastly, we examined whether the intervention impacted students' career trajectories. Specifically, we wanted to know whether it made them want to stay in school, study math and science, or pursue a STEM-related job through role-model effects or better-quality instruction. Over two thirds of control students planned to pursue post-secondary education, three fourths wanted to study a STEM subject in high school, and a third aspired to a STEM-related job. The intervention had statistically insignificant effects on these metrics (Table A.15, panel C).

We did not find any evidence of heterogeneous effects on any of these sets of outcomes by students' sex, caste, socio-economic status, or baseline achievement (Tables A.16-A.19).

[36] In fact, if we estimate the effect of the program only among high-scoring instructors (i.e., PMC teachers and SEI fellows), the point estimates (Table A.14) resemble the average effects of the intervention (Table 7), suggesting that effects are not explained solely by a selection effect.

5.6 Robustness checks

We conducted three sets of robustness checks to rule out alternative mechanisms of impact. First, we checked whether the intervention could have increased achievement mostly by making students more likely to go to school. We measured student attendance in three different ways: by calculating the share of present students observed during unannounced visits, digitizing school attendance records, and asking students how often they were late to or missed school. None of these approaches suggest the intervention impacted student attendance (Table A.20).

We also checked whether the intervention made students more likely to seek private tuition. As it has been documented elsewhere, many Indian students already attend private tuition—even those from low-income families (e.g., Jayachandran, 2014; Berry and Mukherjee, 2019). Accordingly, between a fifth and a third of students in our sample receive tuition in one subject. The intervention did not impact their propensity to seek tuition in a target (math and science) or non-target subject (English and Urdu) or the amount of tuition received (Table A.21).

Whenever resources (e.g., fellows) are (randomly) allocated to some classes and not others, principal may try to compensate the non-selected classes with other resources (e.g., materials). To explore this possibility, we leveraged the information on class materials collected during the announced observations to compare the availability of materials across experimental groups. Control classrooms tend to have more materials than treatment classrooms, and they are statistically significantly more likely to have textbooks for teachers (52 pp.) and students (46 pp.; $p < 0.01$ in both cases) and math or science equipment (14 pp., $p < 0.05$; Table A.22). These results suggest that our estimates may understate the true impact of the intervention (i.e., the impact if all resources were equally distributed across control and treatment classes).

6 Conclusion

Developing math and science skills can help individuals improve their economic prospects. Yet, there is very little evidence on how to improve instructional quality in these subjects—especially, in contexts with low teacher capacity, which are more prevalent among LMICs.

We present experimental evidence on a novel approach to raise teacher capacity in math and science: hiring college students in these fields and providing them pedagogical support. We find that the introduction of these fellows results in moderate-to-large gains in student achievement in both of the subjects that they teach as well as in other subjects, and that those gains persist over time, influencing their students' preparation level for the next school year. We also identify potential mechanisms of impact (e.g., increase in instructors' likelihood to be in their class, increase in their use of positive pedagogical practices) and rule out confounders (e.g., increase in instructor or student attendance or in student demand for private tuition).

These findings shed new light on the importance of teachers' subject-matter expertise. Until recently, its value had been largely inferred from correlations with students' performance on standardized tests (Hill et al., 2005; Santibanez, 2006; Rockoff et al., 2011; Kane et al., 2013; Gitomer et al., 2014; Bold et al., 2017; Cruz-Aguayo et al., 2017; Hanushek et al., 2019). We only know one quasi-experimental study on this question (Metzler and Woessmann, 2012). We provide experimental evidence that hiring instructors with high levels of content knowledge to replace teachers with lower levels for part of the school week raises student achievement.[37] The fellows in our study received considerable pedagogical support (e.g., pre-service training, lesson scripts, instructional coaches), so we cannot attribute their impact to their knowledge. Yet, if this intervention were to be taken to scale, fellows would likely receive such supports, so the parameter of policy interest is the estimated effect of combining fellows and supports.

Our results also add nuance to education-policy debates on the merits on lesson scripts. Opponents often argue that any gains from standardizing instruction must be weighted against the ensuing losses to teachers' professional autonomy (Valencia et al., 2006; Dresser, 2012). Yet, we found that the vast majority of fellows adhered to lesson scripts, suggesting that they may be a useful complement even for individuals with high levels of subject-matter expertise. In fact, the fellows who performed best in the assessment of content and pedagogical knowledge were less likely to omit, change, or add to the material in such scripts, implying that the fellows who were most likely to exercise discretion were precisely those least well equipped to do so. Future research should explore the extent to which this pattern is observed in other settings, and if so, how scripts can best encourage instructors to leverage their expertise appropriately.

Having demonstrated that this approach works, a logical next question is whether it could be implemented with comparable levels of fidelity and improve student achievement at scale. Of the reasons identified by List (2022) for why interventions that are successful in efficacy trials fail to sustain gains at scale, we are least concerned with the credibility of the evidence. Our randomized evaluation offers one of the most rigorous studies to date demonstrating that hiring individuals with subject-matter expertise can improve student achievement in a LMIC. The fact that we relied on a convenience sample, however, suggests that this intervention has more chances of scaling in urban, medium-sized, English-language public and charter schools. The motivation and capacity of the implementing organization seems much harder to replicate. SEI not only recruited, selected, and supported fellows; it also established partnership with the local governments to place them in schools and with teacher-training colleges for certification; and it created detailed lesson scripts and hired instructional coaches for pedagogical support.[38]

[37] While fellows were expected to teach alongside regular teachers, as we show in section 5, the latter were typically not engaged in instructional tasks while the former were teaching.

[38] Although this combination of know-how may be rare on a global scale, some of India's most successful education organizations already engage in similar tasks (e.g., Pratham, Educational Initiatives, Central Square Foundation, and their partner institutions), so such talent may be easier to find within the country.

Yet, perhaps the most important challenge to take this intervention to scale is to identify enough college students in math and science who are willing and able to become teachers. According to data for the 2020-2021 year, there were 32.7 million undergraduate students in India, 16% of whom studied science and 12% of whom were studying engineering (MoE, 2022). In fact, science and engineering were the second and fourth most popular majors, respectively. By comparison, there were 247,236 students in teacher-training colleges, which means that there were 21 science majors and 16 engineer majors per aspiring teacher across the country, suggesting that there is not a shortage of college students who can teach math and science.[39] Female enrollment in science and engineering is relatively high (52% and 29%, respectively), so getting more undergraduate students in these fields to enter teaching would not necessarily alter the sex breakdown in the profession. The question is whether they would *want* to teach. We do not have data on the career preferences of science and engineering majors across India. The only comparable program (Teach for India) has recruited 4,000 fellows since 2008,[40] but unlike the SEI fellowship, this program does not intend to keep its graduates in the profession. These figures, however, suggest that there is scope to increase the number of undergraduates in math and science who could enter teaching and raise the achievement of Indian students.

[39]This is true even if we include students in education, who account for 5.3% of undegraduates nationwide.
[40]See Teach for India's website: `https://www.teachforindia.org/fellowship`.

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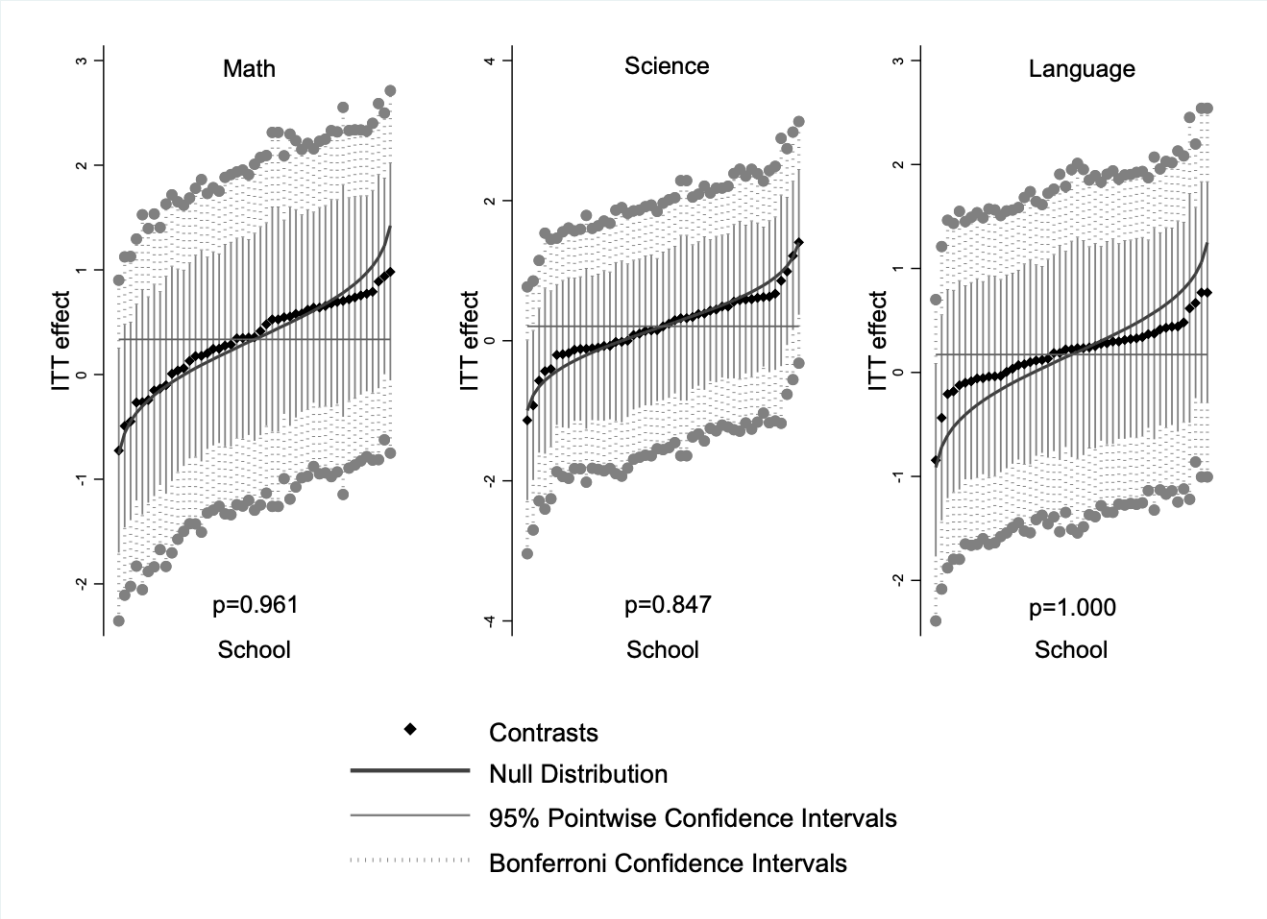
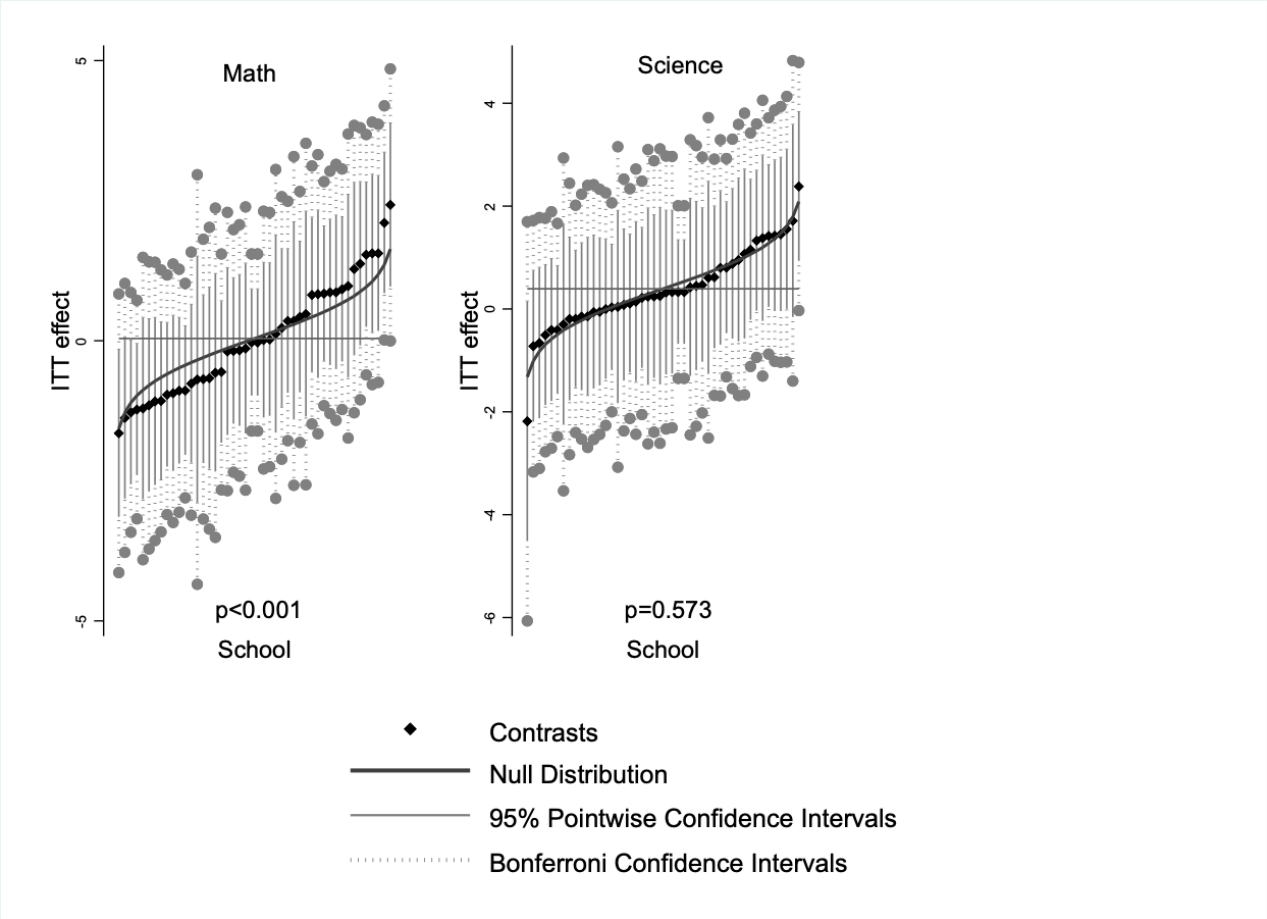
Figure 1: Heterogeneous impact on standardized test scores by school (endline)



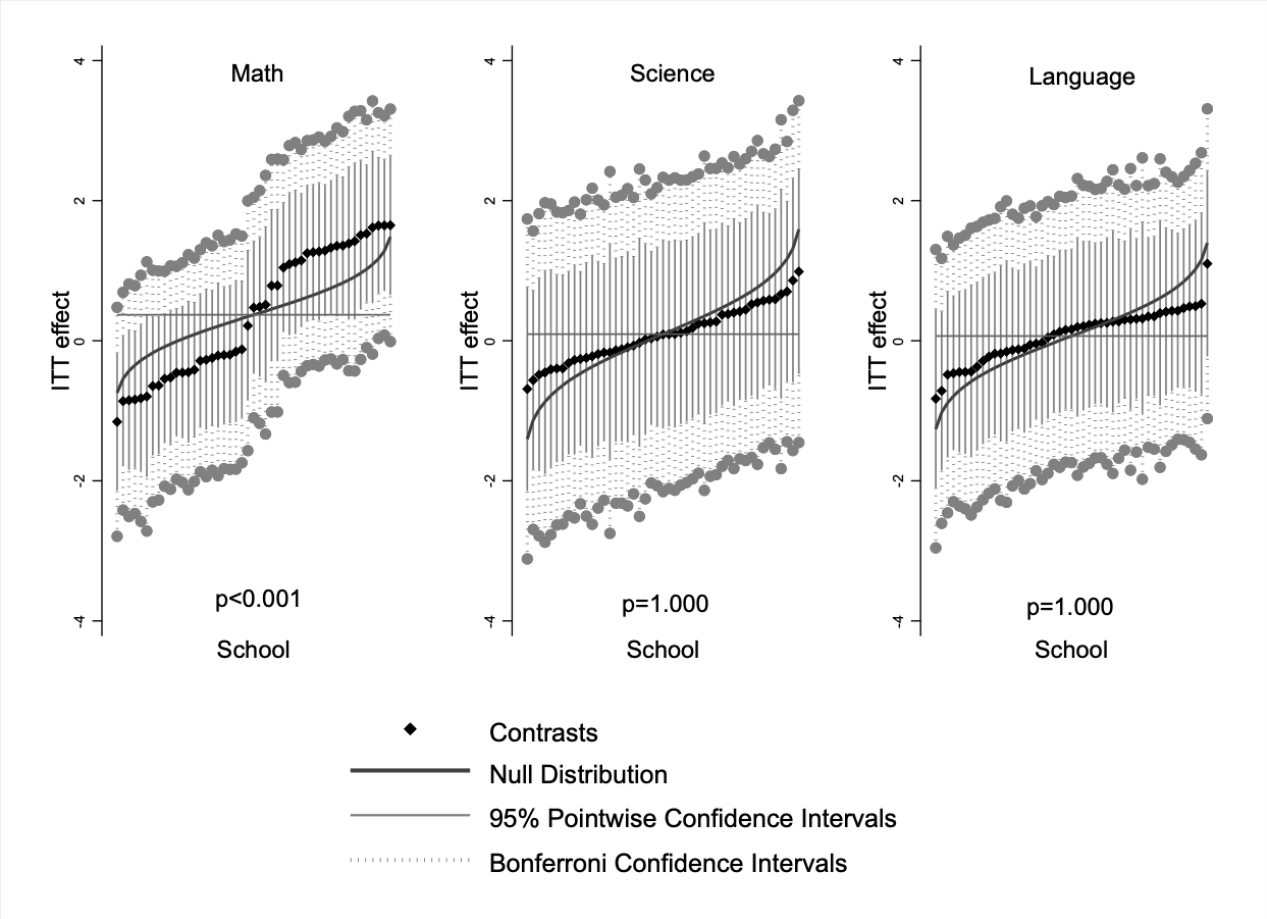
Notes: The figure provides a "caterpillar plot" (von Hippel and Bellows, 2018) of impacts on standardized scores by school at endline, about nine months after the rollout of the intervention (April 2018). Each black dot refers to the point estimate for a given school. Bonferroni confidence intervals adjust standard errors for multiple hypothesis testing. The black solid line shows the null distribution of "effects" that can be expected due to error. The p-values are for a test of the null hypothesis of no heterogeneity. Estimates come from regressions of test scores on interactions between a treatment indicator and indicators for each school with controls for randomization-strata (i.e., grade) fixed effects, accounting for baseline performance.

Figure 2: Heterogeneous impact on standardized test scores by school (endline audit)



Notes: The figure provides a "caterpillar plot" (von Hippel and Bellows, 2018) of impacts on standardized scores by school at the endline "audit," about nine months after the rollout of the intervention (April 2018). Each black dot refers to the point estimate for a given school. Bonferroni confidence intervals adjust standard errors for multiple hypothesis testing. The black solid line shows the null distribution of "effects" that can be expected due to error. The p-values are for a test of the null hypothesis of no heterogeneity. Estimates come from regressions of test scores on interactions between a treatment indicator and indicators for each school with controls for randomization-strata (i.e., grade) fixed effects, accounting for baseline performance.

Figure 3: Heterogeneous impact on standardized test scores by school (follow-up)



Notes: The figure provides a "caterpillar plot" (von Hippel and Bellows, 2018) of impacts on standardized scores by school at follow-up, about 11 months after the rollout of the intervention (June 2018). Each black dot refers to the point estimate for a given school. Bonferroni confidence intervals adjust standard errors for multiple hypothesis testing. The black solid line shows the null distribution of "effects" that can be expected due to error. The p-values are for a test of the null hypothesis of no heterogeneity. Estimates come from regressions of test scores on interactions between a treatment indicator and indicators for each school with controls for randomization-strata (i.e., grade) fixed effects, accounting for baseline performance.

Table 1: Differences between student characteristics (baseline)

	(1) Control	(2) Treatment	(3) Col. (2)-(1)	(4) N
Female	0.506	0.542	0.036	2,581
	[0.500]	[0.498]	(0.031)	
Age	10.781	10.852	0.055	2,404
	[1.011]	[1.038]	(0.048)	
Speaks Marathi at home	0.690	0.690	-0.000	2,581
	[0.463]	[0.463]	(0.023)	
Speaks Hindi at home	0.169	0.164	-0.005	2,581
	[0.375]	[0.370]	(0.018)	
Speaks English at home	0.011	0.006	-0.004	2,581
	[0.102]	[0.080]	(0.005)	
Mother completed primary school	0.634	0.600	-0.034	2,657
	[0.482]	[0.490]	(0.023)	
Father completed primary school	0.768	0.778	0.009	2,657
	[0.422]	[0.416]	(0.019)	
Student has a desk to study	0.279	0.257	-0.022	2,581
	[0.449]	[0.437]	(0.029)	
Student has own room	0.172	0.204	0.032	2,581
	[0.378]	[0.403]	(0.030)	
Student has a computer	0.195	0.176	-0.019	2,579
	[0.396]	[0.381]	(0.025)	
Student has Internet	0.425	0.373	-0.053	2,579
	[0.495]	[0.484]	(0.045)	
Student has a TV	0.901	0.894	-0.006	2,581
	[0.299]	[0.307]	(0.017)	
Attends tuition in math	0.347	0.315	-0.032	2,288
	[0.476]	[0.465]	(0.036)	
Attends tuition in science	0.260	0.228	-0.032	2,267
	[0.439]	[0.420]	(0.042)	
Attends tuition in Marathi or Hindi	0.368	0.356	-0.012	2,293
	[0.482]	[0.479]	(0.027)	
Attends tuition in English	0.327	0.305	-0.022	2,353
	[0.469]	[0.461]	(0.034)	
Math (standardized score)	0.000	-0.131	-0.131*	2,657
	[1.000]	[0.955]	(0.068)	
Science (standardized score)	-0.000	-0.050	-0.050	2,657
	[1.000]	[1.011]	(0.066)	
Language (standardized score)	0.000	-0.066	-0.065	2,657
	[1.000]	[1.016]	(0.060)	
Raven's matrices (standardized score)	0.000	0.045	0.045	2,657
	[1.000]	[0.984]	(0.055)	

Notes: This table compares the students in the control and treatment groups at baseline. It shows the means and standard deviations for each group (columns 1-2) and tests for differences between groups including randomization-strata (i.e., grade) fixed effects (column 3). The sample includes all students observed at baseline. Standard deviations appear in brackets, and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2: Differences between teacher characteristics (endline)

	(1) PMC teachers	(2) SEI fellows	(3) Col. (2)-(1)	(4) N
Female	0.761	0.633	-0.128	141
	[0.429]	[0.487]	(0.089)	
Age	41.707	20.816	-20.868***	141
	[8.129]	[1.811]	(0.932)	
Years of teaching experience (total)	18.272	1.184	-17.050***	141
	[8.418]	[0.486]	(0.947)	
Years of teaching experience (at this school)	5.141	1.000	-4.132***	141
	[4.424]	[0.000]	(0.448)	
Years of math teaching experience (at this school)	2.011	1.000	-1.005***	141
	[2.141]	[0.000]	(0.200)	
Years of science teaching experience (at this school)	2.000	1.000	-0.995***	141
	[2.138]	[0.000]	(0.199)	
Has bachelor's degree or higher	0.837	0.347	-0.490***	141
	[0.371]	[0.481]	(0.080)	
Also teaches English	0.978	0.000	-0.978***	141
	[0.147]	[0.000]	(0.015)	
Also teaches Indian languages	0.946	0.000	-0.946***	141
	[0.228]	[0.000]	(0.023)	
Teaches in English	0.050	0.040	-0.010	150
	[0.219]	[0.198]	(0.010)	
Teaches in Hindi	0.110	0.160	0.050*	150
	[0.314]	[0.370]	(0.026)	
Teaches in Marathi	0.840	0.800	-0.040	150
	[0.368]	[0.404]	(0.025)	
Teacher assessment (proportion-correct score)	0.632	0.771	0.139***	139
	[0.097]	[0.077]	(0.016)	
Instructional practices (proportion-correct score)	0.294	0.493	0.199***	139
	[0.171]	[0.163]	(0.034)	
Student errors (proportion-correct score)	0.523	0.689	0.166***	139
	[0.161]	[0.165]	(0.028)	
Content knowledge (proportion-correct score)	0.833	0.931	0.098***	139
	[0.120]	[0.071]	(0.018)	

Notes: This table compares Pune Municipal Corporation (PMC) teachers and Science Education Initiative (SEI) fellows at endline. It shows the means and standard deviations for each group (columns 1-2) and tests for differences between groups including randomization-strata (i.e., grade) fixed effects (column 3). The sample includes all teachers observed at baseline. Standard deviations appear in brackets, and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Timeline of the study

(1)	(2)	(3)	(4)	(5)	(6)
			Student participation rates		
Month	Event	Grades	Total classrooms	Control classrooms	Treatment classrooms
A. 2017-2018 school year					
June	School year starts				
July	Student assessments	5, 6	100%	100%	100%
	Student surveys	5, 6	100%	100%	100%
December	Announced classroom observations	5, 6	99%	100%	98%
February	Unannounced school visits	5, 6	99%	100%	98%
	Administrative data on student attendance	5, 6	100%	100%	100%
March-	Student assessments	5, 6			
April	– Math and science		88%	87%	89%
	– Language		86%	85%	87%
	Student surveys	5, 6	80%	79%	81%
	Student "audit" assessments of math and science	5, 6	21%	22%	22%
April-	Teacher surveys	5, 6	95%	93%	98%
May	Teacher assessments	5, 6	94%	92%	98%
B. 2018-2019 school year					
June	School year starts				
June-	Student assessments	6, 7			
August	– Math		81%	81%	81%
	– Science		80%	80%	81%
	– Language		81%	80%	81%
	Administrative data on student attendance	6, 7			

Notes: The table shows the timeline for the interventions and rounds of data collection for the study, including the month in which each event occurred (column 1), a brief description of the event (column 2), the target grades (column 3), and the percentage of students that participated in each event by experimental group (columns 4-6). The student "audit" assessments only targeted 25% of the study sample.

Table 4: Impact on attendance and punctuality (unannounced school visits)

	(1)	(2)	(3)	(4)	(5)
	PMC teachers		Impact on PMC teachers	SEI fellows	Difference between PMC teachers and SEI fellows
	Control	Treatment	Col. (2)-(1)	Treatment	Col. (4)-(1)
A. Attendance and punctuality					
Share of instructors who...					
...attended today	0.840	0.878	0.038	0.720	-0.120
	[0.370]	[0.331]	(0.078)	[0.454]	(0.089)
...arrived on time today	0.740	0.714	-0.026	0.440	-0.300***
	[0.443]	[0.456]	(0.087)	[0.501]	(0.084)
N (instructors)	50	49	99	50	100
B. Location at the school					
Share of present instructors who...					
...were in the classroom	0.048	0.047	-0.001	0.556	0.508***
	[0.216]	[0.213]	(0.031)	[0.504]	(0.094)
...were in the principal's office	0.690	0.744	0.049	0.167	-0.523***
	[0.468]	[0.441]	(0.079)	[0.378]	(0.095)
...were elsewhere in the school	0.262	0.209	-0.048	0.278	0.015
	[0.445]	[0.412]	(0.074)	[0.454]	(0.098)
N (present instructors)	42	43	85	36	78

Notes: This table compares average attendance and punctuality of Pune Municipal Corporation (PMC) teachers in control and treatment schools and of Science Education Initiative (SEI) fellows and facilitators in treatment schools, based on unannounced visits about seven months after the rollout of the intervention (February 2018). Panel A displays results for all instructors and Panel B shows results only for instructors who were present on the day of the visit. Impact estimates come from regressions of each variable on a treatment indicator and randomization-strata (i.e., grade) fixed effects. Standard deviations appear in brackets, and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Impact on allocation of instructional time (announced observations)

	(1)	(2)	(3)	(4)	(5)
	PMC teachers		Impact on PMC teachers	SEI fellows	Difference between PMC teachers and SEI fellows
	Control	Treatment	Col. (2)-(1)	Treatment	Col. (4)-(1)
A. Share of lesson					
Proportion of lesson time...					
...on task	0.778	0.080	-0.698***	0.745	-0.034
	[0.199]	[0.134]	(0.026)	[0.156]	(0.031)
...on class management	0.139	0.208	0.069*	0.159	0.021
	[0.134]	[0.264]	(0.041)	[0.126]	(0.021)
...off task	0.083	0.692	0.609***	0.096	0.013
	[0.135]	[0.342]	(0.048)	[0.112]	(0.021)
N (instructors)	96	50	146	50	146
B. Time in lesson					
Minutes of lesson time...					
...on task	23.344	9.600	-13.744***	89.388	65.992***
	[5.956]	[16.081]	(2.295)	[18.665]	(2.720)
...on class management	4.156	24.960	20.804***	19.102	14.967***
	[4.022]	[31.688]	(4.293)	[15.083]	(2.161)
...off task	2.500	83.040	80.540***	11.510	9.041***
	[4.052]	[41.060]	(5.557)	[13.407]	(1.904)
N (instructors)	96	50	146	50	146

Notes: This table compares the allocation of instructional time of Pune Municipal Corporation (PMC) teachers in control and treatment schools and of Science Education Initiative (SEI) fellows and facilitators in treatment schools, measured in announced observations about five months after the rollout of the intervention (December 2017). All lessons taught by PMC teachers lasted 30 minutes and all lessons taught by SEI fellows lasted 120 minutes. Panel A displays results expressed as a proportion of lesson time and Panel B shows results in terms of minutes per lesson. Standard deviations appear in brackets and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Impact on instructional practices (announced observations)

	(1) PMC teachers	(2) SEI fellows	(3) Difference between PMC teachers and SEI fellows
	Control	Treatment	Col. (2)-(1)
A. Negative practices			
Share of instructors who...			
...stayed in one spot during the lesson	0.188	0.060	-0.127**
	[0.392]	[0.240]	(0.060)
...sat down for a long period of time	0.042	0.020	-0.022
	[0.201]	[0.141]	(0.029)
...used phone during lesson	0.052	0.080	0.028
	[0.223]	[0.274]	(0.049)
...were upset at incorrect answers	0.062	0.180	0.117*
	[0.243]	[0.388]	(0.062)
...hit, pinched, or slapped students	0.021	0.000	-0.021
	[0.144]	[0.000]	(0.014)
...shouted or used harsh language	0.031	0.020	-0.011
	[0.175]	[0.141]	(0.027)
Composite index (std.)	-0.000	0.174	0.174
	[1.000]	[0.919]	(0.172)
N (instructors)	96	50	146
B. Positive practices			
Share of instructors who...			
...made eye contact with nearly all students	0.750	0.740	-0.010
	[0.435]	[0.443]	(0.062)
...called on nearly all students by name	0.635	0.640	0.005
	[0.484]	[0.485]	(0.084)
...called on students from all seating rows	0.688	0.780	0.093
	[0.466]	[0.418]	(0.098)
...asked both closed and open questions	0.625	0.780	0.155*
	[0.487]	[0.418]	(0.088)
...asked students to explain their answers	0.469	0.760	0.291***
	[0.502]	[0.431]	(0.095)
...corrected student answers	0.719	0.940	0.221***
	[0.452]	[0.240]	(0.070)
...allowed students to ask questions	0.385	0.580	0.195**
	[0.489]	[0.499]	(0.095)
...assigned classwork	0.635	0.740	0.105
	[0.484]	[0.443]	(0.069)
...helped individual students	0.677	0.960	0.283***
	[0.470]	[0.198]	(0.064)
...summarized lesson at the end	0.271	0.360	0.089
	[0.447]	[0.485]	(0.091)
...assigned homework	0.479	0.640	0.161*
	[0.502]	[0.485]	(0.092)
...praise or encouraged students	0.760	0.960	0.200***
	[0.429]	[0.198]	(0.058)
...smiled, joked, or laughed	0.167	0.280	0.113
	[0.375]	[0.454]	(0.090)
Composite index (std.)	-0.000	0.730	0.730***
	[1.000]	[0.687]	(0.175)
N (instructors)	96	50	146

Notes: This table compares the instructional practices of Pune Municipal Corporation (PMC) teachers in control and treatment schools and of Science Education Initiative (SEI) fellows and facilitators in treatment schools, measured in announced observations about five months after the rollout of the intervention (December 2017). All lessons taught by PMC teachers lasted 30 minutes and all lessons taught by SEI fellows lasted 120 minutes. Panel A displays results expressed as a proportion of class time and Panel B shows results in terms of minutes per lesson. Standard deviations appear in brackets and standard errors (clustered by school) appear in parentheses. The composite indexes are the first principal components of the variables in each panel, standardized with respect to the control group. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Impact on standardized test scores (endline, endline audit, and follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	0.247***	0.340***	0.207***	0.216***	0.132**	0.151***
	(0.048)	(0.048)	(0.051)	(0.049)	(0.052)	(0.039)
Baseline score		0.646***		0.485***		0.555***
		(0.022)		(0.030)		(0.026)
N (students)	2524	2307	2524	2307	2524	2307
B. Endline audit						
Treatment	0.046	0.091	0.367***	0.399***		
	(0.106)	(0.104)	(0.116)	(0.113)		
Baseline score		0.139**		0.203***		
		(0.061)		(0.061)		
N (students)	553	551	553	551		
C. Follow-up						
Treatment	0.283***	0.356***	0.120**	0.142**	0.059	0.081**
	(0.051)	(0.046)	(0.046)	(0.053)	(0.042)	(0.037)
Baseline score		0.651***		0.321***		0.446***
		(0.028)		(0.028)		(0.029)
N (students)	2369	2023	2369	2023	2369	2023

Notes: This table compares students' standardized scores in the control and treatment groups based on assessments administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018) and at follow-up, about 11 months after the rollout of the intervention (June 2018). Estimates come from regressions of test scores on a treatment indicator with controls for randomization-strata (i.e., grade) fixed effects, with and without accounting for baseline performance. Scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

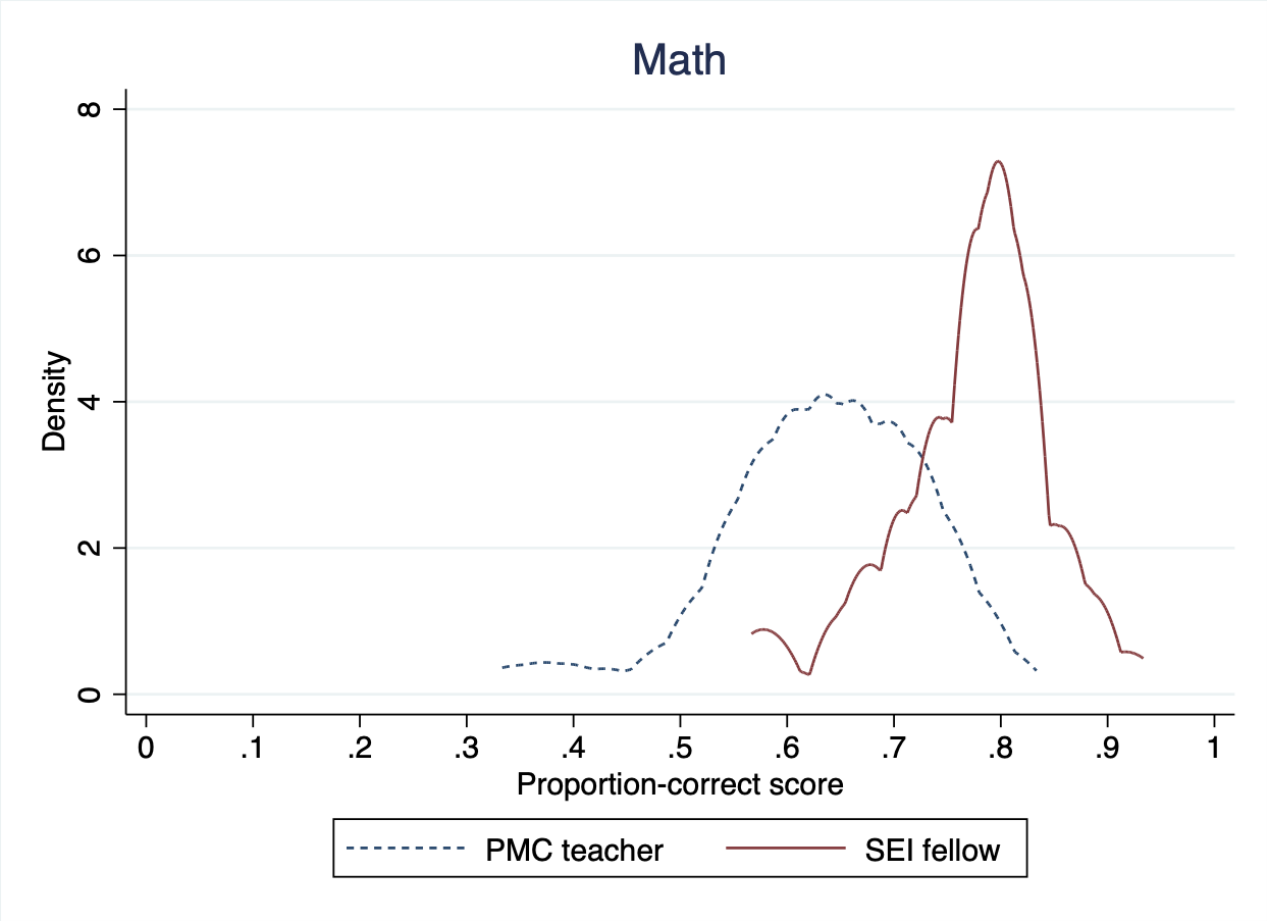
Table 8: Impact on proportion-correct repeated and non-repeated items (endline)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. All items						
Treatment	0.049***	0.067***	0.036***	0.038***	0.031**	0.035***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.012)	(0.009)
Baseline score		0.584***		0.509***		0.520***
		(0.019)		(0.031)		(0.024)
Control mean						
N (students)	2524	2307	2524	2307	2524	2307
B. Repeated items						
Treatment	0.071***	0.094***	0.054***	0.058***	0.031**	0.036***
	(0.012)	(0.012)	(0.011)	(0.011)	(0.013)	(0.010)
Baseline score		0.749***		0.619***		0.522***
		(0.024)		(0.036)		(0.027)
Control mean	0.542		0.462		0.539	
N (students)	2524	2307	2524	2307	2524	2307
C. Non-repeated items						
Treatment	0.028***	0.041***	0.018**	0.017**	0.030**	0.034***
	(0.008)	(0.008)	(0.009)	(0.008)	(0.013)	(0.010)
Baseline score		0.428***		0.393***		0.517***
		(0.018)		(0.030)		(0.024)
Control mean	0.382		0.388		0.512	
N (students)	2524	2307	2524	2307	2524	2307
P-value of the difference	0.000	0.000	0.933	0.000	0.000	0.862

Notes: This table compares students' proportion-correct scores on items in both the baseline and endline assessments ("repeated") and on items only in the endline assessments ("non-repeated") across the control and treatment groups, about nine months after the rollout of the intervention (April 2018). Estimates come from regressions of test scores on a treatment indicator with controls for randomization-strata (i.e., grade) fixed effects, with and without accounting for baseline performance. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. As Table 7 indicates, the mean standardized effects on all items without accounting for baseline performance are 0.247 SDs ($p < 0.01$) in math, 0.207 SDs ($p < 0.01$) in science, and 0.132 ($p < 0.05$) in language. The corresponding effects for repeated items are 0.275 SDs ($p < 0.01$) in math, 0.251 SDs ($p < 0.01$) in science, and 0.125 SDs ($p < 0.05$) in language. The corresponding effects for non-repeated items are 0.169 SDs ($p < 0.01$) in math, 0.104 SDs ($p < 0.05$) in science, and 0.124 SDs ($p < 0.05$) in language.

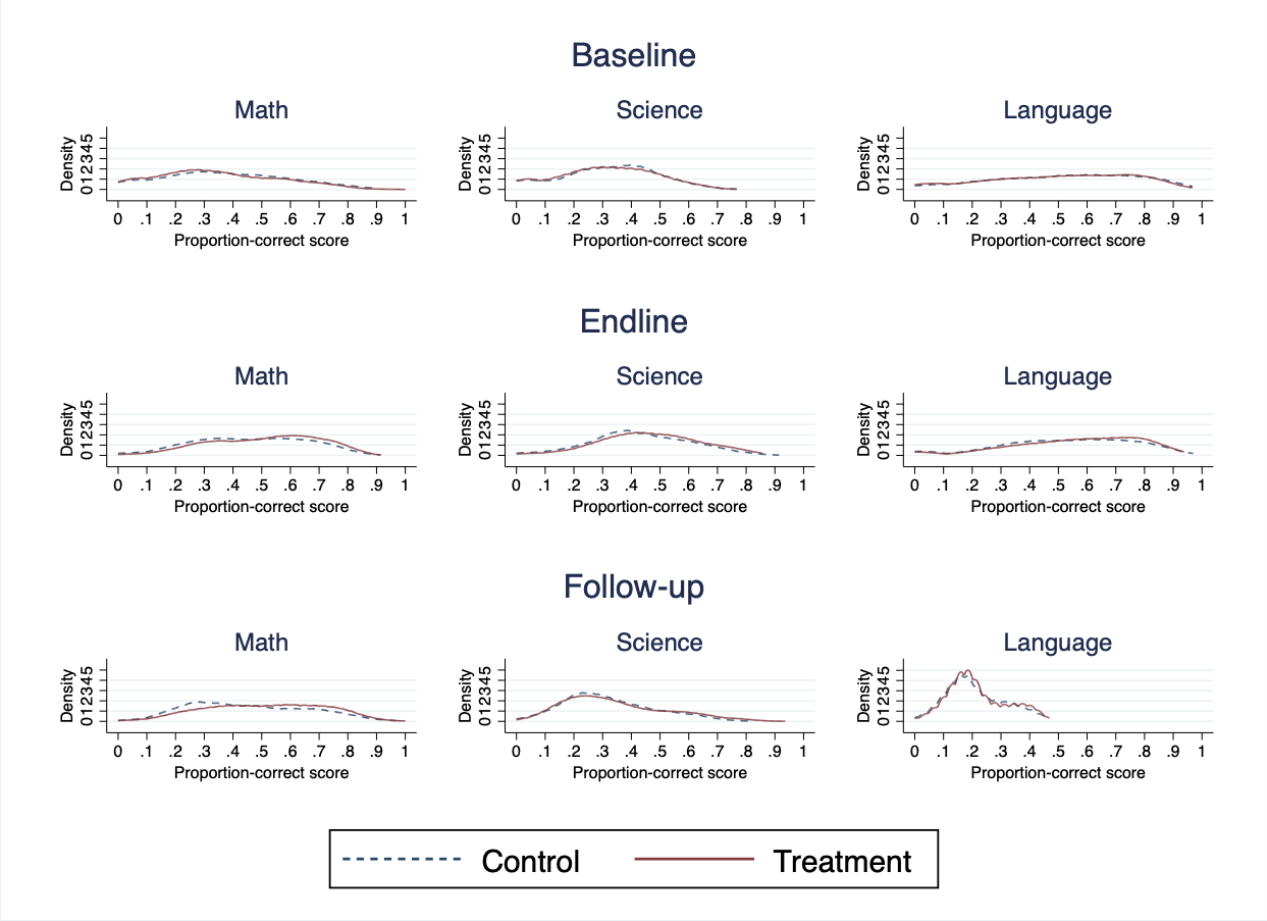
Appendix A Additional figures and tables

Figure A.1: Distributions of proportion-correct scores on teacher assessments



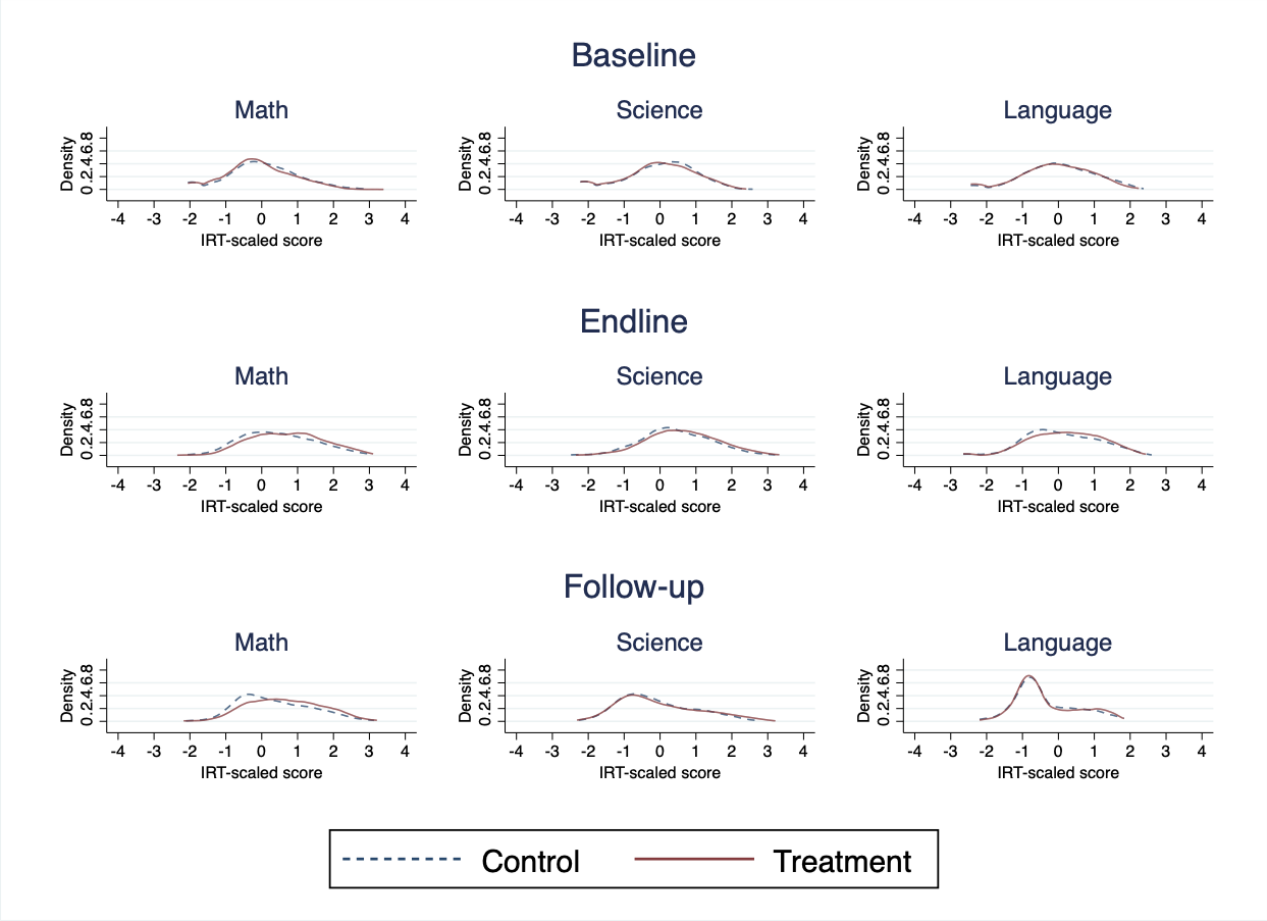
Notes: The figure shows the distribution of proportion-scaled scores on the teacher assessments. Proportion-correct scores indicate the proportion of items on each test answered correctly.

Figure A.2: Distributions of proportion-correct scores on student assessments



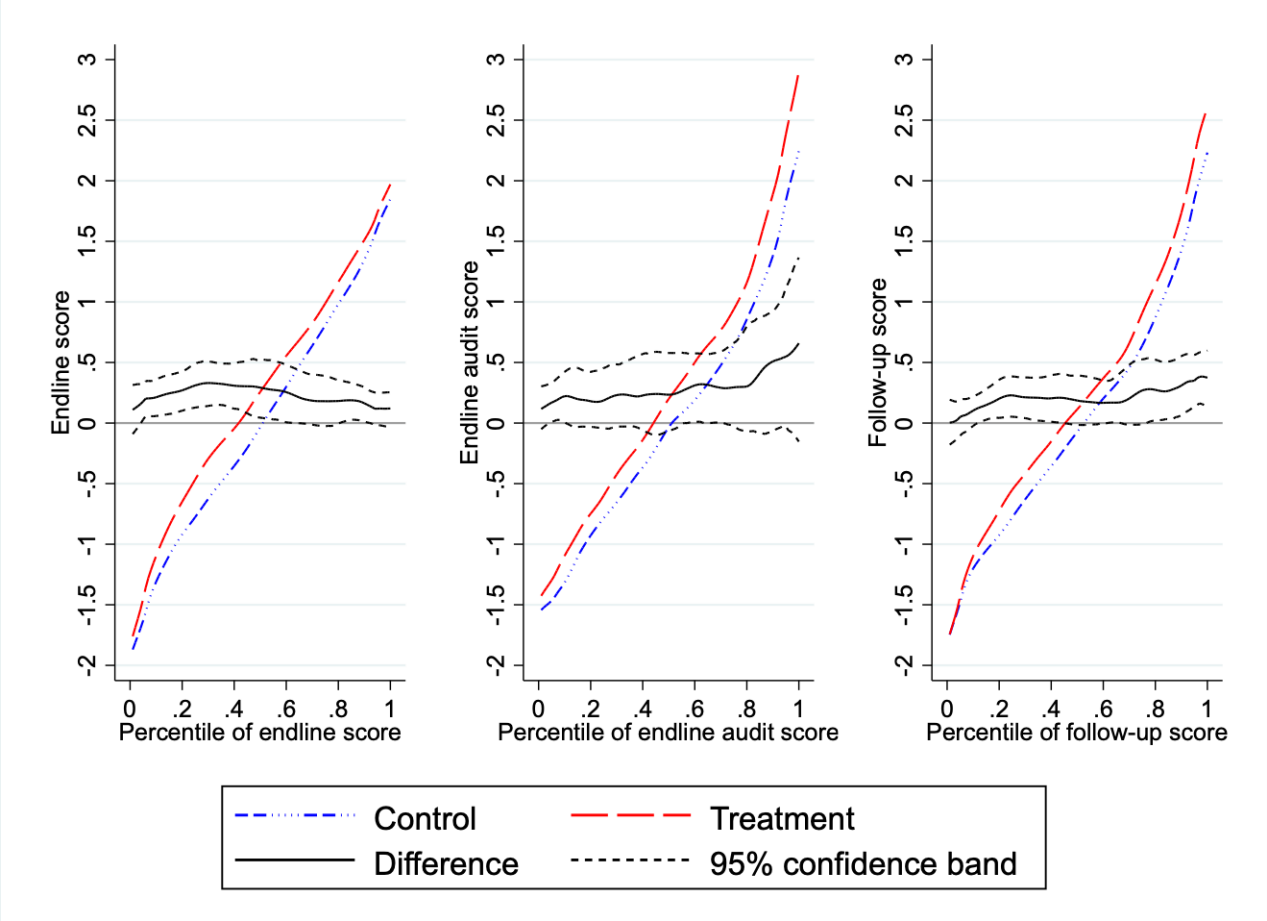
Notes: The figure shows the distribution of proportion-scaled scores on the student assessments. Proportion-correct scores indicate the proportion of items on each test answered correctly.

Figure A.3: Distributions of IRT-scaled scores on student assessments



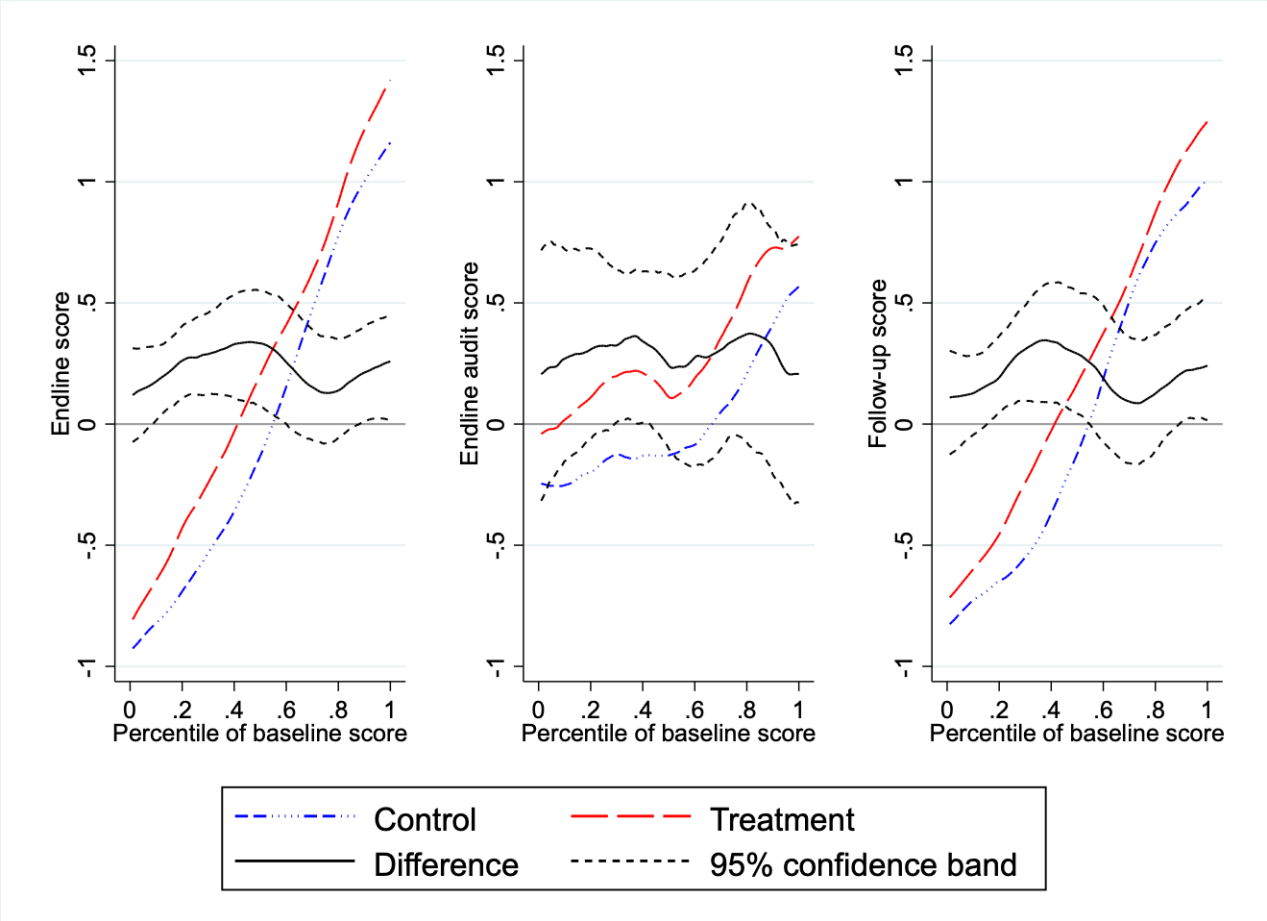
Notes: The figure shows the distribution of IRT-scaled scores on the student assessments. IRT-scaled scores are expressed in standard deviations.

Figure A.4: Quantile treatment effects (endline, endline audit, and follow-up)



Notes: The figure shows quantiles of endline, endline audit, and follow-up composite scores (the first principal component of a principal component analysis of all subjects assessed at each round) for treatment and control students who participated in the baseline and each of these rounds, estimated by polynomial regressions of each round's scores on that round's percentiles separately by experimental group. Dashed black lines display bootstrapped 95% confidence intervals.

Figure A.5: Average treatment effects by baseline score (endline, endline audit, and follow-up)



Notes: The figure shows estimates of average endline, endline audit, and follow-up composite scores (the first principal component of a principal component analysis of all subjects assessed at each round) and treatment effects at each percentile of baseline composite score for treatment and control students who participated in the baseline and each of these rounds, estimated by polynomial regression. Dashed black lines display bootstrapped 95% confidence intervals.

Table A.1: Script adherence among SEI fellows (announced observations)

	(1) All fellows	(2) High-scoring	(3) Low-scoring
Share of SEI fellows who...			
...wrote on blackboard as indicated in the script	0.818	0.800	0.842
	[0.390]	[0.408]	[0.375]
...asked students questions in the script	0.750	0.800	0.684
	[0.438]	[0.408]	[0.478]
...used materials as indicated in the script	0.523	0.480	0.579
	[0.505]	[0.510]	[0.507]
...assigned students activities in the script	0.500	0.600	0.368
	[0.506]	[0.500]	[0.496]
...changed/rephrased parts of the script	0.341	0.280	0.421
	[0.479]	[0.458]	[0.507]
...added to or expanded on parts of the script	0.341	0.240	0.474
	[0.479]	[0.436]	[0.513]
...excluded parts of the script	0.182	0.160	0.211
	[0.390]	[0.374]	[0.419]
N (fellows)	50	28	22

Notes: This table displays the prevalence of script adherence among Science Education Initiative (SEI) fellows in treatment grades, based on announced observations about five months after the rollout of the intervention (December 2017). Standard deviations appear in brackets. High-scoring fellows are those who scored above the fellow-specific median on a test of content knowledge, instructional practices, and understanding of students' misconceptions (described in section 3); low-scoring fellows are those below that median.

Table A.2: Impact on allocation of time on task (announced observations)

	(1)	(2)	(3)	(4)	(5)
	PMC teachers		Impact on PMC teachers	SEI fellows	Difference between PMC teachers and SEI fellows
	Control	Treatment	Col. (2)-(1)	Treatment	Col. (4)-(1)
A. Share of lesson					
Proportion of lesson time...					
...reading aloud	0.049	0.000	-0.049***	0.014	-0.035**
	[0.122]	[0.000]	(0.013)	[0.041]	(0.014)
...on explanation/lecture	0.343	0.020	-0.323***	0.337	-0.007
	[0.208]	[0.061]	(0.022)	[0.191]	(0.030)
...on interactive demo	0.036	0.000	-0.036***	0.008	-0.028***
	[0.070]	[0.000]	(0.007)	[0.028]	(0.009)
...on question and answers	0.181	0.002	-0.179***	0.153	-0.028
	[0.153]	[0.014]	(0.016)	[0.126]	(0.021)
...on practice and drill	0.007	0.002	-0.005*	0.004	-0.003
	[0.030]	[0.014]	(0.003)	[0.020]	(0.005)
...on classwork	0.129	0.002	-0.127***	0.157	0.028
	[0.147]	[0.014]	(0.016)	[0.117]	(0.021)
...copying	0.032	0.000	-0.032***	0.057	0.025**
	[0.067]	[0.000]	(0.007)	[0.076]	(0.012)
N (instructors)	96	50	146	50	146
B. Time in lesson					
Minutes of lesson time...					
...reading aloud	1.469	0.000	-1.469***	1.714	0.245
	[3.668]	[0.000]	(0.395)	[4.899]	(0.765)
...on explanation/lecture	10.281	2.400	-7.881***	40.408	30.067***
	[6.228]	[7.273]	(1.213)	[22.938]	(3.194)
...on interactive demo	1.094	0.000	-1.094***	0.980	-0.116
	[2.093]	[0.000]	(0.218)	[3.320]	(0.544)
...on question and answers	5.438	0.240	-5.198***	18.367	12.943***
	[4.592]	[1.697]	(0.511)	[15.120]	(2.009)
...on practice and drill	0.219	0.240	0.021	0.490	0.273
	[0.897]	[1.697]	(0.199)	[2.399]	(0.364)
...on classwork	3.875	0.240	-3.635***	18.857	14.969***
	[4.416]	[1.697]	(0.525)	[14.071]	(1.971)
...copying	0.969	0.000	-0.969***	6.857	5.892***
	[2.018]	[0.000]	(0.209)	[9.165]	(1.270)
N (instructors)	96	50	146	50	146

Notes: This table compares the allocation of time on task of Pune Municipal Corporation (PMC) teachers in control and treatment schools and of Science Education Initiative (SEI) fellows and facilitators in treatment schools, measured in announced observations about five months after the rollout of the intervention (December 2017). All lessons taught by PMC teachers lasted 30 minutes and all lessons taught by SEI fellows lasted 120 minutes. Panel A displays results expressed as a proportion of lesson time and Panel B shows results in terms of minutes per lesson. Standard deviations appear in brackets and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.3: Impact on allocation of time on classroom management (announced observations)

	(1)	(2)	(3)	(4)	(5)
	PMC teachers		Impact on PMC teachers	SEI fellows	Difference between PMC teachers and SEI fellows
	Control	Treatment	Col. (2)-(1)	Treatment	Col. (4)-(1)
A. Share of lesson					
Proportion of lesson time...					
...on instructions	0.011	0.006	-0.005	0.043	0.031***
	[0.035]	[0.024]	(0.005)	[0.076]	(0.010)
...on discipline	0.015	0.010	-0.005	0.016	0.002
	[0.038]	[0.036]	(0.006)	[0.051]	(0.008)
...on class mgmt. w/students	0.057	0.008	-0.049***	0.082	0.024*
	[0.084]	[0.027]	(0.011)	[0.075]	(0.014)
...on class mgmt. alone	0.055	0.184	0.129***	0.018	-0.037***
	[0.099]	[0.245]	(0.035)	[0.049]	(0.013)
N (instructors)	96	50	146	50	146
B. Time in lesson					
Minutes of lesson time...					
...on instructions	0.344	0.720	0.376	5.143	4.799***
	[1.055]	[2.879]	(0.410)	[9.165]	(1.262)
...on discipline	0.438	1.200	0.762	1.959	1.523*
	[1.150]	[4.371]	(0.595)	[6.171]	(0.874)
...on class mgmt. w/students	1.719	0.960	-0.759	9.796	8.088***
	[2.529]	[3.289]	(0.582)	[9.058]	(1.312)
...on class mgmt. alone	1.656	22.080	20.424***	2.204	0.558
	[2.980]	[29.430]	(3.930)	[5.834]	(0.853)
N (instructors)	96	50	146	50	146

Notes: This table compares the allocation of time on classroom management of Pune Municipal Corporation (PMC) teachers in control and treatment schools and of Science Education Initiative (SEI) fellows and facilitators in treatment schools, measured in announced observations about five months after the rollout of the intervention (December 2017). All lessons taught by PMC teachers lasted 30 minutes and all lessons taught by SEI fellows lasted 120 minutes. Panel A displays results expressed as a proportion of class time and Panel B shows results in terms of minutes per lesson. Standard deviations appear in brackets and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.4: Impact on allocation of time off task (announced observations)

	(1)	(2)	(3)	(4)	(5)
	PMC teachers		Impact on PMC teachers	SEI fellows	Difference between PMC teachers and SEI fellows
	Control	Treatment	Col. (2)-(1)	Treatment	Col. (4)-(1)
A. Share of lesson					
Proportion of lesson time...					
...on social interaction w/students	0.007	0.002	-0.005	0.002	-0.005
	[0.030]	[0.014]	(0.004)	[0.014]	(0.004)
...on social interaction w/adults	0.015	0.016	0.001	0.006	-0.008
	[0.046]	[0.037]	(0.008)	[0.024]	(0.006)
...uninvolved	0.038	0.102	0.064*	0.018	-0.019
	[0.094]	[0.235]	(0.034)	[0.049]	(0.014)
...out of room	0.024	0.572	0.548***	0.069	0.046***
	[0.086]	[0.395]	(0.053)	[0.102]	(0.016)
N (instructors)	96	50	146	50	146
B. Time in lesson					
Minutes of lesson time...					
...on social interaction w/students	0.219	0.240	0.021	0.245	0.030
	[0.897]	[1.697]	(0.263)	[1.714]	(0.261)
...on social interaction w/adults	0.438	1.920	1.482**	0.735	0.298
	[1.375]	[4.444]	(0.660)	[2.907]	(0.419)
...uninvolved	1.125	12.240	11.115***	2.204	1.084
	[2.829]	[28.220]	(3.925)	[5.834]	(0.927)
...out of the room	0.719	68.640	67.921***	8.327	7.628***
	[2.566]	[47.448]	(6.274)	[12.297]	(1.717)
N (instructors)	96	50	146	50	146

Notes: This table compares the allocation of time off task of Pune Municipal Corporation (PMC) teachers in control and treatment schools and of Science Education Initiative (SEI) fellows and facilitators in treatment schools, measured in announced observations about five months after the rollout of the intervention (December 2017). All lessons taught by PMC teachers lasted 30 minutes and all lessons taught by SEI fellows lasted 120 minutes. Panel A displays results expressed as a proportion of class time and Panel B shows results in terms of minutes per lesson. Standard deviations appear in brackets and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.5: Impact on allocation of time engaging students (announced observations)

	(1)	(2)	(3)	(4)	(5)
	PMC teachers		Impact on PMC teachers	SEI fellows	Difference between PMC teachers and SEI fellows
	Control	Treatment	Col. (2)-(1)	Treatment	Col. (4)-(1)
A. Share of lesson					
Share of lesson time engaging...					
...no students	0.091	0.746	0.655***	0.106	0.016
	[0.143]	[0.296]	(0.042)	[0.121]	(0.022)
...one student	0.119	0.014	-0.105***	0.129	0.010
	[0.127]	[0.040]	(0.015)	[0.122]	(0.023)
...a small group of students	0.044	0.000	-0.044***	0.045	0.001
	[0.084]	[0.000]	(0.009)	[0.074]	(0.014)
...a large group of students	0.232	0.016	-0.216***	0.298	0.066
	[0.199]	[0.042]	(0.024)	[0.230]	(0.045)
...all students	0.451	0.024	-0.427***	0.404	-0.048
	[0.248]	[0.059]	(0.031)	[0.230]	(0.041)
N (instructors)	96	50	146	50	146
B. Time in lesson					
Number of minutes engaging...					
...no students	2.719	89.520	86.801***	12.735	10.049***
	[4.289]	[35.566]	(4.775)	[14.576]	(2.024)
...one student	3.562	1.680	-1.883**	15.429	11.850***
	[3.803]	[4.855]	(0.765)	[14.697]	(2.142)
...a small group of students	1.312	0.000	-1.312***	5.388	4.070***
	[2.531]	[0.000]	(0.272)	[8.853]	(1.291)
...a large group of students	6.969	1.920	-5.049***	35.755	28.789***
	[5.976]	[5.062]	(0.952)	[27.549]	(4.302)
...all students	13.531	2.880	-10.651***	48.490	34.938***
	[7.425]	[7.093]	(1.472)	[27.600]	(4.203)
N (instructors)	96	50	146	50	146

Notes: This table compares the allocation of time engaging students of Pune Municipal Corporation (PMC) teachers in control and treatment schools and of Science Education Initiative (SEI) fellows and facilitators in treatment schools, measured in announced observations about five months after the rollout of the intervention (December 2017). All lessons taught by PMC teachers lasted 30 minutes and all lessons taught by SEI fellows lasted 120 minutes. Panel A displays results expressed as a proportion of class time and Panel B shows results in terms of minutes per lesson. Standard deviations appear in brackets and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.6: Impact on IRT-scaled test scores (endline, endline audit, and follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	0.266***	0.347***	0.192***	0.192***	0.115**	0.159***
	(0.047)	(0.043)	(0.053)	(0.048)	(0.048)	(0.035)
Baseline score		0.703***		0.508***		0.598***
		(0.024)		(0.031)		(0.026)
N (students)	2504	2288	2497	2284	2432	2231
B. Endline audit						
Treatment	0.098	0.134	0.222***	0.236***		
	(0.085)	(0.085)	(0.067)	(0.063)		
Baseline score		0.140***		0.119***		
		(0.043)		(0.032)		
N (students)	553	551	553	551		
C. Follow-up						
Treatment	0.293***	0.353***	0.089*	0.106**	0.033	0.055
	(0.055)	(0.048)	(0.044)	(0.052)	(0.038)	(0.034)
Baseline score		0.672***		0.284***		0.354***
		(0.032)		(0.028)		(0.029)
N (students)	2270	1925	2271	1926	2267	1924

Notes: This table compares students' Item Response Theory (IRT)-scaled scores in the control and treatment groups based on assessments administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018) and at follow-up, about 11 months after the rollout of the intervention (June 2018). Estimates come from regressions of test scores on a treatment indicator with controls for randomization-strata (i.e., grade) fixed effects, with and without accounting for baseline performance. Scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.7: Impact on proportion-correct test scores (endline, endline audit, and follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	0.049***	0.067***	0.036***	0.038***	0.031**	0.035***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.012)	(0.009)
Baseline score		0.584***		0.509***		0.520***
		(0.019)		(0.031)		(0.024)
N (students)	2524	2307	2524	2307	2524	2307
B. Endline audit						
Treatment	0.011	0.067***	0.092***	0.038***		
	(0.026)	(0.009)	(0.029)	(0.009)		
Baseline score		0.584***		0.509***		
		(0.019)		(0.031)		
N (students)	553	2307	553	2307		
C. Follow-up						
Treatment	0.058***	0.073***	0.019**	0.022**	0.006	0.008**
	(0.010)	(0.009)	(0.007)	(0.008)	(0.004)	(0.004)
Baseline score		0.609***		0.303***		0.186***
		(0.026)		(0.026)		(0.012)
N (students)	2369	2023	2369	2023	2369	2023

Notes: This table compares students' proportion-correct scores in the control and treatment groups based on assessments administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018) and at follow-up, about 11 months after the rollout of the intervention (June 2018). Estimates come from regressions of test scores on a treatment indicator with controls for randomization-strata (i.e., grade) fixed effects, with and without accounting for baseline performance. Scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.8: Heterogeneous impact on standardized test scores by students' baseline score (endline, endline audit, follow-up)

	Math	Science	Language
	(1)	(2)	(3)
A. Endline			
Treatment	0.339***	0.214***	0.154***
	(0.048)	(0.050)	(0.038)
Baseline score	0.627***	0.463***	0.585***
	(0.030)	(0.037)	(0.029)
Treat × Baseline score	0.040	0.044	-0.063
	(0.038)	(0.040)	(0.043)
P-value of the sum	0.000	0.000	0.000
N (students)	2307	2307	2307
B. Endline audit			
Treatment	0.091	0.399***	
	(0.104)	(0.113)	
Baseline score	0.133*	0.206**	
	(0.078)	(0.078)	
Treat × Baseline score	0.014	-0.006	
	(0.121)	(0.105)	
P-value of the sum	0.129	0.021	
N (students)	551	551	
C. Follow-up			
Treatment	0.356***	0.138**	0.081**
	(0.046)	(0.052)	(0.036)
Baseline score	0.654***	0.291***	0.448***
	(0.039)	(0.035)	(0.037)
Treat × Baseline score	-0.006	0.061	-0.004
	(0.048)	(0.050)	(0.043)
P-value of the sum	0.000	0.000	0.000
N (students)	2023	2023	2023

Notes: This table shows the impact of the intervention on assessments of math, science, and language administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018), and at follow-up, about 11 months after the rollout of the intervention (June 2018) by students' baseline score. Estimates come from regressions of endline test scores on a treatment indicator, students' baseline performance, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects. Endline scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.9: Heterogeneous impact on standardized test scores by students' socio-economic status (endline, endline audit, follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	0.261***	0.344***	0.191***	0.210***	0.113**	0.138***
	(0.051)	(0.048)	(0.052)	(0.050)	(0.052)	(0.041)
SES index	-0.030	-0.039*	-0.069*	-0.096***	-0.084**	-0.074**
	(0.033)	(0.021)	(0.037)	(0.032)	(0.037)	(0.029)
Treat × SES index	0.112**	0.056*	0.165***	0.144***	0.111*	0.057
	(0.045)	(0.032)	(0.053)	(0.045)	(0.058)	(0.046)
Baseline score		0.654***		0.502***		0.574***
		(0.023)		(0.032)		(0.026)
P-value of the sum	0.017	0.464	0.010	0.173	0.539	0.648
N (students)	2255	2255	2255	2255	2255	2255
B. Endline audit						
Treatment	0.059	0.098	0.392***	0.424***		
	(0.111)	(0.111)	(0.121)	(0.115)		
SES index	0.016	0.003	-0.082	-0.105		
	(0.080)	(0.079)	(0.067)	(0.066)		
Treat × SES index	-0.093	-0.091	0.106	0.104		
	(0.104)	(0.100)	(0.108)	(0.108)		
Baseline score		0.154**		0.224***		
		(0.064)		(0.063)		
P-value of the sum	0.210	0.117	0.751	0.985		
N (students)	532	532	532	532		
C. Follow-up						
Treatment	0.274***	0.355***	0.133***	0.152***	0.059	0.077**
	(0.052)	(0.045)	(0.049)	(0.053)	(0.048)	(0.036)
SES index	-0.001	-0.012	-0.048	-0.068**	-0.035	-0.032
	(0.033)	(0.024)	(0.030)	(0.029)	(0.032)	(0.026)
Treat × SES index	0.083*	0.036	0.138***	0.125***	0.089**	0.049
	(0.044)	(0.033)	(0.046)	(0.041)	(0.042)	(0.032)
Baseline score		0.658***		0.340***		0.451***
		(0.030)		(0.029)		(0.029)
P-value of the sum	0.031	0.405	0.013	0.057	0.139	0.555
N (students)	1972	1972	1972	1972	1972	1972

Notes: This table shows the impact of the intervention on assessments of math, science, and language administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018), and at follow-up, about 11 months after the rollout of the intervention (June 2018) by students' socio-economic status. Estimates come from regressions of endline test scores on a treatment indicator, the first principal component from a principal component analysis of students' home assets, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects, with and without accounting for students' baseline performance. Endline scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.10: Heterogeneous impact on standardized test scores by students' sex (endline, endline audit, follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	0.218**	0.267***	0.131	0.148**	0.025	0.095
	(0.083)	(0.065)	(0.080)	(0.070)	(0.074)	(0.064)
Female	-0.016	-0.004	0.064	0.053	0.161**	0.147***
	(0.067)	(0.047)	(0.087)	(0.073)	(0.067)	(0.053)
Treat × Female	0.085	0.145*	0.114	0.117	0.163*	0.080
	(0.104)	(0.081)	(0.114)	(0.105)	(0.089)	(0.088)
Baseline score		0.657***		0.501***		0.568***
		(0.022)		(0.032)		(0.028)
P-value of the sum	0.433	0.047	0.066	0.038	0.000	0.002
N (students)	2258	2258	2258	2258	2258	2258
B. Endline audit						
Treatment	-0.005	0.028	0.452***	0.487***		
	(0.136)	(0.141)	(0.124)	(0.126)		
Female	0.155	0.158	0.154	0.162		
	(0.171)	(0.167)	(0.131)	(0.124)		
Treat × Female	0.121	0.130	-0.142	-0.149		
	(0.219)	(0.221)	(0.202)	(0.199)		
Baseline score		0.155**		0.220***		
		(0.061)		(0.062)		
P-value of the sum	0.121	0.103	0.950	0.943		
N (students)	533	533	533	533		
C. Follow-up						
Treatment	0.262***	0.310***	0.096	0.104	0.052	0.114*
	(0.096)	(0.066)	(0.074)	(0.077)	(0.080)	(0.061)
Female	0.068	0.071	0.103	0.087	0.134*	0.115*
	(0.083)	(0.057)	(0.073)	(0.069)	(0.072)	(0.059)
Treat × Female	0.022	0.083	0.065	0.086	0.014	-0.066
	(0.122)	(0.081)	(0.099)	(0.103)	(0.103)	(0.086)
Baseline score		0.662***		0.340***		0.450***
		(0.030)		(0.028)		(0.029)
P-value of the sum	0.305	0.012	0.029	0.020	0.069	0.435
N (students)	1974	1974	1974	1974	1974	1974

Notes: This table shows the impact of the intervention on assessments of math, science, and language administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018), and at follow-up, about 11 months after the rollout of the intervention (June 2018) by students' sex. Estimates come from regressions of endline test scores on a treatment indicator, a female student indicator, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects, with and without accounting for students' baseline performance. Endline scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.11: Heterogeneous impact on standardized test scores by students' caste (endline, endline audit, follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	0.337***	0.389***	0.223***	0.205***	0.108	0.077
	(0.055)	(0.045)	(0.067)	(0.065)	(0.066)	(0.067)
Scheduled caste/tribe	-0.111	-0.017	-0.101	-0.071	-0.132	-0.122**
	(0.078)	(0.047)	(0.084)	(0.065)	(0.081)	(0.060)
Treat × SC/ST	-0.106	-0.092	-0.018	0.022	0.079	0.160*
	(0.088)	(0.062)	(0.101)	(0.087)	(0.091)	(0.083)
Baseline score		0.647***		0.494***		0.559***
		(0.021)		(0.027)		(0.025)
P-value of the sum	0.003	0.061	0.094	0.405	0.538	0.602
N (students)	2236	2236	2236	2236	2236	2236
B. Endline audit						
Treatment	0.206	0.232	0.517***	0.523***		
	(0.165)	(0.165)	(0.155)	(0.154)		
Scheduled caste/tribe	0.019	0.043	0.048	0.038		
	(0.139)	(0.143)	(0.102)	(0.100)		
Treat × SC/ST	-0.274	-0.276	-0.188	-0.169		
	(0.197)	(0.198)	(0.178)	(0.181)		
Baseline score		0.129**		0.174***		
		(0.064)		(0.061)		
P-value of the sum	0.042	0.063	0.369	0.411		
N (students)	537	537	537	537		
C. Follow-up						
Treatment	0.307***	0.366***	0.178**	0.158**	0.136**	0.096*
	(0.055)	(0.051)	(0.068)	(0.065)	(0.056)	(0.053)
Scheduled caste/tribe	-0.082	0.013	-0.057	-0.048	-0.001	-0.009
	(0.071)	(0.043)	(0.063)	(0.057)	(0.064)	(0.061)
Treat × SC/ST	-0.025	-0.023	-0.090	-0.041	-0.090	-0.006
	(0.086)	(0.061)	(0.093)	(0.085)	(0.086)	(0.085)
Baseline score		0.665***		0.323***		0.448***
		(0.026)		(0.029)		(0.030)
P-value of the sum	0.202	0.866	0.037	0.153	0.173	0.780
N (students)	1955	1955	1955	1955	1955	1955

Notes: This table shows the impact of the intervention on assessments of math, science, and language administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018), and at follow-up, about 11 months after the rollout of the intervention (June 2018) by students' caste. Estimates come from regressions of endline test scores on a treatment indicator, a scheduled caste/tribe indicator, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects, with and without accounting for students' baseline performance. Endline scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.12: Heterogeneous impact on standardized test scores by student-teacher sex match (endline, endline audit, follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	0.312***	0.367***	0.220***	0.242***	0.148**	0.161***
	(0.068)	(0.047)	(0.071)	(0.061)	(0.066)	(0.059)
Sex match	0.084	0.042	0.147	0.124	0.228***	0.137**
	(0.074)	(0.049)	(0.097)	(0.076)	(0.075)	(0.057)
Treat × Sex match	-0.037	0.029	0.014	-0.010	-0.022	0.003
	(0.101)	(0.087)	(0.122)	(0.109)	(0.116)	(0.105)
Baseline score		0.656***		0.505***		0.566***
		(0.021)		(0.030)		(0.028)
P-value of the sum	0.585	0.327	0.107	0.203	0.037	0.104
N (students)	2128	2128	2128	2128	2128	2128
B. Endline audit						
Treatment	-0.054	-0.016	0.466***	0.505***		
	(0.137)	(0.137)	(0.152)	(0.140)		
Sex match	-0.028	-0.032	0.146	0.165		
	(0.170)	(0.165)	(0.134)	(0.119)		
Treat × Sex match	0.344	0.346	-0.180	-0.207		
	(0.251)	(0.255)	(0.244)	(0.243)		
Baseline score		0.154**		0.230***		
		(0.065)		(0.063)		
P-value of the sum	0.145	0.145	0.872	0.846		
N (students)	506	506	506	506		
C. Follow-up						
Treatment	0.337***	0.378***	0.121**	0.128*	0.101	0.100**
	(0.081)	(0.055)	(0.059)	(0.064)	(0.062)	(0.048)
Sex match	0.159*	0.105*	0.103	0.076	0.150*	0.071
	(0.084)	(0.058)	(0.065)	(0.055)	(0.079)	(0.064)
Treat × Sex match	-0.068	0.012	0.089	0.094	-0.039	-0.012
	(0.127)	(0.093)	(0.094)	(0.093)	(0.102)	(0.079)
Baseline score		0.655***		0.342***		0.449***
		(0.030)		(0.030)		(0.028)
P-value of the sum	0.318	0.076	0.024	0.039	0.128	0.288
N (students)	1870	1870	1870	1870	1870	1870

Notes: This table shows the impact of the intervention on assessments of math, science, and language administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018), and at follow-up, about 11 months after the rollout of the intervention (June 2018) by whether the sex of the teacher matches the sex of the student. Estimates come from regressions of endline test scores on a treatment indicator, a sex match indicator, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects. Endline scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.13: Heterogeneous impact on standardized test scores by teachers' assessment score (endline, endline audit, follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	0.255**	0.449***	0.162	0.211**	0.127	0.242***
	(0.101)	(0.064)	(0.107)	(0.091)	(0.123)	(0.082)
Teacher score	0.104*	0.068	0.117**	0.093*	0.039	0.021
	(0.058)	(0.046)	(0.053)	(0.050)	(0.075)	(0.052)
Treat × Teacher score	-0.083	-0.123**	-0.064	-0.073	-0.026	-0.079
	(0.089)	(0.053)	(0.098)	(0.074)	(0.105)	(0.074)
Baseline score		0.649***		0.486***		0.557***
		(0.021)		(0.029)		(0.025)
P-value of the sum	0.723	0.119	0.444	0.687	0.873	0.395
N (students)	2374	2174	2374	2174	2374	2174
B. Endline audit						
Treatment	0.099	0.167	0.295	0.365*		
	(0.236)	(0.242)	(0.219)	(0.214)		
Teacher score	-0.178*	-0.196**	0.014	-0.011		
	(0.097)	(0.095)	(0.096)	(0.094)		
Treat × Teacher score	0.119	0.123	0.046	0.037		
	(0.196)	(0.198)	(0.190)	(0.179)		
Baseline score		0.169***		0.212***		
		(0.061)		(0.063)		
P-value of the sum	0.704	0.641	0.689	0.849		
N (students)	523	521	523	521		
C. Follow-up						
Treatment	0.233**	0.368***	0.052	0.106	0.008	0.090
	(0.114)	(0.080)	(0.078)	(0.064)	(0.083)	(0.064)
Teacher score	0.118*	0.072	0.028	0.004	0.051	-0.004
	(0.064)	(0.046)	(0.036)	(0.038)	(0.043)	(0.032)
Treat × Teacher score	-0.059	-0.070	0.028	0.024	-0.004	0.001
	(0.101)	(0.069)	(0.067)	(0.058)	(0.072)	(0.057)
Baseline score		0.649***		0.325***		0.446***
		(0.029)		(0.030)		(0.027)
P-value of the sum	0.323	0.955	0.272	0.533	0.338	0.956
N (students)	2231	1909	2231	1909	2231	1909

Notes: This table shows the impact of the intervention on assessments of math, science, and language administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018), and at follow-up, about 11 months after the rollout of the intervention (June 2018) by teachers' standardized scores on an assessment of instructional practice, knowledge of student errors, and content knowledge in math (calculated as the first principal component of a principal-component analysis of the scores on all three domains, standardized with respect to instructors in the control group). Estimates come from regressions of endline test scores on a treatment indicator, teachers' score, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects. Endline scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.14: Impact on standardized test scores among high-scoring teachers (endline, endline audit, follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	0.256***	0.390***	0.176**	0.200**	0.102	0.193***
	(0.069)	(0.060)	(0.085)	(0.079)	(0.091)	(0.054)
Baseline score		0.634***		0.460***		0.554***
		(0.032)		(0.035)		(0.028)
N (students)	1382	1276	1382	1276	1382	1276
B. Endline audit						
Treatment	0.252	0.331*	0.305*	0.337*		
	(0.175)	(0.175)	(0.176)	(0.172)		
Baseline score		0.214***		0.144**		
		(0.075)		(0.068)		
N (students)	308	307	308	307		
C. Follow-up						
Treatment	0.232**	0.371***	0.118	0.160**	0.039	0.127**
	(0.093)	(0.076)	(0.076)	(0.064)	(0.077)	(0.053)
Baseline score		0.675***		0.287***		0.440***
		(0.042)		(0.036)		(0.031)
N (students)	1312	1130	1312	1130	1312	1130

Notes: This table shows the impact of the intervention on assessments of math, science, and language administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018), and at follow-up, about 11 months after the rollout of the intervention (June 2018) among teachers who obtained a proportion-correct score between 60 and 80% on an assessment of instructional practice, knowledge of student errors, and content knowledge in math. Estimates come from regressions of test scores on a treatment indicator with controls for randomization-strata (i.e., grade) fixed effects, with and without accounting for baseline performance. Endline scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.15: Impact on student attitudes, mindsets, and aspirations (endline)

	(1) Control	(2) Treatment	(3) Difference	(4) N
A. Math and science				
Share of students who...				
...enjoy learning math	0.814	0.830	0.017	2,524
	[0.390]	[0.376]	(0.017)	
...enjoy learning science	0.740	0.773	0.033	2,524
	[0.439]	[0.419]	(0.020)	
...wish they did not have math	0.277	0.257	-0.019	2,524
	[0.448]	[0.437]	(0.026)	
...wish they did not have science	0.278	0.257	-0.020	2,524
	[0.448]	[0.437]	(0.028)	
...feel nervous about math	0.422	0.380	-0.041	2,524
	[0.494]	[0.486]	(0.027)	
...feel nervous about science	0.402	0.404	0.002	2,524
	[0.491]	[0.491]	(0.028)	
...find math useful for life	0.728	0.761	0.033*	2,524
	[0.445]	[0.427]	(0.020)	
...find science useful for life	0.721	0.725	0.005	2,524
	[0.449]	[0.446]	(0.020)	
...stop trying when math gets hard	0.234	0.262	0.029	2,524
	[0.424]	[0.440]	(0.025)	
...stop trying when science gets hard	0.193	0.234	0.041*	2,524
	[0.395]	[0.424]	(0.022)	
Composite index (std.)	-0.000	0.038	0.039	2,524
	[1.000]	[0.992]	(0.041)	
B. Intelligence				
Share of students who believe...				
...people cannot change their intelligence	0.538	0.519	-0.019	2,524
	[0.499]	[0.500]	(0.028)	
...people have a fixed amount of intelligence	0.461	0.447	-0.014	2,524
	[0.499]	[0.497]	(0.028)	
...only some people are people	0.439	0.424	-0.014	2,524
	[0.496]	[0.494]	(0.025)	
...boys are more intelligent than girls	0.385	0.365	-0.020	2,524
	[0.487]	[0.482]	(0.025)	
...boys are better at math and science	0.371	0.357	-0.014	2,524
	[0.483]	[0.479]	(0.029)	
Composite index (std.)	-0.000	-0.052	-0.050	2,524
	[1.000]	[1.015]	(0.061)	
C. Aspirations				
Share of students who...				
...want to continue studying after high school	0.675	0.696	0.021	2,246
	[0.469]	[0.460]	(0.024)	
...want to study a STEM subject in high school	0.756	0.800	0.044	2,239
	[0.430]	[0.400]	(0.027)	
...want a STEM-related job	0.333	0.334	0.000	2,231
	[0.472]	[0.472]	(0.027)	
Composite index (std.)	-0.000	0.044	0.042	2,216
	[1.000]	[0.975]	(0.052)	

Notes: This table compares students' attitudes, midnsets, and aspirations in the control and treatment groups based on surveys administered at endline about nine months after the rollout of the intervention (April 2018). It shows the means and standard deviations for each group (columns 1-2) and tests for differences between groups including randomization-strata (i.e., grade) fixed effects (column 3). Standard deviations appear in brackets, and standard errors (clustered by school) appear in parentheses. The composite indexes are the first principal components of the variables in each panel, standardized with respect to the control group. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.16: Heterogeneous impact on student beliefs by students' sex (endline)

	STEM	Intelligence	Aspirations
	(1)	(2)	(3)
A. Endline			
Treatment	0.067	-0.017	0.115
	(0.067)	(0.068)	(0.076)
Female	0.080	-0.326***	0.220***
	(0.072)	(0.081)	(0.079)
Treat × Female	-0.010	-0.022	-0.058
	(0.091)	(0.112)	(0.096)
P-value of the sum	0.211	0.000	0.014
N (students)	2258	2258	2009

Notes: This table shows the impact of the intervention on surveys administered at endline about nine months after the rollout of the intervention (April 2018), by students' sex. Estimates come from composite indexes from Table A.15 on a treatment indicator, an indicator for female students, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects. Indexes are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.17: Heterogeneous impact on student beliefs by students' caste (endline)

	STEM	Intelligence	Aspirations
	(1)	(2)	(3)
A. Endline			
Treatment	0.060	-0.115*	0.148**
	(0.063)	(0.068)	(0.065)
Scheduled caste/tribe	-0.090	-0.112**	0.042
	(0.056)	(0.048)	(0.067)
Treat × SC/ST	0.009	0.122	-0.114
	(0.081)	(0.085)	(0.094)
P-value of the sum	0.158	0.882	0.248
N (students)	2236	2236	1989

Notes: This table shows the impact of the intervention on surveys administered at endline about nine months after the rollout of the intervention (April 2018), by students' socio-economic status. Estimates come from composite indexes from Table A.15 on a treatment indicator, the first principal component from a principal component analysis of students' home assets, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects. Indexes are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.18: Heterogeneous impact on student beliefs by students' socio-economic status (endline)

	STEM	Intelligence	Aspirations
	(1)	(2)	(3)
A. Endline			
Treatment	0.066	-0.032	0.084
	(0.043)	(0.065)	(0.052)
SES index	0.007	0.040	0.038
	(0.039)	(0.032)	(0.035)
Treat × SES index	-0.014	-0.082*	0.025
	(0.048)	(0.045)	(0.048)
P-value of the sum	0.806	0.176	0.102
N (students)	2255	2255	2006

Notes: This table shows the impact of the intervention on surveys administered at endline about nine months after the rollout of the intervention (April 2018), by students' socio-economic status. Estimates come from composite indexes from Table A.15 on a treatment indicator, an indicator for the lowest quartile from a principal component analysis of students' home assets, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects. Indexes are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.19: Heterogeneous impact on student beliefs by baseline score (endline)

	STEM	Intelligence	Aspirations
	(1)	(2)	(3)
A. Endline			
Treatment	0.063	-0.050	0.094*
	(0.042)	(0.064)	(0.055)
Baseline score	0.034	-0.042*	0.111***
	(0.022)	(0.023)	(0.023)
Treat × Baseline	-0.046	0.018	0.008
	(0.028)	(0.037)	(0.027)
P-value of the sum	0.568	0.396	0.000
N (students)	2307	2307	2052

Notes: This table shows the impact of the intervention on surveys administered at endline about nine months after the rollout of the intervention (April 2018), by students' socio-economic status. Estimates come from composite indexes from Table A.15 on a treatment indicator, an indicator for the lowest quartile from a principal component analysis of students' home assets, the interaction between these two variables, and controls for randomization-strata (i.e., grade) fixed effects. Indexes are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.20: Impact on student attendance (unannounced visits, school registers, and endline)

	(1) Control	(2) Treatment	(3) Difference	(4) N
A. Unannounced visits				
Share of students observed at school	0.787	0.789	0.002	2,442
	[0.410]	[0.408]	(0.028)	
B. School registers				
Share of students marked as...				
...absent 0 days this week	0.551	0.544	-0.008	2,619
	[0.498]	[0.498]	(0.025)	
...absent 1-2 days this week	0.274	0.271	-0.003	2,619
	[0.446]	[0.445]	(0.022)	
...absent 3-4 days this week	0.074	0.083	0.009	2,619
	[0.261]	[0.276]	(0.012)	
...absent 5+ days this week	0.101	0.102	0.001	2,619
	[0.301]	[0.303]	(0.017)	
C. Endline student survey				
Share of students who reported being...				
...late 0 days this week	0.401	0.388	-0.014	2,219
	[0.490]	[0.487]	(0.030)	
...late 1-2 days this week	0.299	0.337	0.038**	2,219
	[0.458]	[0.473]	(0.018)	
...late 3-4 days this week	0.165	0.140	-0.025	2,219
	[0.372]	[0.348]	(0.020)	
...late 5+ days this week	0.135	0.135	0.001	2,219
	[0.341]	[0.342]	(0.018)	
...absent 0 days this week	0.410	0.412	0.002	2,222
	[0.492]	[0.492]	(0.031)	
...absent 1-2 days this week	0.334	0.308	-0.026	2,222
	[0.472]	[0.462]	(0.022)	
...absent 3-4 days this week	0.128	0.148	0.021	2,222
	[0.334]	[0.356]	(0.017)	
...absent 5+ days this week	0.129	0.133	0.003	2,222
	[0.335]	[0.339]	(0.017)	

Notes: This table compares the attendance of students in the control and treatment groups based on various measures collected during unannounced visits to schools about seven months after the rollout of the intervention (February 2018) and at endline two months later (April 2018). It shows the means and standard deviations for each group (columns 1-2) and tests for differences between groups including randomization-strata (i.e., grade) fixed effects (column 3). Standard deviations appear in brackets, and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.21: Impact on student demand for tuition (endline)

	(1) Control	(2) Treatment	(3) Difference	(4) N
A. Subjects				
Share of students who attend tuition...				
...in math	0.323	0.352	0.030	2,195
	[0.468]	[0.478]	(0.026)	
...in science	0.256	0.286	0.029	2,173
	[0.437]	[0.452]	(0.026)	
...in language	0.238	0.266	0.029	2,151
	[0.426]	[0.442]	(0.028)	
...in English	0.318	0.345	0.027	2,196
	[0.466]	[0.476]	(0.028)	
B. Duration				
Share of students who attend tuition...				
...less than 2 hours per week	0.036	0.038	0.002	2,237
	[0.186]	[0.192]	(0.010)	
...2-4 hours per week	0.097	0.099	0.003	2,237
	[0.296]	[0.299]	(0.015)	
...4-6 hours per week	0.048	0.046	-0.001	2,237
	[0.215]	[0.210]	(0.011)	
...more than 6 hours per week	0.216	0.252	0.036*	2,237
	[0.412]	[0.434]	(0.020)	

Notes: This table compares students' demand for private tuition in the control and treatment groups based on surveys administered at endline about nine months after the rollout of the intervention (April 2018). It shows the means and standard deviations for each group (columns 1-2) and tests for differences between groups including randomization-strata (i.e., grade) fixed effects (column 3). Standard deviations appear in brackets, and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.22: Impact on class materials (announced observations)

	(1) Control	(2) Treatment	(3) Col. (2)-(1)	(4) N
Class has blackboard	0.980	1.000	0.020	150
	[0.141]	[0.000]	(0.020)	
Class has whiteboard	0.020	0.000	-0.020	150
	[0.141]	[0.000]	(0.020)	
Class has chalk/markers	0.980	1.000	0.020	150
	[0.141]	[0.000]	(0.019)	
Class has textbook for teachers	0.820	0.300	-0.520***	150
	[0.388]	[0.461]	(0.083)	
Class has textbook for students	0.720	0.260	-0.460***	150
	[0.454]	[0.441]	(0.098)	
Class has laptop	0.060	0.040	-0.020	150
	[0.240]	[0.197]	(0.045)	
Class has digital whiteboard	0.040	0.040	0.000	150
	[0.198]	[0.197]	(0.004)	
Class has LCD projector	0.080	0.060	-0.020	150
	[0.274]	[0.239]	(0.036)	
Class has TV	0.160	0.060	-0.100	150
	[0.370]	[0.239]	(0.066)	
Class has science/math equipment	0.300	0.160	-0.140*	150
	[0.463]	[0.368]	(0.081)	
Class has maps	0.240	0.120	-0.120	150
	[0.431]	[0.327]	(0.078)	
Class has charts/poster	0.760	0.780	0.020	150
	[0.431]	[0.416]	(0.080)	
Class has toys/games	0.080	0.040	-0.040	150
	[0.274]	[0.197]	(0.040)	

Notes: This table compares the availability of materials in control and treatment classes, based on announced observations about five months after the rollout of the intervention (December 2017). Impact estimates come from regressions of each variable on a treatment indicator and randomization-strata (i.e., grade) fixed effects. Standard deviations appear in brackets, and standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.23: Impact on variability of standardized test scores (endline, endline audit, and follow-up)

	Math		Science		Language	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Endline						
Treatment	-0.001	0.014	0.015	0.030	-0.053*	-0.061*
	(0.035)	(0.034)	(0.040)	(0.038)	(0.030)	(0.033)
Baseline score		0.287**		0.330**		0.419**
		(0.122)		(0.163)		(0.207)
N (students)	93	93	93	93	93	93
B. Endline audit						
Treatment	-0.119	-0.114	0.015	0.017		
	(0.075)	(0.069)	(0.064)	(0.066)		
Baseline score		0.080		0.039		
		(0.259)		(0.240)		
N (students)	91	91	91	91		
C. Follow-up						
Treatment	0.006	0.032	0.129***	0.136***	-0.000	-0.007
	(0.039)	(0.038)	(0.035)	(0.037)	(0.035)	(0.034)
Baseline score		0.432***		0.157		0.437***
		(0.131)		(0.189)		(0.153)
N (students)	94	94	94	94	94	94

Notes: This table compares the variability in students' standardized scores in the control and treatment groups based on assessments administered at endline and endline "audit", about nine months after the rollout of the intervention (April 2018) and at follow-up, about 11 months after the rollout of the intervention (June 2018). Estimates come from regressions of the class-level standard deviation of test scores on a treatment indicator with controls for randomization-strata (i.e., grade) fixed effects, with and without accounting for the class-level standard deviation of baseline performance. Scores are standardized to have mean zero and standard deviation one in the control group. Standard errors (clustered by school) appear in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.