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Simon Burgess
University of Bristol

Shenila Rawal
Oxford Partnership for Education Research and Analysis

Eric S. Taylor
Harvard University

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Simon Burgess, University of Bristol
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This paper reports improvements in teacher job performance, as measured by student test scores, resulting from a program of (zero-) low-stakes peer evaluation. Teachers working at the same school observed and scored each other's teaching. Students in randomly-assigned treatment schools scored 0.07σ higher on math and English exams (0.09σ lower-bound on TOT). Within each treatment school, teachers were further randomly assigned to roles: observer and observee. Teachers in both roles improved, perhaps slightly more for observers. The typical treatment school completed 2-3 observations per observee teacher. Variation in observations was generated partly by randomly assigning a low and high (2∗low) dose of suggested number of observations. Benefits were quite similar across dose conditions.

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[†] Burgess: simon.burgess@bristol.ac.uk. Rawal: shenilarawal@aol.com. Taylor: eric_taylor@harvard.edu. We first thank the Education Endowment Foundation for generous financial support, the NFER for their input, as well as the Department for Education for access to the National Pupil Database. We are indebted to Julia Carey and her team for outstanding project management, and to the schools and teachers for their participation. Thanks also to Anna Vignoles, Ellen Greaves, Hans Sievertsen and seminar participants at the IZA Economics of Education Workshop, the University of Sydney, AEFP, and APPAM who provided helpful comments and questions.

Employers measure (evaluate) the job performance of their employees with the goal of improving that performance. In typical practice, job performance measures are combined with incentives; the measures determine bonuses, dismissals, promotions, etc. The combination of measures with incentives is well motivated by the principal-agent problem. Yet, the process of evaluation might improve performance even without attaching (explicit) incentives. Performance measures, and the process of being measured itself, can reveal new information about an individual's current skills or effort, or emphasize the employer's expectations, and thus motivate or direct an individual's efforts to improve.

In this paper's experiment we estimate the effects of evaluation, if any, where the potential mechanisms for those effects cannot rely on explicit incentives, but where effects can arise through learning more about own and coworker current performance. We focus on low-stakes peer evaluation among teachers. We report on an experiment in which teachers assessed and scored each other's job performance, using structured observations of teaching in the classroom, and discussed the results together. These peer evaluations were "low stakes" in the sense that there were no formal incentives or consequences attached to the scores, though there may have been informal incentives like social pressure or career concerns. The stated goal of the program was continuous improvement.

A distinctive new contribution of our experimental design is that we can separately estimate effects on the teachers being evaluated (observees) and teachers serving as peer evaluators (observers). Within each of the treatment schools, individual teachers were randomly assigned to be an observee or observer or both. Observers' performance may suffer, for example, because they reallocate effort to conducting evaluations; such potential losses are an important opportunity cost of peer evaluation to weigh against benefits for observees. Observers' performance may also benefit from the opportunity to learn from colleagues and to reflect on their own teaching practice. We believe that our evidence is the first to isolate the impacts on observees and observers in this way.

A second new contribution is to examine the intensive margin of the number of peer observations. In a random half of treatment schools, math department teachers were expected to be observed twice as many times as their English department colleagues; in the other half, the English department was assigned to the double dose.

The effect of evaluation on performance is of growing interest to school administrators and policymakers tasked with managing the teacher workforce. Econometric research beginning in the 1970s, and accelerated in the past decade, demonstrates educationally and economically significant differences in teachers' contributions to their students' learning; those differences make teachers the most influential public input in student outcomes. However, we still understand comparatively little about how to select better teachers, or how to train teachers before or on the job. (For a recent review of the literature see Jackson, Rockoff, and Staiger 2014). The importance of teachers, but lack of management tools, has prompted new attention to teacher performance evaluation in recent years. One common proposal is probationary screening: measure on-the-job performance early and dismiss observed low performers (Gordon, Kane, and Staiger 2006). While the first steps are intuitive, the equilibrium effect of this proposal likely depends more on how labor supply responds than on the measurement process (Staiger and Rockoff 2010, Rothstein 2015). A second common proposal is that the process of evaluation itself should, if done well, improve performance (Milanowski and Henemen 2001, Taylor and Tyler 2012). This proposal is made largely without empirical evidence, though we discuss notable exceptions below, and the present experiment is designed in part to help close that evidence gap.[1]

Our peer evaluation experiment took place over two school years in 82 secondary schools in England. In schools randomly assigned to treatment, year 10 and 11 math and English teachers were asked to participate in a new program of peer classroom observations.[2] Observer teachers visited the classrooms of observee teachers and scored the observees using a rubric based on Charlotte Danielson's *Framework for Teaching* (2007). Within each treatment school, teachers were randomly assigned to roles: observer, observee, or both (1/3 to each). The stated goal was that an observee should be observed six (twelve) times per year in the low (high) dose condition; dose was randomly assigned at the department level. Control schools continued business as usual, which, notably, did not generally involve teacher classroom observations by peers. The main analysis sample includes just over 28,000 students, each with GCSE (end of year 11) test scores in math and English, and approximately 1,300 teachers.

The paper details three main results. First, the program of low-stakes teacher peer evaluation meaningfully improves student achievement in math and English. Students in treatment schools scored 0.076 student standard deviations (σ) higher, on average, than their counterparts in control schools. Nearly four of five of treatment schools completed at least some peer observations during the experiment, suggesting a treatment-on-the-treated effect of 0.097σ at a minimum.

Second, the program benefits are not (strongly) related to the number of peer observations, at least over the range induced by the experiment. The simple experimental difference between outcomes in high- and low-dose conditions was -

[1] Evaluative measures are also an important input to the long-standing proposals for teacher pay for performance. For a review of the theory and empirical literature see Neal (2011) and Jackson, Rockoff, and Staiger (2014), along with a notable more recent example Dee and Wyckoff (2015).
[2] Year 10 and 11 in English schools are the final years of compulsory schooling. Students are typically 14-16 years old in these grades.

0.002σ. A small outcome difference even though the actual number of observations conducted did differ: high-dose departments completed nearly twice as many observations than low-dose departments (2.9 and 1.6 per observee respectively). Additionally, using program and dose random assignment as instruments in 2SLS, we also fit linear and quadratic functions of the actual number of observations conducted; the results suggest diminishing returns.

Third, student achievement improved in the classes of observee teachers as might perhaps be expected. Our new result is that achievement also increased for pupils of the observer teachers. We cannot reject the null hypothesis that treatment effects are equal across the randomly-assigned teacher roles; the point estimates are, however, larger for observers than for observees. This pattern of results rules out the concern that benefits for observees' students come at the expense of losses for observers' students. It may well be that observers' students received less attention from their teacher because she reallocated effort to peer evaluation, but those costs were more than offset by benefits accrued from being an observer.

Our evidence suggests some promising leads for those designing school or system approaches to teacher observation, with valuable improvements for a relatively cheap and straightforward intervention. We return to discuss these implications in the Conclusion.

These results contribute most directly to the still-small literature on how evaluation affects teacher performance. Three papers are especially closely related.[3] First, studying teachers in Cincinnati, Ohio, Taylor and Tyler (2012) also

[3] In a fourth related paper, Steinberg and Sartain (2015) report on a pilot-year experiment in Chicago elementary schools where the treatment was a school-wide program of teacher evaluation based on multiple classroom observations using a Danielson rubric, however, evaluations were conducted by school administrators not peers. Students scored 0.05σ higher in math and 0.10σ higher in English.

An additional experiment in several U.S. school districts, reported by Garet et al. (2017), involved a package of treatments including teacher classroom observation by school estimate the effect of a program of teacher peer evaluation based on multiple classroom observations using a Danielson rubric. The study uses quasi-experimental variation in the timing (the specific school year) of when a teacher was evaluated. During the year a teacher was being evaluated her students scored 0.05σ higher in math, compared to students she taught before being evaluated, and higher, 0.11σ, in the years after she was evaluated. Second, the Cincinnati setting and positive effects of peer evaluation are also mirrored in recent analysis of teachers in France (Briole and Maurin 2019).

While much is similar, including the estimated effects, between the Cincinnati study and the present experiment, there are notable differences which contribute to understanding potential mechanisms. In Cincinnati peer evaluator is a full-time job given to experienced high-performing teachers; by using randomly selected evaluators the present experiment suggests such a high-cost evaluator may not be necessary for the benefits. Moreover, we report estimates of the effects on the observers themselves. In Cincinnati peer evaluation scores are used for dismissal decisions, even if rare empirically; the present experiment suggests explicit incentives may not be necessary for teachers to put effort into the evaluation process.

In contemporaneous work, Murphy, Weinhardt, and Wyness (2018, MWW) study teacher peer classroom observation and feedback among year 4 and 5 teachers in England, also using a school-level experiment. They find no effects on the achievement of participating teachers' students. The MWW treatment and our treatment share some key features—multiple peer classroom observations, low-stakes—but are also different in potentially important ways. The MWW treatment is modeled on the originally-Japanese idea of "lesson study," which is intended to "provide a space for non-judgmental discussion" (p. 6), while our

administrators, but also evaluation measures for principals and test-score based evaluation measures for teachers. The package of treatments improved student math scores by 0.05σ.

treatment is modeled on explicit evaluation programs like the one in Cincinnati.[4] Also, the MWW treatment uses no common rubric, such as Danielson's *Framework for Teaching*, for observations and feedback. While these features are potential explanations for effect differences, we do not have any direct tests. Before any such direct tests, we see these studies—the present study, MWW, Taylor and Tyler, and others—as complementary evidence in understanding the effects of evaluation on teacher performance.

Our results on teacher roles also contribute to another implicit literature, relevant to a practical management question, on "mentor-mentee" or "adviser-advisee" relationships for teachers. Randomly assigning roles, as we do, is a strong contrast to most empirical work studying such relationships. Nearly all existing evidence comes from settings where the "advisor" or "evaluator" is (i) a formal job, with training, filled by an experienced high-performing teacher; or (ii) a school administrator.[5] Papay, Taylor, Tyler, and Laski (in-press) study teacher pairs which are peer coworkers, as in our present experiment, but where pairings were intentionally based on strong-to-weak skill matching. In Murphy, Weinhardt, and Wyness (2018) roles were assigned by the headteacher (principal). Our results based on randomly assigned roles show benefits for both from being observed and from doing the observing. This suggests a plausible alternative to formal, and likely more costly, programs which use administrators or high-skilled former teachers.

In the next section we detail the treatment, setting, and data. In sections 2 and 3 we describe estimation methods and results, respectively. Results are

[4] While neither the MWW nor our treatment included explicit or formal incentives—thus the characterization of "low stakes"—the scope for informal incentives may have differed. Because teachers were actually scored in our treatment, but not in MWW, participants may have believed those scores would end up informing future personnel decisions.

[5] In the case of teacher evaluation specifically, examples of type (i) include Taylor and Tyler (2012), Dee and Wyckoff (2015), Briole and Maurin (2019), and of type (ii) Steinberg and Sartain (2015), Dee and Wyckoff (2015). For a recent review of the broader "advisor-advisee" or "coaching" literature see Kraft, Blazar, and Hogan (2018).

divided into three topics: average total effects at the school level, the relationship between effects and the number of classroom observations, and differential effects for observers and observees. The final section concludes.

1. Treatment, setting, and data

This paper reports on a new field experiment in which coworker teachers observed each other teaching in the classroom, scored performance using a detailed rubric, and discussed the results. The intervention was conducted in secondary schools in England, focusing on years 10 and 11 math and English teachers, over two school years, 2014-15 and 2015-16. This section describes the treatment in detail, the study design, data, and other features of the setting.

1.1 Random assignment design, and covariate balance tests

The experiment involved randomizing aspects of the intervention at three levels: school, department, and teacher. We first randomly assigned 82 schools, half to the treatment—the new peer observation program—and half to a business-as-usual control condition.[6] We describe the recruitment and characteristics of the 82 schools below, as well as what "business-as-usual" means for these schools. Schools were assigned within eight randomization blocks defined by the interaction of three indicator variables, each indicator = 1 if the school was above the sample median for (i) percent of students who are white, (ii) percent of students eligible for free school meals, and (iii) prior student achievement growth at the school (school value-added scores).

Second, we randomly assigned departments to either a "high dose" or "low dose" condition. In half of treatment schools, the math department was

[6] Under the funder's rules, the random assignment procedures were designed jointly by the authors of this paper and the National Foundation for Education Research (NFER), and then carried out by NFER.

assigned "high dose" and the English department "low dose"; and in half of schools the reverse. In the low dose condition observee teachers were expected to be observed 6 times per year. The high dose condition doubled the ask to 12. Department dose randomization was within the same eight blocks.

Finally, within each treatment school, we randomly assigned teachers to different roles in the program. One-third were assigned to be "observers" who visited other teachers' classrooms and scored the peer they watched. Observers were not paired with observees, and could observe either math or English lessons. One-third were assigned to be "observees" whose teaching would be observed and scored by the observers. And the final one-third were assigned to take both observer and observee roles. However, in eight treatment schools teacher rosters were not available to the research team, and thus teachers in those eight schools are excluded from the teacher role estimates.[7] These eight schools are excluded from the main comparisons by teacher role, but are included in all other estimates.

Table 1 shows the conventional pre-treatment covariate balance tests relevant to judging the success of the random assignment. As shown in column 2, treatment and control schools are well balanced on observables. None of the differences are statistically significant at any conventional levels, except the Income Deprivation Affecting Children Index (IDACI) score ($p = 0.065$). A given neighborhood's IDACI score is the proportion of children under 16 living in a low-income household; a student's IDACI value is the score for the neighborhood where they live.

Departments assigned to high- and low-dose are also well balanced on observables, as shown in column 3. The difference in means reported in column 3 is between (i) treatment schools assigned to high-dose for the math department and low-dose for English, and (ii) treatment schools assigned to the reverse case.

[7] While much of the data for this study was drawn from administrative data sources, as described below, we have to rely on treatment schools to provide lists of teachers and class rosters.

Last, we examine balance for the teacher role assignment experiment. Unfortunately, we do not have data on teachers themselves, so columns 4-5 compare characteristics of students in the classrooms of observer versus observee teachers (and both role versus observee teachers). Here, for the role assignment, we do see differences which we would not expect after successful random assignment. Students of observer or both-role teachers may well have higher potential GCSE scores, compared to observee students, at least by conventional predictors. Observer students have lower exposure to poverty in their homes and neighborhoods, and perhaps higher prior math and English scores. Both-role students have higher prior scores, and somewhat less exposure to poverty. Below, after presenting the basic results on teacher role, we discuss interpretation of those results given the imbalance in Table 1 and provide some relevant robustness tests.

1.2 Description of the treatment

The treatment, in short, is peer classroom observations among coworkers teaching in the same school. As described above, teachers were randomly assigned to either observe or to be observed. Each classroom observation was guided by and scored using a detailed rubric based on Charlotte Danielson's *Framework for Teaching* (2007, "FFT"), and lasted approximately 15-20 minutes. The stated goal was 6 or 12 observations per observee teacher per year, where 6 or 12 was randomly assigned as described above. While not required, teachers were encouraged to meet after observations to discuss feedback and share strategies for improvement.

The FFT rubric is widely used by schools and in research (for example, Kane et al. 2011, Kane et al. 2013, Bacher-Hicks et al. 2017). The rubric is divided into four "domains"—classroom environment, instruction, planning, and assessment—with several "standards" within each domain. In the current experiment, classroom observations used only the "classroom environment" and “teaching” domains, which are measured while watching teachers teach. In Figure 1 the left-hand column lists the ten standards on which teachers were evaluated. For each standard, the rubric includes descriptions of what observed behaviors should be scored as “Highly Effective” teaching, “Effective,” “Basic,” and “Ineffective.” In Figure 1 we reproduce the descriptions for “Effective” as an example. The full rubric is provided in the Appendix. The current experiment’s rubric language differs slightly from the standard FFT rubric descriptions; the text was edited slightly to be more appropriate for the English school setting.

The FFT, and other similar observation rubrics and protocols, were not explicitly designed as a tool to improve student achievement scores, as we measure in this experiment. Nevertheless, existing evidence is consistent with expecting positive effects of this “treatment.” First, several studies now find a similar, if moderate, positive correlation between observation scores and student test scores, including some settings where students are randomly assigned to teachers (Kane et al. 2011, Kane et al. 2013, Garrett and Steinberg 2015, Araujo et al. 2016, Bacher-Hicks et al. 2017). Second, as cited in the introduction, a growing number of (quasi)-experimental studies document positive effects on student achievement of programs where teachers are observed and scored using FFT or similar rubrics (Taylor and Tyler 2012, Steinberg and Sartain 2015, Garet et al. 2017, Briole and Maurin 2019).

Conventionally the FFT rubric is scored on a 1-4 integer scale, with 1-4 corresponding to “Ineffective” through “Highly Effective”. We instead asked observers to use a 1-12 integer scale where 12 was “Highly Effective+”, 11 “Highly Effective”, 10 “Highly Effective−", 9 “Effective+”, and so on. The 1-12 scale was motivated in part by the typical skewness toward scores of 3-4 on evaluation ratings using a 1-4 scale generally, and with teacher observation rubrics specifically (Kane et al. 2011, Kraft and Gilmour 2016). This tendency was confirmed in our pilot stage work with schools.[8]

In addition to the FFT-based assessments of teaching quality, we also asked observers to record other relatively-objective data on teaching practices. For example, how often—never, some of the time, all of the time—the teacher lectured the whole class, had students work in small groups, or taught students one-on-one. These measures were also drawn from previous research, and the complete instrument is provided in the Appendix.

Teachers were provided training on the FFT rubric and other aspects of the program, primarily in-person training but supplemented with phone and email conversations. Treatment schools were also given a few tablet computers to facilitate observations. Observers could access the complete FFT rubric, record scores, and make notes during their observations.[9] The centrally-stored database of observations allowed the research team to monitor progress of individual schools, and contact those who were clearly lagging. However, the specific schedule and pace of observations was left to each school to determine.

1.3 Evaluation and classroom observation in English secondary schools

Classroom observations of teaching are certainly not new to the schools we study, and observations were likely occurring in control schools during the study. However, the treatment observations had three (potentially) first-order features distinct from business-as-usual in English secondary schools: observation

[8] Pooling the 10 standards, the average rubric item score was 9.05 with a standard deviation of 2.10. If we convert the 1-12 scale to the more common 1-4 scale (i.e., 12 = 4.33, 11 = 4, 10 = 3.66, 9 = 3, and so on) the average item score is 3.35 (st.dev. 0.70). This mean is quite similar to other contexts, for example, Kane et al. (2011) where the mean is 3.23. However, in our setting the standard deviation is a wider 0.70 versus, for example, 0.49 in Kane et al. (2011). The wider variation may be due in part to the 1-12 scale.

[9] The tablets were Apple iPads, and the software was created by RANDA Solutions.

by peer teachers, observations based on an FFT-style rubric, and simply more observations regardless of who or what rubric.

In our conversations with study schools, most reported that some form of class observations were part of their normal business. In contrast to the treatment peer observations, these school-initiated observations were conducted by school leaders, unstructured, and much rarer. The average teacher would be observed perhaps once per year and often less than annually. Moreover, the frequency of observations was curtailed partly by union opposition, sometimes codified into rules limiting observations. Consistent with this description of limited status-quo observation, treatment teachers reported appreciating that the program included more frequent observations and observations from peers instead of school leaders.

Beyond school-initiated efforts, classroom observations occur for two other reasons. First, observations are part of the formal induction and assessment process for novice teachers, known as “NQT teachers” or the “NQT year” (NQT = newly qualified teacher). An NQT teacher might be observed as many as six times, but the teachers are typically only NQT for one year.

Second, observations are part of England’s formal performance evaluation process for schools. The school inspection process, conducted by the Office of Standards in Education, Children’s Services and Skills (Ofsted), does include classroom observations among many other forms of evaluation. However, Ofsted’s classroom observations are not salient to individual teachers. During a school inspection Ofsted observers visit several teachers’ classes, but far from all teachers; the goal of Ofsted’s observations is to make overall assessments of teaching in the school, not of each individual teacher. Moreover, a typical school might only be visited by Ofsted every 3-4 years. In short, the chances of a given teacher in a given year being observed by Ofsted are low, and there would be little individual consequence of the results.[10]

1.4 Sample and data

Our study sample is composed of 82 schools, over 28,000 students with GCSE math and English test scores, and approximately 1,300 teachers. We initially contacted almost all high-poverty public (state) secondary schools and invited them to participate in the study.[11] School performance levels (test scores) were not used as a criterion for inviting schools. Of the invited schools, 93 responded and volunteered to participate (8.5 percent). We randomized 82 schools to treatment and control; ten schools dropped out before randomization and one was excluded by the research team[12]. The schools initially invited were intentionally selected to have high poverty rates. These characteristics are reflected in the study sample, as shown in Table 1 column 1. Nearly 40 percent of students are (or ever have been) eligible for free school meals, substantially higher than the national average.

Much of the data for our analysis come from the UK government's National Pupil Database (NPD). These administrative data include student level records with math and English GCSE scores, prior test scores (KS2), demographic and other characteristics of students, and their school. The NPD data are sufficient for our ITT estimates of average treatment effect.

[10] Ofsted and the Department for Education (DfE) also set expectations and guidelines for each school's own self-evaluation practices. Classroom observations broadly-speaking should be part of each school's plans, but Ofsted and DfE do not require a specific minimum number, type of observer, or criteria for what should be evaluated in the observation. Moreover, until recently there was a rule limiting observations to no more than three per year.
[11] We excluded, ex-ante, boarding schools, single-gender schools, as well as schools in select geographic areas where the funder was conducting different interventions. The final list invited was 1,097 schools.
[12] The school was in Wales rather than England and so was not in the NPD.

We add to the NPD data in two ways. First, the data recorded during peer observations allow us to measure participation, for example, the number of observations completed for a given school, department, or teacher. The data also include observation scores, which, at present, we do not use in this paper. Second, we asked treatment schools to provide a list of their math and English teachers, and the specific year 10 and 11 students assigned to those teachers. We link these rosters to NPD data using unique student IDs, and to the observation data using teacher IDs created for this study. Additionally, the teachers listed in these rosters were the teachers included in the role randomization. Note that we lack these teacher-student linking rosters for 8 of the 41 treatment schools.[13]

Our estimates are not threatened by attrition, at least not in the first-order sense of attrition. The NPD data include the universe of students and schools. Thus, even if a school chose to withdraw from the study, we observe outcomes and can still include the school in our analysis. If, however, treatment induced students to move to different schools at a rate higher (lower) than control schools, those movements would be relevant to the interpretation of our results. Treatment effects on student school switching seems unlikely. Treatment effects on *teachers* switching schools may be more plausible. Our analysis of differences between teacher roles uses teacher's class rosters provided at the beginning of each school year, in the spirit of intent-to-treat. We cannot observe teacher movements between schools in the data as we do not know teacher names, but any such moves induced by treatment would be relevant to interpreting our results.

2. Analysis methods

[13] Some schools never provided complete rosters. In other cases, the school provided the rosters, but subsequently withdrew from the study and asked that their previously-provided rosters not be used in the research.

Our analysis of the experiment data follows conventional experimental methods. To begin we estimate the difference between average student GCSE scores in treatment and control schools by fitting the following regression specification:

$$Y_{imt} = \delta T_s + \pi_b + X_i\beta + \varepsilon_{imt} \tag{1}$$

where Y_{imt} is the GCSE score for student i in subject m (math or English) taken in year t (2015 or 2016). Student scores, Y_{imt}, are standardized (mean 0, s.d. 1) by subject and year within our analysis sample. The indicator $T_s = 1$ for all schools s randomly assigned to the treatment, and π_b represents fixed effects for the eight blocks b within which random assignment occurred.[14] The vector X_i includes the student characteristics measured pre-treatment listed in Table 1, most notably prior achievement scores in math and English, along with a cohort dummy (the year the exams were taken) and a subject dummy (math or English). We report heteroskedasticity-cluster robust standard errors where the clusters are schools s, the unit at which treatment is assigned.[15]

Fitting specification 1 returns intent-to-treat effects. We also report treatment-on-the treated estimates where T_s is replaced with an indicator if the school actually implemented the peer observation program, and we instrument for that endogenous treatment indicator with the randomly assigned T_s. Because the latent characteristic "implemented" is not binary, we show a few different alternatives for the endogenous treatment indicator.

To examine how the number of peer observations contributes to treatment effects, we use the "high-" and "low-dose" experimental conditions. We add an

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[14] Students are nested in schools, $s = s(i)$, and schools are nested in randomization blocks, $b = b(s)$. To streamline the presentation we use the simple s and b subscripts.

[15] Our main estimates pool subjects. Students' math and English score errors are also likely correlated. Since students are nested within schools, clustering by school is identical to clustering by school and student.

indicator $H_m = 1$ if department m was randomly assigned to the high dose condition, $H_m = 0$ if department m was low dose, and $H_m = 0$ for all control schools.

$$Y_{imt} = \delta T_s + \gamma H_m + \pi_b + X_i \beta + \varepsilon_{imt} \tag{2}$$

Thus, the coefficient γ measures the added (reduced) treatment effect above (below) δ. Again, we also fit instrumental variables models where T_s and H_m in equation 2 are replaced with (some function of) the count of observations actually completed in by department m in school s, and we instrument for that endogenous count using T_s and H_m.

Last, we estimate differences in student outcomes by their teacher's role in the peer observation: observer or observee or both. We fit the specification:

$$Y_{imt} = \alpha_1 VEE_{j(imt)} + \alpha_2 BOTH_{j(imt)} + \omega_s + X_i \beta + \nu_{imt} \tag{3}$$

where $VEE_{j(imt)}$ is an indicator $= 1$ if student i's teacher j in year t for subject m was randomly assigned to be an observee, and similarly $BOTH_{j(imt)} = 1$ if the teacher was assigned to both roles. The omitted category is when the teacher was assigned to be an observer. The ω_s represents fixed effects for each treatment school; random assignment of roles occurred within schools. Recall that the sample for estimating specification 3 is a subset of the sample for specifications 1 and 2 because we lack teacher-student class rosters for eight treatment schools. We report heteroskedasticity-cluster robust standard errors where the clusters are teachers j, the unit at which treatment is assigned, as well as where the clusters are schools for comparison.

A causal interpretation of our key estimates—$\hat{\delta}$, $\hat{\gamma}$, $\hat{\alpha}_1$, and $\hat{\alpha}_2$—requires the conventional experimental identification assumption: In the absence of the experiment, students in treatment and control schools (high- and low-dose departments, observee and observer teacher classes) would have had equal GCSE scores at expectation. Balance in pre-treatment covariates (Table 1) across treatment and control schools and high- and low-dose departments is consistent with this assumption. By contrast, the imbalance across observer and observee classrooms suggests caution giving a strong causal interpretation to $\hat{\alpha}_1$ and $\hat{\alpha}_2$. We return to the interpretation of the role differences below.

3. Results

3.1 Effect of the peer evaluation program

Teacher peer evaluation, with the program features described in section 2, produced educationally and economically meaningful improvements in student learning, most likely by boosting teachers' contributions to their students' learning. During the two treatment years, students in treatment schools scored 0.07 student standard deviations (σ) higher (ITT), on average, than their counterparts in control schools. In treatment schools that took-up the program the benefit was at least 0.09σ (TOT).

Estimates of the differences between treatment and control schools are formalized in Table 2. The simplest treatment effect estimate, 0.056σ, is shown in column 1: intent-to-treat, pooling the two treatment years and both subjects, and controlling only for randomization block fixed effects. That simple estimate is not precisely estimated, however. In column 2 we add pre-treatment controls to improve precision. Under the assumption that random assignment balanced expected potential outcomes, both column 1 and 2 are consistent estimates for the causal effect of treatment. While the point estimates are somewhat different, 0.056σ and 0.073σ, we cannot reject the statistical null hypothesis that they are the same.[16]

Improvements of 0.06-0.08σ in math and English are educationally and economically significant. The most likely mechanisms for these results, which we discuss in more detail below, are mechanisms which operate through teachers' causal contributions to student test scores. Improving a teacher's contribution by 0.06-0.08σ would move her up perhaps one-fifth to one-half of a standard deviation in the teacher value-added distribution.[17] Such improvements in teacher performance are large, but not unprecedented in the literature. As cited in the introduction, Taylor and Tyler (2012) find improvements of 0.05-0.11σ from a relatively similar peer evaluation program. Jackson and Makarin (2017) report improvements of 0.06-0.09σ in an experiment where teachers were provided with high-quality math lesson plans. Finally, our estimate of 0.06-0.08σ is roughly similar to the performance improvements made by new teachers in the first 3-5 years on the job (see Jackson, Rockoff, and Staiger 2014 for a recent review).

As is common in field experiments, some schools randomly assigned to the treatment and encouraged to implement the new peer evaluation program nevertheless chose not to participate or participated relatively little. In columns 3-5 of Table 2 we report rough bounds for the treatment effect on the treated. Of the

[16] Controlling for pre-treatment covariates increases the effect point estimate in part because treatment schools had students with slightly lower prior achievement, though the difference is not statistically significant, and slightly higher rates of poverty.

[17] Slater, Davies, and Burgess (2011) estimate the standard deviation of teacher contributions to GCSE (value-added) scores is 0.272 student standard deviations. This estimate comes from English secondary schools and GCSE courses, as in our current study, though the sample in Slater, Davies, and Burgess (2011) is broader. Judged against this 0.272 estimate, the treatment effects would be one-fifth to one-third of a teacher standard deviation. The 0.272 estimate may be larger than other estimates (e.g., from US elementary and middle schools) in part because students spend two years with their GCSE teacher.

For a general summary of estimates on the teacher value-added distribution see Jackson, Rockoff, and Staiger (2014) and Hanushek and Rivkin (2010). Though many estimates of the teacher value-added distribution come from elementary and middle schools, and the variation may be greater or smaller in (English) secondary schools.

41 assigned to treatment, 7 schools did not complete any peer observations at all. Thus a lower bound on the TOT estimate is 0.076σ divided by $34/41 = 0.83$, or about 0.092σ. Column 3 of Table 2 formalizes this estimate using 2SLS, where the endogenous treatment indicator = 1 if the school completed at least one peer observation. This estimate is weighted by the number of students in each school. (First stage estimates for columns 3-5 are provided in Appendix Table 2.) Our upper bound TOT estimate is rougher because we must choose some cutoff for a more restrictive definition of "implemented" the treatment. In column 4 of Table 2 the endogenous treatment indicator = 1 if the school completed 10 percent or more of the peer observations they were originally asked to conduct.[18] Of the 41 schools, 25 met the, admittedly somewhat arbitrary, condition of 10 percent or more. If we scale up the ITT by this stricter first stage, the implied TOT would be 0.13σ. In column 5 the endogenous indicator $= 1$ if more than half of teachers participated in at least one observation, as observer or observee. If we scale up the ITT by the implied first stage, we get an estimate of 0.16σ. In short, we believe a plausible range for the treatment-on-the-treated effect is roughly 0.09-0.16σ. In the next subsection we return to the question of whether treatment effects depend on the number of peer observations.

The results in Table 2, and throughout most of the paper, pool subjects and years. Pooling simplifies the presentation of results and improves precision. In Appendix Table 3 we report estimates separately by subject and student cohort. The point estimates are slightly larger for English GCSE scores and in the first year of the treatment, but differences across subject and year are not statistically significant at conventional levels.

[18] We asked schools to complete 6 or 12 observations per year for each observee teacher. Whether 6 or 12 was determined by the department-level dosage assignment discussed and analyzed in the next subsection.

3.2 Treatment effects and the number of peer observations

A first-order design feature of a teacher observation program, like the one we study, is the number of classroom observations conducted. Our treatment effect estimates, discussed in the previous subsection, are effects for the average treatment school. The corresponding number of observations completed by the average treatment school is 2.27 per observee teacher per year (standard deviation 2.67). The natural follow-up question is: Would the estimated average treatment effect be larger or smaller than 0.06-0.08σ if the number of observations conducted were larger or smaller? Are the marginal returns linear, increasing, decreasing? In this subsection we provide some estimates relevant to these questions, but our answers are constrained by the variation in our experiment.

The cleanest result comes from direct experimental variation in the number of observations. Recall that, within each treatment school, one of the two departments (math or English) was randomly assigned to a low-dose condition of six observations per observee per year, and thus the other of the two departments (English or math) was randomly assigned to a high-dose condition of twelve observations. The average number of observations actually conducted was 1.6 and 2.9 in the normal- and double-dose conditions respectively (see the first stage results in Appendix Table 4). Actual observations were certainly short of what teachers were initially asked to do, but the *difference* in actual observations caused by the dosage random assignment was still nearly a doubling of observations.

That experimental doubling of observations did not, however, increase the treatment effect. In Table 3 column 1 we show ITT estimates from a specification identical to Table 2 column 2 except that we have added a right-hand-side indicator = 1 if the department was randomly assigned to the high-dose condition (the new indicator is zero for all control cases). The estimated effect is close to zero, -0.002σ, and far from statistically significant. This test, though blunt, suggests little covariance between treatment effects and number of observations over the range of 1.6-2.9 observations.[19]

Table 3 also reports estimates of the relationship between student achievement scores and the actual number of observations completed per observee teacher. These are 2SLS estimates where we instrument for actual observations with two random assignment indicators: school-level treatment and department-level high-dose.[20] The estimated marginal benefit of an additional observation per observee is 0.032σ if we restrict the relationship to be linear (column 3). Alternatively, if we allow a quadratic relationship, the marginal benefits are somewhat larger near the sample mean, but then diminishing (column 4). We can reject the null hypothesis that the linear fits the data as well as the quadratic. As shown in columns 5-6, this pattern of results is the same if we use a different measure of observations completed: the proportion of suggested observations completed.

Taken together the results in Table 3 are somewhat ambiguous. The reduced form estimates in column 1 suggest no relationship between treatment effects and number of observations. The 2SLS estimates in columns 3-6, by contrast, suggest a positive, diminishing relationship. Both the reduced form and 2SLS estimates rely on the same fundamental identifying variation arising from random assignment. The 2SLS estimates, however, require an additional parametric assumption about the relationship (linear or quadratic).[21] If that assumption is incorrect, then the 2SLS estimates may be misleading. Appendix

[19] The estimates in Table 3 column 1 are identified by random assignment. As a robustness check, in column 2 we estimate a student fixed effects version of equation 2. The results are the same.

[20] The first stage results are provided in Appendix Table 4. The endogenous variable is the total number of lessons observed for department d in school s for cohort t, divided by the number of observees in the same cell. This variable is zero in all control school cases.

[21] The 2SLS estimates also require an exclusion restriction, which seems reasonable given that the instruments are randomly assigned.

Figure 1 provides some evidence, if imperfect and noisy, suggesting the parametric assumptions are reasonable.[22]

3.3 Treatment effects and teacher roles

Another first-order feature of a teacher observation program is deciding who should be observed (observee) and by whom (observer). By randomly assigning these teacher roles, our experiment contributes distinctively new evidence to the literature on teacher peer evaluation, the first to estimate the impact on observers. First, the estimates of total effect and dosage presented above are not (less likely) driven by selecting observers and observees on some (un)observable potential outcomes. To this point in the paper the results presented have pooled all teachers and students regardless of role. Second, in this subsection we estimate treatment effects separately for observer and observee teachers.

Table 4 shows test score differences by teacher role. We begin with a regression that includes all teachers and schools in the study. For convenience, Table 4 column 1 repeats Table 2 column 2—our main between-school treatment-control difference. In Table 4 column 2, the omitted category remains all control schools and teachers, but we now divide treatment school students into five mutually exclusive and exhaustive groups. Groups (i)-(iii) are the students of teachers who were randomly assigned to the experiment roles (i) observer, (ii) observee, and (iii) both roles. For example, students in the classrooms of observer teachers scored 0.15σ higher than the average control student. Group (iv) is students assigned to all other teachers in treatment schools; teachers who did not

[22] In Appendix Figure 1 we provide some less-parametric evidence on the relationship between outcomes and number of observations per observee. First, for each treatment school, we calculate the school's mean GCSE test score, minus the overall control mean score. This estimate is not, strictly speaking, a school specific treatment effect estimate identified by random assignment, but we think it is informative. Second, we plot these 41 estimates against the number of observations completed per observee, and fit a lowess line. The fitted line suggests increasing returns at low levels of observations (less than 2-3), but flat returns after that point.

participate in peer observation experiment, and thus were not assigned a role. Non-participating teachers include temporary teachers—called cover teachers in England (long-term substitutes in the US)—as well as other teachers excluded by their schools for other reasons. Group (v) is all students in the eight treatment schools where we do not have any student-teacher class rosters.[23]

The differences in Table 4 column 2 are not strictly identified by random assignment, however. Only the groups (i)-(iii) were randomly assigned. The expected student score in the control group, as a whole, is not necessarily an appropriate counterfactual for any one of the five groups shown in column 2.

The cleanest, intent-to-treat results for the role experiment are in Table 4 column 4. The differences in column 4 are by design identified by random assignment. The sample is limited to teachers and schools which participated in the role random assignment, and we use only variation within schools.

The students of observer teachers scored higher, on average, than the students of teachers who were observed by 0.043σ, but the difference is not statistically significant at any conventional level. Scores for "both role" teachers are between observers and observees, but again not statistically different. Additionally, we find little difference across assigned roles in extensive or intensive margins of actual participation in peer observations, so that treatment-on-the-treated estimates show the same pattern as in Table 4 column 4.[24]

These results imply that the average treatment effects, reported in Table 2, are shared relatively evenly by both observer and observee teachers. Similar effects do not necessarily imply similar mechanisms, however, and we return to a discussion of mechanisms for observers and observees below. These results also

[23] As a robustness check, we re-estimate the overall treatment effects in Table 2 dropping these eight schools. The estimates are larger: 0.10σ for the ITT with covariates, and 0.11σ for our lower-bound TOT. These larger estimates are partly a reflection of the correlation between providing rosters and participating in other ways.

[24] 2SLS TOT estimates and associated first stages for the role experiment are provided in Appendix Tables 5 and 6 respectively.

rule out the hypothesis that benefits for observees come at the opportunity cost of reduced achievement in observers' classrooms as observers shift effort or time to observations. There may well have been such opportunity costs, but they were offset by gains for the observers.

For one final observation about mechanisms we return to Table 4 column 3. As in column 4 we find little difference across the randomly assigned teacher roles. By contrast, the students of other non-participating teachers scored noticeably lower than their schoolmates in observers' and observees' classrooms; a difference of -0.202σ. This pattern would be consistent with effects for participating teachers, but no (small) spillovers to non-participating teachers. But this pattern could also be consistent with some selection of teachers into the role experiment randomly-assigned sample, presumably by school administrators but based on factors unobservable to us. Regarding this second explanation, we do not have pre-treatment teacher performance measures, but we can compare students' pre-treatment characteristics and we find no differences for participating and non-participating classrooms.

3.3.1 Robustness

An interpretation of the differences between roles—the results in Table 4 column 4—as causal relies on the random assignment successfully balancing potential outcomes, as noted above. The conventional test of balance is to compare pre-treatment covariates across conditions, and, as shown in Table 1, there is imbalance on prior student achievement and poverty measures. We can and do control for these observable differences, but, as always, the concern is that observable differences suggest scope for unobserved differences. In the end, this imbalance should add caution to a strong causal interpretation of the differences between roles. However, in this section we provide evidence to inform judgements about the potential bias from this imbalance.

The first result is simply to re-estimate Table 4 column 4 without the pre-treatment covariates, as shown in Table 4 column 5. These point estimates are larger, as expected given the imbalance, but still not statistically significantly different. Thus, no change to the substantive conclusion that treatment effects were similar across roles.

For a second robustness test we use only within student variation: differences, for a given student, in her math teacher's role and her English teacher's role. These student fixed effects estimates, in Table 4 column 6, are consistent with our main estimates: no differences between observers and observees. Again these are assigned roles, thus in the spirit of ITT. Also consistent, achievement was about 0.08σ higher in the subject where a student's teacher was assigned to a role in the peer evaluation compared to the subject where the student's teacher was not involved. Student fixed effects weaken the identifying assumption. In the conventional experiment, sorting between students would potentially threaten identification. With student fixed effects, threats require sorting based on differences in math and English potential outcomes for a given student.

A third robustness test builds on the fact that role assignment occurred within each treatment school. We can thus estimate the degree of pre-treatment covariate (im)balance for each school, and then see whether treatment effects covary with the estimate of (im)balance. In Appendix Table 7 we show estimates like Table 4 column 4 separately for two subsamples of treatment schools segmented into relatively "low imbalance" and "high imbalance" in the following way. First, for each student, we convert the available pre-treatment covariates into a scalar index measure. Using the control sample, we regress GCSE score on those covariates, and then calculate the fitted GCSE score for treatment cases. That fitted score is our index. Second, for each school, we estimate the mean difference in that index between observers and observees. We define relatively "low imbalance" schools as schools where the absolute fitted-score difference of less than 0.10σ, and "high imbalance" schools those with 0.10σ or greater. In the relatively low imbalance schools, the pattern of effects mirrors the main estimates in Table 4 column 4: small differences which are not statistically significant. However, we cannot say what made some schools "low imbalance" and others "high imbalance," and so we are cautious about generalizing the results.[25]

4. Conclusion

In this paper we report improvements in teachers' job performance, as measured by their contributions to student test scores, resulting from a program of low-stakes peer evaluation. In randomly assigned treatment schools, teachers visited the classrooms of other teachers in the school, and scored the teaching they observed using a structured rubric. Students in treatment schools scored 0.07 student standard deviations (σ) higher on high-stakes GCSE exams in math and English (ITT estimate). In treatment schools which took-up the program students cored at least 0.09σ higher (minimum TOT estimate). Explanations for the effects of this peer evaluation cannot be based on explicit incentive structures typical in other formal evaluation settings as these were absent by design; rather the effects likely operate through new information about individual performance as well as interaction among peer coworkers.

The school-level results we report contribute to a small literature on how evaluation, in different forms, affects teacher job performance (e.g., Taylor and

[25] The results by teacher role are also robust to how we pool observations. The experiment included teachers of year 10 and year 11 students over two school years. Our data include "Cohort 1" students who where in year 11 in the experiment's first year, and took their GCSE exams at the end of that year. And "Cohort 2" who were in year 10 in the experiment's first year, then year 11 the experiment's second year, and thus took GCSEs at the end of year two. Most "Cohort 2" students had the same teacher (and thus the same role indicator variables) in both years. However, we estimated role differences separately using (i) Cohort 1, year 11 teacher, (ii) Cohort 2, year 10 teacher, and (iii) Cohort 2, year 11 teacher to construct the role indicators. The results are consistent across these approaches, as shown in Appendix Table 8a and 8b.

Tyler 2012, Steinberg and Sartain 2015, Bergman and Hill 2015, Dee and Wyckoff 2015, Briole and Maurin 2019). Our school-level estimates are broadly consistent with similar treatments in quite different settings. For example, Taylor and Tyler (2012) report quasi-experimental estimates of the effect of evaluation using rubric-based classroom observations. They find teacher performance improved by 0.05-0.11σ in a rather different setting to ours: younger pupils (grades 4-8); evaluation with explicit consequences for the teachers; and the evaluators were trained, external specialists. Steinberg and Sartain (2015) find similar results when the evaluators are school principals. In contemporaneous work, Murphy, Weinhardt, and Wyness (2018) find that teacher performance did not significantly change in an experimental implementation of "lesson study" in primary schools (other operational differences from our study are noted above).

The main new contribution of the paper is based on our randomization of teacher roles, allowing us to estimate the benefits to the different evaluation roles. We believe this is the first experimental evidence on the job performance benefits to the observers in a teacher evaluation program. As might be expected, the observed teachers do benefit from peer observation, as measured by the test score gains of their pupils. We show that the pupils of the observing teachers also benefit, and perhaps to a greater extent than the teachers they observe. The most likely interpretation of this is that the observers learn new ideas for their own teaching and have an opportunity to reflect on their own practice in a non-challenging setting. The implication of this is quite striking: schools that out-source the evaluator role and schools in which observation/evaluation are just another task for school leaders are missing out on more than half of the potential gain to observation.

The benefits of peer evaluation documented in this experiment suggest practical and promising policy ideas for improving the job performance of a sizable workforce. Relative to other educational interventions, this program seems both politically and financially attractive. Politically, the program is likely less threatening because of the absence of strong or explicit consequences attached to the scores, and because observers are peers rather than outside experts or school leaders. Financially because it is a cheap intervention, at least in budget terms. The first-order costs of the program are the opportunity costs of participating teachers' time. Potential forgone uses of teachers' time and effort include (i) attention to their current students (or other job responsibilities), (ii) other investments in developing new or existing skills, or (iii) teachers' own leisure. These opportunity costs are potentially offset, as we find, when observers themselves improve as a result of their role. Indeed, schools or school systems that pay for external evaluators pay twice: the fee for the service and the lost learning experiences for the school's own teachers.

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Figure 1—Rubric standards and associated description of “Effective”

Domain 1. Classroom Environment

1.a Creating an Environment of Respect and Rapport	Classroom interactions, both between teacher and students and among students, are polite and respectful, reflecting general warmth and caring, and are appropriate to the cultural and developmental differences among groups of students.
1.b Establishing a Culture for Learning	The classroom culture is characterised by high expectations for most students and genuine commitment to the subject by both teacher and students, with teacher demonstrating enthusiasm for the content and students demonstrating pride in their work.
1.c Managing Classroom Procedures	Little teaching time is lost because of classroom routines and procedures for transitions, handling of supplies, and performance of non-teaching duties, which occur smoothly. Group work is well-organised and most students are productively engaged while working unsupervised.
1.d Managing Student Behaviour	Standards of conduct appear to be clear to students, and the teacher monitors student behaviour against those standards. The teacher response to student misbehaviour is consistent, proportionate, appropriate and respects the students’ dignity.
1.e Organising Physical Space	The classroom is safe, and learning is accessible to all students; the teacher ensures that the physical arrangement is appropriate for the learning activities. The teacher makes effective use of physical resources, including computer technology.

Figure 1 (cont.)—Rubric standards and associated description of “Effective”

Domain 2. Teaching

2a Communicating with Students	Expectations for learning, directions and procedures, and explanations of content are clear to students. Communications are accurate as well as appropriate for students’ cultures and levels of development. The teacher’s explanation of content is scaffolded, clear, and accurate and connects with students’ knowledge and experience. During the explanation of content, the teacher focuses, as appropriate, on strategies students can use when working independently and invites student intellectual engagement.
2b Using Questioning and Discussion Techniques	Most of the teacher’s questions elicit a thoughtful response, and the teacher allows sufficient time for students to answer. All students participate in the discussion, with the teacher stepping aside when appropriate.
2c Engaging Students in Learning	Activities and assignments, materials, and groupings of students are fully appropriate for the learning outcomes and students’ cultures and levels of understanding. All students are engaged in work of a high level of rigour. The lesson’s structure is coherent, with appropriate pace.
2d Use of Assessment	Assessment is regularly used in teaching, through self- or peer-assessment by students, monitoring of progress of learning by the teacher and/or students, and high-quality feedback to students. Students are fully aware of the assessment criteria used to evaluate their work and frequently do so.
2e Demonstrating Flexibility and Responsiveness	The teacher promotes the successful learning of all students, making adjustments as needed to lesson plans and accommodating student questions, needs, and interests.

Note: Adapted from *Framework for Teaching* (Danielson 2007) for the current experiment.

Table 1—Pretreatment characteristics

	Sample mean (st.dev.)	School assigned to treatment – control [p-value]	School assigned to math dept. high dose – math low [p-value]	Teacher, in a treatment school, assigned to: Observer – observee [p-value]	Teacher, in a treatment school, assigned to: Both roles – observee [p-value]
		Difference in means			
	(1)	(2)	(3)	(4)	(5)
Prior math score	0.007	-0.029	-0.023	0.121	0.177
	(0.998)	[0.872]	[0.485]	[0.107]	[0.015]
Prior English score	0.006	-0.006	-0.037	0.108	0.146
	(0.999)	[0.475]	[0.693]	[0.104]	[0.022]
Female	0.487	-0.020	0.016	0.017	0.016
		[0.275]	[0.364]	[0.285]	[0.395]
IDACI	0.276	0.031	0.023	-0.015	-0.004
	(0.171)	[0.064]	[0.362]	[0.004]	[0.444]
Free school meals	0.398	0.019	0.018	-0.044	-0.03
	(0.490)	[0.379]	[0.610]	[0.009]	[0.06]
Birth month (1-12)	6.569	-0.034	0.060	-0.156	-0.049
	(3.419)	[0.287]	[0.200]	[0.095]	[0.615]
London school	0.162	0.028	0.102		
		[0.702]	[0.437]		
Diffs. jointly zero, p-value		[0.323]	[0.246]	[0.026]	

Note: For each pre-treatment characteristic, column 1 reports the study sample mean (standard deviation), with 28,704 student observations, the full sample.

Column 2 reports the treatment minus control difference in means, and p-value for the null that the difference is zero. Differences and p-values come from a regression of the pre-treatment characteristic on an indicator for treatment and randomization block fixed effects. The standard errors allow for clustering at the school level. The bottom row reports on a joint test that all the treatment-control differences are simultaneously zero.

Column 3 reports the difference in means between treatment schools assigned to (i) high dose math and low dose English and those assigned (ii) low dose math and high dose English. Differences and p-values are estimates as in column 2, except that the sample is limited to treatment schools.

Column 4 reports the difference in means between students of (i) teachers randomly assigned to the observer role and those assigned (ii) the observee role. Column 5 similarly reports the (i) both role minus (ii) observee role difference. The sample is limited to 33 schools which returned class lists for the role experiment. Differences and p-values come from a regression of pre-treatment characteristic on an indicator for observer and an indicator for both roles, with observee the omitted category, as well as school fixed effects. The standard errors allow for clustering at the teacher level.

Table 2—Effect of teacher peer observation program on student achievement scores

(A) Intent to treat estimates			
	(1)	(2)	
School randomly assigned to treatment	0.056	0.073*	
	(0.040)	(0.032)	
Pre-treatment covariates		√	
Adjusted R-squared	0.022	0.343	
(B) Treatment on the treated estimates under alternative definitions of treatment take-up			
	(3)	(4)	(5)
School completed at least one peer observation	0.093*		
	(0.039)		
School completed at least 10 percent of suggested observations		0.133*	
		(0.056)	
At least 50 percent of teachers participated once or more often			0.157*
			(0.072)
Pre-treatment covariates	√	√	√
Adjusted *R*-squared	0.345	0.344	0.342
First-stage *F*-statistic excl. instrument	2562.5	704.1	354.3

Note: Each column reports results from a separate least squares (panel A) or two-stage least squares (panel B) regression, with 56,148 student-by-subject observations. The dependent variable is student math or English GCSE score in student standard deviation units. All specifications include randomization block fixed effects, and an indicator for math observation. Pre-treatment covariates include the characteristics listed in Table 1 and an indicator for cohort 1. When a pre-treatment covariate is missing, we replace it with zero and include an indicator variable = 1 for missing on the given characteristic. For the instrumental variables estimates in panel B, the row headers describe the endogenous treatment indicator variable, which is instrumented for with the randomly assigned treatment condition indicator. Heteroskedasticity-cluster robust standard errors in parentheses, with clustering at the school level.
+ indicates p<0.10, * 0.05, and ** 0.01

Table 3—Number of peer observations and treatment effects

	ITT	Student FE	Instrumenting for the quantity of observations completed			
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment school	0.074*					
	(0.033)					
High dose department	-0.002	-0.002				
	(0.020)	(0.028)				
Completed observations per observee teacher			0.032*	0.073*		
			(0.014)	(0.035)		
Completed … teacher ^ 2				-0.008		
				(0.005)		
Fraction of suggested observations completed					0.425*	0.959*
					(0.182)	(0.471)
Fraction … completed ^ 2						-1.479
						(0.955)
Rand. block fixed effects	√		√	√	√	√
Student fixed effects		√				
Adjusted R-squared	0.343	0.684	0.342	0.342	0.343	0.344
Observations	56,148	56,148	56,148	56,148	56,148	56,148
First-stage F-statistic excl. instrument(s)			572.1	580.3	43.7	646.3
Linear and square term jointly zero, p-value				0.059		0.059
Linear and quadratic fit equally well						
likelihood-ratio χ^2			26.9		51.9	
p-value			0.000		0.000	

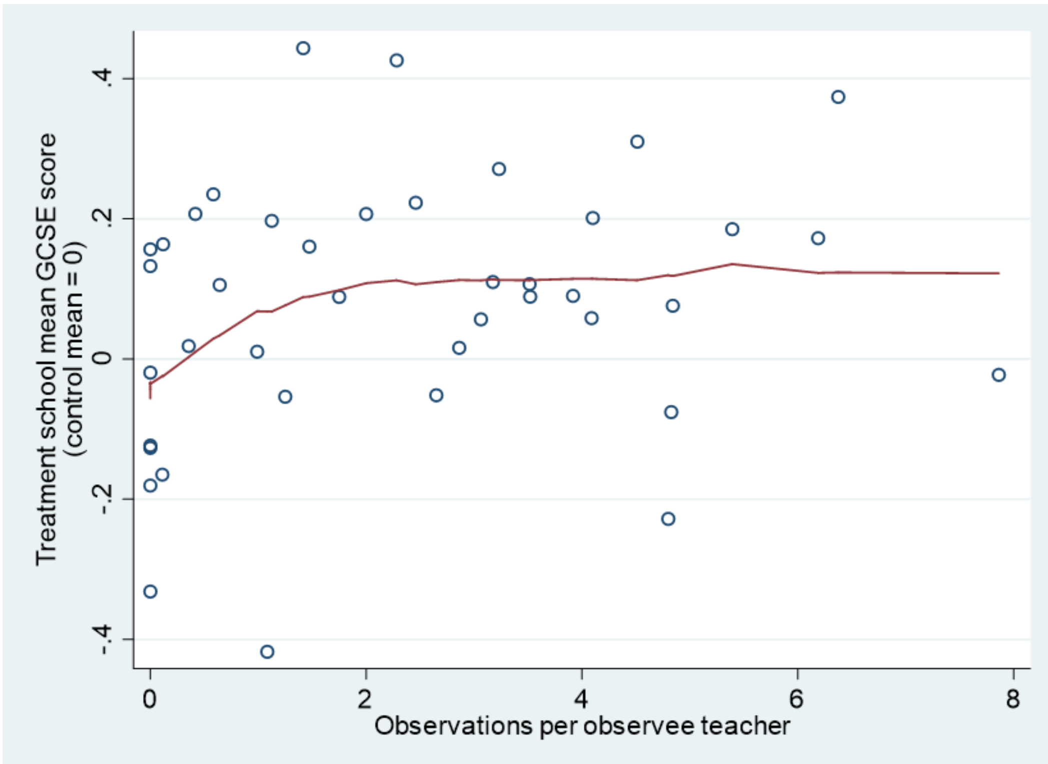
Note: Each column reports results from a separate least squares (columns 1-2) or two-stage least squares (columns 3-6) regression. Column 2 is limited to treatment observations. The dependent variable is student math or English GCSE score in student standard deviation units. All specifications, except column 2, include an indicator for math observation, an indicator for cohort 1, and the pre-treatment covariates listed in Table 1. When a pre-treatment covariate is missing, we replace it with zero and include an indicator variable = 1 for missing on the given characteristic. For the IV estimates in columns 3-4 the endogenous variable is the fraction (i) number of observations completed by the school before the end of the student's year 11, divided by (ii) number of observee teachers. For columns 5-6 the numerator is the same, but the denominator is the number of observations suggested by the program instructions. We instrument for the endogenous variables using the two randomly assigned indicators: school level treatment assignment, and high dose department assignment. Heteroskedasticity-cluster robust standard errors in parentheses, with clustering at the school level.
+ indicates p<0.10, * 0.05, and ** 0.01

Table 4—Effects by teacher role

Teacher type	Full sample: Student achievement relative to… all control school teachers (1)	(2)	Treatment schools with class rosters: Student achievement relative to… observer teachers (3)	(4)	(5)	(6)
All treatment school teachers	0.073*					
	(0.032)					
Randomly assigned						
Observer		0.150**				
		(0.043)				
Observee		0.118**	-0.039	-0.043	-0.113	-0.007
		(0.043)	(0.038)	(0.040)	(0.070)	(0.037)
			[0.034]	[0.034]	[0.056]*	[0.025]
Both roles		0.132**	-0.019	-0.017	-0.013	-0.006
		(0.039)	(0.028)	(0.028)	(0.052)	(0.038)
			[0.031]	[0.031]	[0.053]	[0.024]
Other non-participating teachers		0.046	-0.202			-0.082
		(0.046)	(0.045)**			(0.054)
			[0.038]**			[0.033]*
Treatment schools without class rosters		-0.038				
		(0.056)				
Rand. block fixed effects	√	√				
School fixed effects			√	√	√	
Student fixed effects						√
Pre-treatment covariates	√	√	√	√		
Adjusted *R*-squared	0.344	0.346	0.369	0.362	0.055	0.699
Observations	56,148	56,148	24,042	15,077	15,077	24,042

Note: Each column reports results from a separate least squares regression. The dependent variable is student math or English GCSE score in student standard deviation units. All specifications, except column 6, include an indicator for math observation, an indicator for cohort 1, and the “pre-treatment covariates” are listed in Table 1. When a pre-treatment covariate is missing, we replace it with zero and include an indicator variable = 1 for missing on the given characteristic. Heteroskedasticity-cluster robust standard errors (i) in parentheses for clustering at the school level, and (ii) in brackets for clustering at the teacher level.
+ indicates $p<0.10$, * 0.05, and ** 0.01

Online Appendix



Appendix Figure 1—School mean test scores and
the number of observations completed

Note: Each hollow circle represents one treatment school. The y-axis measures, for a given school, the school's mean student GCSE test score in standard deviation units, minus the overall control mean score. This is not, strictly speaking, a school specific treatment effect identified by random assignment. The x-axis is the number of observations completed per observee teacher. The line is a lowess fit using the 41 observations shown as hollow circles.

Appendix Table 1—Teacher role experiment baseline covariate summary measure

	(1)	(2)
Teacher type (omitted category: observer teacher)		
Randomly assigned to observee	-0.077*	-0.077*
	(0.037)	(0.037)
	[0.030]	[0.030]
Randomly assigned to both roles	0.003	0.005
	(0.034)	(0.034)
	[0.030]	[0.029]
Other non-participating teachers		-0.123**
		(0.039)
		[0.033]
Observations	15,077	24,042

Note: Each column reports results from a separate least squares regression. The dependent variable is a summary index of pre-treatment covariates, constructed as follows: Using only control school observations, we regress outcome test score (student math or English GCSE score in student standard deviation units) on all pre-treatment covariates; and then use the estimated coefficients calculate fitted values for the treatment schools. This fitted value is the summary index. In the regression reported in this table, we regress the summary index on the teacher role indicators and school fixed effects. The sample in column 2 is limited to the 33 treatment schools for which we have teacher-student class rosters. Heteroskedasticity-cluster robust standard errors (i) in parentheses for clustering at the school level, and (ii) in brackets for clustering at the teacher level. + indicates p<0.10, * 0.05, and ** 0.01

Appendix Table 2—First-stage results for Table 2

	Dep. var. = endogenous treatment in Table 2…		
	Col (3)	Col (4)	Col (5)
School randomly assigned to treatment	0.787**	0.550**	0.467**
	(0.062)	(0.065)	(0.078)
Adjusted *R*-squared	0.661	0.446	0.346

Note: Each column reports results from a separate least squares regression; each a first-stage regression associated with the 2SLS estimates in Table 2. Heteroskedasticity-cluster robust standard errors in parentheses, clustering at the school level.
+ indicates p<0.10, * 0.05, and ** 0.01

Appendix Table 3—ITT estimates for subsamples

	Subsample			
	Math scores	English scores	Grade 11 in 2014-15	Grade 11 in 2015-16
	(1)	(2)	(3)	(4)
School randomly assigned to treatment	0.044	0.102*	0.106**	0.037
	(0.031)	(0.040)	(0.036)	(0.034)
Observations	28,074	28,074	28,410	27,738
Adjusted R-squared	0.398	0.332	0.296	0.427

Note: Each column reports results from a separate least squares regression. Estimation is identical to Table 2 column 2, except that the estimation sample is limited to a subsample described in the column header. Heteroskedasticity-cluster robust standard errors in parentheses, clustering at the school level.

+ indicates p<0.10, * 0.05, and ** 0.01

Appendix Table 4—First-stage results for Table 3

	Dep. var. = endogenous treatment in Table 3			
	Completed observations per observee teacher		Fraction of suggested observations completed	
	linear	squared	linear	squared
Treatment school	1.603**	5.689**	0.123**	0.030**
	(0.254)	(1.794)	(0.019)	(0.008)
High dose department	1.334**	12.779**	0.100**	0.065**
	(0.310)	(3.200)	(0.023)	(0.016)
Adjusted R-squared	0.432	0.278	0.471	0.367

Note: Each column reports results from a separate least squares regression; each a first-stage regression associated with the 2SLS estimates in Table 3. Heteroskedasticity-cluster robust standard errors in parentheses, clustering at the school level.
+ indicates p<0.10, * 0.05, and ** 0.01

Appendix Table 5—Participation by teachers

Endogenous treatment:	(1)	(2)
(A) Indicator for participated in one or more observation * assigned role (omitted category: participated * observer)		
Participated * observee	-0.053	
	(0.049)	
	[0.042]	
Participated * both roles	-0.022	
	(0.033)	
	[0.037]	
(B) Number of times participated in an observation, regardless of actual role		0.007
		(0.004)
		[0.004]
Adjusted R-squared	0.36	0.358
Observations	15,077	15,077
First-stage F-statistic excl. instrument(s)		
School level clustering	573.1	36.3
Teacher level clustering	4842.4	670

Note: Each column reports results from a separate two stage least squares regression. The dependent variable is student math or English GCSE score in student standard deviation units. The endogenous treatment variable(s) are described in the row headers. In all specifications the excluded instruments include indicators for the teacher's randomly assigned role: observer (omitted), observee, or both; and the randomly assigned department dose indicator. All specifications include an indicator for math observation, the pre-treatment covariates listed in Table 1, and an indicator for cohort 1. When a pre-treatment covariate is missing, we replace it with zero and include an indicator variable = 1 for missing on the given characteristic. Heteroskedasticity-cluster robust standard errors (i) in parentheses for clustering at the school level, and (ii) in brackets for clustering at the teacher level.
+ indicates p<0.10, * 0.05, and ** 0.01

Appendix Table 6—First-stage results for Appendix Table 5

	Dep. var. = endogenous treatment		
	Participated * observee	Participated * both roles	Number of times participated
	(1)	(2)	(3)
Randomly assigned role (omitted category: observer)			
Observee	0.807	0.006	-0.534
	(0.049)**	(0.010)	(0.511)
	[0.033]**	[0.011]	[0.773]
Both roles	0.013	0.829	3.340
	(0.007)+	(0.045)**	(0.908)**
	[0.010]	[0.029]**	[0.868]**
Dept. assigned high dose	0.012	-0.029	6.666
	(0.027)	(0.028)	(1.410)**
	[0.020]	[0.018]	[0.802]**
Observations	15,077	15,077	15,077
Adjusted R-squared	0.775	0.795	0.430

Note: Each column reports results from a separate least squares regression; each a first-stage regression associated with the 2SLS estimates in Appendix Table 5. Heteroskedasticity-cluster robust standard errors (i) in parentheses for clustering at the school level, and (ii) in brackets for clustering at the teacher level.
+ indicates p<0.10, * 0.05, and ** 0.01

Appendix Table 7—Robustness to degree of imbalance

	Low imbalance	High imbalance
	(1)	(2)
Assigned role (Observer omitted)		
Observee	0.017	-0.11
	[0.034]	[0.07]
Both roles	-0.052	0.023
	[0.037]	[0.041]
Adjusted R-squared	0.336	0.39
Observations	7,582	7,371

Note: Each column reports results from a separate least squares regression. The estimation details are identical to Table 4 column 4, except that here each column is estimating using a subsample. The two subsamples relatively "low imbalance" and "high imbalance" are defined in the following way. First, for each student, we convert the available pre-treatment covariates into a scalar index measure. Using the control sample, we regress GCSE score on those covariates, and then calculate the fitted GCSE score for treatment cases. That fitted score is our index. Second, for each treatment school, we estimate the mean difference in that index between teachers randomly assigned to be observers and observees. We define relatively "low imbalance" schools as schools where the absolute fitted-score difference of less than 0.10σ, and "high imbalance" schools those with 0.10σ or greater. Heteroskedasticity-cluster robust standard errors (i) in parentheses for clustering at the school level, and (ii) in brackets for clustering at the teacher level.
+ indicates p<0.10, * 0.05, and ** 0.01

Appendix Table 8a—Effects by teacher role, additional results

	Full sample					
	Student achievement relative to...all control school teachers					
	Table 4 column 2	Cohort 1	Cohort 2			Weights
	(1)	(2)	(3)	(4)	(5)	(6)
Year 11 teacher						
Observer	0.150***	0.188***	0.098**		-0.113	0.138***
	(0.043)	(0.050)	(0.046)		(0.094)	(0.041)
Observee	0.118***	0.132**	0.098**		-0.14	0.127***
	(0.043)	(0.054)	(0.040)		(0.097)	(0.037)
Both roles	0.132***	0.138***	0.121**		-0.095	0.120***
	(0.039)	(0.052)	(0.049)		(0.098)	(0.037)
Non-participating	0.046	0.045	0.035		-0.169*	0.04
	(0.046)	(0.065)	(0.047)		(0.093)	(0.047)
Year 10 teacher						
Observer				0.068	0.205***	
				(0.046)	(0.076)	
Observee				0.126***	0.268***	
				(0.030)	(0.088)	
Both roles				0.082**	0.212**	
				(0.040)	(0.084)	
Non-participating				0.012	0.167*	
				(0.059)	(0.099)	
Treatment schools without class rosters	-0.038	0.045	-0.109*	-0.110*	-0.109*	-0.038
	(0.056)	(0.058)	(0.059)	(0.058)	(0.059)	(0.055)
Adjusted R-squared	0.346	0.298	0.431	0.431	0.431	0.346
Observations	56,148	28,410	27,738	27,738	27,738	56,148

Note: Each column reports results from a separate least squares regression. Estimation methods are identical to Table 4 column 2, which is repeated above for convenience, except as follows. Column 2 is estimated using only students who were year 11 in the first year of the experiment, “Cohort 1”, and the role of their year 11 teacher. Columns 3-5 are estimated using “Cohort 2” students who were year 11 in the second year of the experiment. Column 3 includes their year 11 teacher’s role, column 4 their year 10 teacher’s role, and column 5 both teachers simultaneously. In Column 6 we include both cohorts, then for “Cohort 2” students we include two observations, one for year 10 teacher and one for year 11, and weight by ½ each observation. Heteroskedasticity-cluster robust standard errors in parentheses for clustering at the school level
+ indicates p<0.10, * 0.05, and ** 0.01

Appendix Table 8b—Effects by teacher role, additional results

	Treatment schools with class rosters					
	Student achievement relative to...observer teachers					
	Table 4 column 4	Cohort 1	Cohort 2			Weights
	(1)	(2)	(3)	(4)	(5)	(6)
Year 11 teacher						
Observee	-0.043	-0.076	0.006		-0.045	-0.022
	(0.034)	(0.048)	(0.039)		(0.047)	(0.034)
Both roles	-0.017	-0.056	0.030		0.027	-0.012
	(0.031)	(0.048)	(0.034)		(0.040)	(0.032)
Year 10 teacher						
Observee				0.048	0.106**	
				(0.032)	(0.044)	
Both roles				0.010	0.018	
				(0.031)	(0.042)	
Adjusted R-squared	0.362	0.322	0.470	0.447	0.467	0.354
Observations	15,077	8,687	6,390	9,146	5,616	19,919

Note: See the note for Appendix Table 8a. Here the exercise is applied to Table 4 column 4, instead of column 2. Heteroskedasticity-cluster robust standard errors in parentheses for clustering at the school level
+ indicates p<0.10, * 0.05, and ** 0.01

DOMAIN 1: THE CLASSROOM ENVIRONMENT				
Component	**Ineffective (1-3)**	**Basic (4-6)**	**Effective (7-9)**	**Highly Effective (10-12)**
1a Creating an Environment of Respect and Rapport	Classroom interactions, both between the teacher and students and among students, are negative, inappropriate, or insensitive to students' cultural backgrounds, ages and developmental levels. Student interactions are characterised by sarcasm, put-downs, or conflict.	Classroom interactions, both between the teacher and students and among students, are generally appropriate and free from conflict, but may reflect occasional displays of insensitivity or lack of responsiveness to cultural or developmental differences among students.	Classroom interactions, both between teacher and students and among students, are polite and respectful, reflecting general warmth and caring, and are appropriate to the cultural and developmental differences among groups of students.	Classroom interactions, both between teacher and students and among students, are highly respectful, reflecting genuine warmth and caring and sensitivity to students' cultures and levels of development. Students themselves ensure high levels of civility among members of the class.
1b Establishing a Culture for Learning	The classroom environment conveys a negative culture for learning, characterised by low teacher commitment to the subject, low expectations for student achievement, and little or no student pride in work.	The teacher's attempts to create a culture for learning are partially successful, with little teacher commitment to the subject, modest expectations for student achievement, and little student pride in work. Both teacher and students appear to be only "going through the motions."	The classroom culture is characterised by high expectations for most students and genuine commitment to the subject by both teacher and students, with teacher demonstrating enthusiasm for the content and students demonstrating pride in their work.	High levels of student energy and teacher passion for the subject create a culture for learning in which everyone shares a belief in the importance of the subject and all students hold themselves to high standards of performance they have internalized.

DOMAIN 1: THE CLASSROOM ENVIRONMENT (cont.)				
Component	**Ineffective (1-3)**	**Basic (4-6)**	**Effective (7-9)**	**Highly Effective (10-12)**
1c Managing Classroom Procedures	Much teaching time is lost because of inefficient classroom routines and procedures for transitions, handling of supplies, and performance of non-teaching duties. Students not working with the teacher are not productively engaged in learning. Little evidence that students know or follow established routines.	Some teaching time is lost because classroom routines and procedures for transitions, handling of supplies, and performance of non-teaching duties are only partially effective. Students in some groups are productively engaged while unsupervised by the teacher.	Little teaching time is lost because of classroom routines and procedures for transitions, handling of supplies, and performance of non-teaching duties, which occur smoothly. Group work is well-organised and most students are productively engaged while working unsupervised.	Teaching time is maximised due to seamless and efficient classroom routines and procedures. Students contribute to the seamless operation of classroom routines and procedures for transitions, handling of supplies, and performance of non-instructional duties. Students in groups assume responsibility for productivity.
1d Managing Student Behaviour	There is no evidence that standards of conduct have been established, and there is little or no teacher monitoring of student behaviour. Response to student misbehaviour is repressive or disrespectful of student dignity.	It appears that the teacher has made an effort to establish standards of conduct for students. The teacher tries, with uneven results, to monitor student behaviour and respond to student misbehaviour.	Standards of conduct appear to be clear to students, and the teacher monitors student behaviour against those standards. The teacher response to student misbehaviour is consistent, proportionate, appropriate and respects the students' dignity.	Standards of conduct are clear, with evidence of student participation in setting them. The teacher's monitoring of student behaviour is subtle and preventive, and the teacher's response to student misbehaviour is sensitive to individual student needs and respects students' dignity. Students take an active role in monitoring the standards of behaviour.

DOMAIN 1: THE CLASSROOM ENVIRONMENT (cont.)				
Component	**Ineffective (1-3)**	**Basic (4-6)**	**Effective (7-9)**	**Highly Effective (10-12)**
1e Organising Physical Space	The physical environment is unsafe, or some students don’t have access to learning. There is poor alignment between the physical arrangement of furniture and resources and the lesson activities.	The classroom is safe, and essential learning is accessible to most students; the teacher’s use of physical resources, including computer technology, is moderately effective. The teacher may attempt to modify the physical arrangement to suit learning activities, with limited effectiveness.	The classroom is safe, and learning is accessible to all students; the teacher ensures that the physical arrangement is appropriate for the learning activities. The teacher makes effective use of physical resources, including computer technology.	The classroom is safe, and the physical environment ensures the learning of all students, including those with special needs. Students contribute to the use or adaptation of the physical environment to advance learning. Technology is used skilfully, as appropriate to the lesson.

DOMAIN 2: TEACHING				
Component	**Ineffective (1-3)**	**Basic (4-6)**	**Effective (7-9)**	**Highly Effective (10-12)**
2a Communicating with Students	Expectations for learning, directions and procedures, and explanations of content are unclear or confusing to students. The teacher's written or spoken language contains errors or is inappropriate for students' cultures or levels of development.	Expectations for learning, directions and procedures, and explanations of content are clarified after initial confusion; the teacher's written or spoken language is correct but may not be completely appropriate for students' cultures or levels of development.	Expectations for learning, directions and procedures, and explanations of content are clear to students. Communications are accurate as well as appropriate for students' cultures and levels of development. The teacher's explanation of content is scaffolded, clear, and accurate and connects with students' knowledge and experience. During the explanation of content, the teacher focuses, as appropriate, on strategies students can use when working independently and invites student intellectual engagement.	Expectations for learning, directions and procedures, and explanations of content are clear to students. The teacher links the instructional purpose of the lesson to the wider curriculum. The teacher's oral and written communication is clear and expressive, appropriate to students' cultures and levels of development, and anticipates possible student misconceptions. The teacher's explanation of content is thorough and clear, developing conceptual understanding through clear scaffolding and connecting with students' interests. Students contribute to extending the content by explaining concepts to their peers and suggesting strategies that might be used.

DOMAIN 2: TEACHING (cont.)				
Component	**Ineffective (1-3)**	**Basic (4-6)**	**Effective (7-9)**	**Highly Effective (10-12)**
2b Using Questioning and Discussion Techniques	The teacher's questions are of low cognitive challenge or inappropriate, eliciting limited student participation, and recitation rather than discussion. A few students dominate the discussion.	Some of the teacher's questions elicit a thoughtful response, but most are low-level, posed in rapid succession. The teacher's attempts to engage all students in the discussion are only partially successful.	Most of the teacher's questions elicit a thoughtful response, and the teacher allows sufficient time for students to answer. All students participate in the discussion, with the teacher stepping aside when appropriate.	Questions reflect high expectations and are culturally and developmentally appropriate. Students formulate many of the high-level questions and ensure that all voices are heard.
2c Engaging Students in Learning	Activities and assignments, materials, and groupings of students are inappropriate for the learning outcomes or students' cultures or levels of understanding, resulting in little intellectual engagement. The lesson has no clearly defined structure or is poorly paced.	Activities and assignments, materials, and groupings of students are partially appropriate for the learning outcomes or students' cultures or levels of understanding, resulting in moderate intellectual engagement. The lesson has a recognisable structure but is not fully maintained and is marked by inconsistent pacing.	Activities and assignments, materials, and groupings of students are fully appropriate for the learning outcomes and students' cultures and levels of understanding. All students are engaged in work of a high level of rigour. The lesson's structure is coherent, with appropriate pace.	Students, throughout the lesson, are highly intellectually engaged in significant learning and make material contributions to the activities, student groupings, and materials. The lesson is adapted as needed to the needs of individuals, and the structure and pacing allow for student reflection and closure.

DOMAIN 2: TEACHING (cont.)				
Component	**Ineffective (1-3)**	**Basic (4-6)**	**Effective (7-9)**	**Highly Effective (10-12)**
2d Use of Assessment	Assessment is not used in teaching, either through monitoring of progress by the teacher or students, or adequate feedback to students. Students are not aware of the assessment criteria used to evaluate their work, nor do they engage in self- or peer-assessment. .	Assessment is occasionally used in teaching, through some monitoring of progress of learning by the teacher and/or students. Feedback to students is uneven, and students are aware of only some of the assessment criteria used to evaluate their work. Students occasionally assess their own or their peers' work.	Assessment is regularly used in teaching, through self- or peer-assessment by students, monitoring of progress of learning by the teacher and/or students, and high-quality feedback to students. Students are fully aware of the assessment criteria used to evaluate their work and frequently do so.	Assessment is used in a sophisticated manner in teaching, through student involvement in establishing the assessment criteria, self-or peer assessment by students, monitoring of progress by both students and the teacher, and high-quality feedback to students from a variety of sources. Students use self-assessment and monitoring to direct their own learning.
2e Demonstrating Flexibility and Responsiveness	The teacher adheres to the lesson plan, even when a change would improve the lesson or address students' lack of interest. The teacher brushes aside student questions; when students experience difficulty, the teacher blames the students or their home environment.	The teacher attempts to modify the lesson when needed and to respond to student questions, with moderate success. The teacher accepts responsibility for student success but has only a limited repertoire of strategies to draw upon.	The teacher promotes the successful learning of all students, making adjustments as needed to lesson plans and accommodating student questions, needs, and interests.	The teacher seizes an opportunity to enhance learning, building on a spontaneous event or student interests, or successfully adjusts and differentiates instruction to address individual student misunderstandings. The teacher ensures the success of all students by using an extensive repertoire of teaching strategies and soliciting additional resources from the school or community. .