Supporting Information

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The question we sought to answer was, "Does active learning work better, in terms of improving student performance in undergraduate science, technology, engineering, and mathematics (STEM) courses, than traditional lecturing?" We evaluated performance using two metrics: (*i*) scores on identical or formally equivalent examinations, concept inventories, or other assessments, and (*ii*) failure rates—in most cases measured as the percentage of Ds, Fs, and/or withdrawals. These were relevant criteria for failure because students with a D, F, or W in a STEM course are usually barred from receiving credit in the major.

SI Materials and Methods

Literature Search. Searching the unpublished or gray literature in addition to peer-reviewed papers is recommended to mitigate the file-drawer effect, i.e., a systematic bias against studies that may have been conducted carefully but were not published because the results failed to show statistical significance and/or had low effect sizes (1, 2).

We had no starting time limit for admission to the study; the ending cutoff for consideration was completion or publication before January 1, 2010. We used four approaches (3) to find papers for consideration:

i) Hand searching: We read the titles of every research paper published between June 1, 1998 and January 1, 2010 in the 55 journals listed in Table S4. If the titles indicated that one of the five criteria for admission might be met, we also read the abstract or summary. Papers that appeared to fulfill most or all of the criteria were downloaded for later coding.

ii) Database searches: We searched Web of Science, Expanded Academic Index, PubMed, Education Resources Information Center, Compendex, ProQuest Science, and ProQuest Dissertations and Theses. Each database was queried using the following terms: active learning, audience response system, case-based learning, clickers, collaborative learning, constructivism or constructivist learning, cooperative learning, peer instruction, peer teaching, personal response device, problem-based learning, reform calculus or calculus reform, studio physics, traditional lecturing, workshop calculus, and workshop physics. To reduce the number of irrelevant hits, these terms were modified by "and student" and/or "and lecture" in certain databases, and/or limited by selecting the modifiers "education," "math," or "college students."

iii) Mining reviews and bibliographies: We collected 33 bibliographies, qualitative reviews, and metaanalyses on undergraduate STEM education and searched their citation lists for papers relevant to this study (4–36).

iv) Snowballing: We searched the citation lists of all of the publications that we coded and checked for papers relevant to this study.

We did not contact individual researchers for unpublished work, and we did not include conference proceedings that included only abstracts.

Criteria for Admission. Five criteria for admission to the study—for coding by at least one researcher—are listed in *Materials and Methods, Criteria for Admission*. These criteria were designed to exclude:

- Experiments with volunteers or paid participants that occurred outside of a normal course.
- Studies on the impact of laboratory exercises, homework, and other assignments; take-home quizzes, supplementary instruction sessions; or other activities that were meant to be completed outside of normal class periods.
- Studies of student affect.

Coding. There were 244 studies classified as easy rejects and excluded from the study after the initial coder (S.F.) determined that they clearly did not meet one or more of the five criteria for admission. Of these, 23 were assigned to one of the second coders. In 22 of 23 cases (96%), the second coder independently evaluated them as easy rejects; in the one case of conflict, a discussion between the two coders resulted in rejection from the study. Thus, it does not appear that a significant number of the 244 easy rejects would actually be admitted if they were subjected to a second, independent coding.

The two coders met to discuss each of the remaining 398 papers (37, 38). The goal was to review and reach consensus on the five issues itemized in *Materials and Methods*, *Criteria for Admission*. On the issue of examination equivalence, it is important to note that we rejected studies where authors either provided no data on examination equivalence or stated that examinations were similar or comparable without providing data to back the claim.

Coding: Student equivalence. To evaluate student equivalence, coders checked whether the experimental design was based on randomization or quasirandomization among treatments. In quasirandomized designs, students register for lecturing or active learning sections blind to the treatment type. However, students may differ among sections or between years in terms of their ability and/or preparation (39, 40). If the experiment was quasirandom, coders noted whether the authors included statistical tests on data concerning prior academic performance—usually college grade point average (GPA) at the time of entering the course, Scholastic Aptitude Test (SAT) or American College Testing (ACT) scores, or pretests directly relevant to the topic in question. Pretests were often a concept inventory; if prescores were statistically indistinguishable, authors frequently reported only postscores. In many cases, we used these postscores to compute an effect size. Note that if a statistical test indicated that students in the lecture section performed significantly worse in terms of the preparation/ability metric analyzed, the study was rejected from the metaanalysis.

This coding system allowed us to create four categories for the quality of studies, in terms of controlling for student equivalence in the active learning and lecture treatments. They are, in order from least well controlled to best controlled, as follows:

- Quasirandom studies where authors provided no data on student equivalence in terms of academic performance. Studies were assigned to this category even if authors claimed that students were indistinguishable but failed to provide a relevant statistical test, or if they provided data showing that students were equivalent in terms of sex, ethnicity, prior courses taken, major field of study, or similar non–performance-related criteria.
- Quasirandom studies where data indicated no statistical difference on a pretest that was directly related to the topic covered in the course and/or to the assessment used to calculate the effect size.
- Quasirandom studies where data indicated no statistical difference on a reliable metric of overall academic preparedness/ability, or where the metric was used as a covariate in the data analysis. These data were usually SAT scores, ACT scores, or college GPA. We did not accept high school GPA to qualify studies for this category.

- Randomized studies, where students were assigned at random (e.g., by the registrar's office) to the two classes being compared, and cross-over designs where the same students experienced both active learning and traditional lecturing in the same course.

Note that in the results on student equivalence given in Table 1*A*, the total sample size is 157 instead of 158. This is because one study had combined outcomes that we used to calculate the effect size (see *Data analysis: Multiple controls/treatments and multiple outcomes*), and because the student controls on the two outcomes used were different. As a result, this study could not be assigned to one of the four categories and had to be dropped from this heterogeneity analysis.

Coding: Identifying studies that were independent, as the unit of analysis. During the coding phase, we took several steps to eliminate pseudoreplication, which can artificially inflate sample sizes in metaanalyses. The coding protocol called for computing a single effect size from each published study, except in instances where a single paper reported the results of studies that were conducted in independent courses and populations (41). The exceptions occurred when experiments reported in the same paper were on the same campus but involved different courses with different student populations (e.g., business calculus versus mathematics for preservice teachers), or courses on different campuses (i.e., that the instructors and students involved did not overlap). In cases where a single paper reported results from multiple courses that may have been taken in sequence by at least some of the same students, we recorded effect size data from only the initial course in the sequence. Note that both coders had to agree that the study populations were completely independent for multiple effect sizes to be reported from the same paper.

Coding: Multiple controls/treatments and multiple outcomes. In cases where papers included data from multiple treatments or multiple controls—from the same course on the same campus, with or without the same instructor(s)—we averaged the replicates to compute a single effect size, using the approach described in *Data analysis: Combining multiple controls and/or treatments* and *Data analysis: Combining multiple outcomes*.

In cases where papers reported multiple outcome variables, the protocol called for coders to choose the assessment that was (*i*) most summative—for example, comprehensive final examinations over midterm examinations or the percentage of students receiving a D or F grade or withdrawing from the course in question (DFW rate) instead of percentage of withdrawals, and/or (*ii*) most comparable to other studies—for example, a widely used concept inventory versus an instructor-written examination. In cases where the assessments were determined to be equivalent—e.g., four hour-long examinations, over the course of a semester—the coders recorded all of the relevant data. We also combined outcomes in cases where summative examination scores (e.g., from a comprehensive final) as well as a concept inventory scores were reported. When coding was complete, we combined data from multiple outcomes to produce a single effect size, using the approaches described in *Data analysis: Combining multiple outcomes*.

Coding: Missing data. A total of 91 papers were judged to be admissible by both coders but lacked some or all of the data required to compute an effect size. In most cases, the missing data were SDs and/or sample sizes. We wrote to every author on all 91 papers for whom we could find an e-mail address, requesting the missing data. If we did not receive a response within 4 wk, we dropped the paper from the study. Via follow-up correspondence, we were able to obtain missing data for 19 of the 91 studies.

Coding: Categorizing class sizes. We coded class size as a continuous variable in all papers where it was mentioned. In cases where the authors reported a range of class sizes, we used the midpoint value in the range. To create class size categories, we made a histogram of the class size distribution, after coding was complete. Our interpretation of this distribution, shown in Fig. S4, was that class sizes fell into three natural groupings: classes with 50 or fewer students, classes of more than 50 up to 110, and classes with more than 110. We designated these as small, medium, and large classes, respectively.

Data Analysis. Data on examination or concept inventory performance were continuous and were reported either as means, SDs, and sample sizes for each treatment group, or as the value and df of Student's *t*. We used these data to compute an effect size from each study as the standardized mean difference weighted by the inverse of the pooled variance, with the correction for clustering in quasirandom studies explained in the *Materials and Methods, Data Analysis*, and with Hedges' correction for small sample sizes (42). Thus, effect sizes for examination-points data are in units of SDs. Effect size calculations, heterogeneity analyses, and publication bias analyses were done in the Comprehensive Meta-Analysis software package; the cluster correction was done in a Microsoft Excel program based on methods developed by Hedges (see *Materials and Methods, Data Analysis*).

Data on failure rates are dichotomous and were reported as the percentage of withdrawals/drops, Ds, Fs, Ds and Fs, DFWs, or students progressing to subsequent courses. We used the odds ratio to compute an effect size for failure rates (43).

It is important to note that papers reporting anything other than DFW—for example, only Ds and Fs or only withdrawals—may underreport the actual overall "raw" failure rate, as it is typical for D and F grades as well as withdrawals to prevent students from getting course credit toward a STEM major and progressing to subsequent courses. However, because experiments admitted to the study had to have the same metric for failing a traditional lecture section and an active learning section, the contrast in failure rate should be consistent across metrics. As a result, the model-based estimate of the change in percentage of students failing should accurately capture the differences observed across different metrics of failure.

The independent studies analyzed in the experiment and the categories used in heterogeneity analyses are identified in Table S4, along with the effect size and associated data from each study analyzed here. Forest plots showing the effect sizes from all of the experiments in the study, organized by discipline, are provided in Fig. S2 for data on examination scores, concept inventories, and other assessments, and in Fig. S3 for failure rate data. Note that due to small sample sizes or in keeping with common departmental organizations, we combined astronomy with physics, statistics with mathematics, and natural resources/nutrition with biology.

Data analysis: Combining multiple controls and/or treatments. Some studies reported multiple control and/or experimental treatments for the same course, meaning that data were reported from more than one section within or across terms/years. In these instances we combined groups (e.g., all of the control terms) to create a single pair-wise comparison (38). For achievement data this involved creating a pooled mean, sample size, and SD. For the dichotomous data on failure rates, we simply summed the sample size and the number of students in each category to create a single group.

Data analysis: Combining multiple outcomes. When studies had multiple outcomes from the same set of students, and when the outcomes were equivalent—in terms of how summative they were or how widespread the use of the assessment was—we computed a summary effect size that combined all of the equivalent outcomes (44). In doing so, we assumed that the correlation between different outcomes within a comparison was 1, because the same students were sampled for each outcome. This is a conservative measure, leading to a less precise estimate of the summary effect, as the actual correlation between outcomes is likely lower than 1.

Because we combined multiple outcomes when they were judged to be equivalent in terms of measuring student-learning gains, each independent study was represented by one effect size in the metaanalysis; 28% of the studies analyzed here had multiple outcomes that we combined to estimate a single effect size.

Discussion/Data Interpretation. To estimate how much average course grades would change if examination scores increased by 0.47 SDs in a STEM course, we obtained data on SDs for examinations in the courses in the introductory biology, chemistry, and physics sequences for majors at the University of Washington (UW). We then calculated how many more total examination points students would receive, on average, with an increase of 0.47 SDs, and compared this increase to the number of points needed to raise a final grade by 0.1 on the 4-point, decimal-based system used at this institution. Average or typical SDs on 100-point examinations were 12.5 in biology, 17.5 in chemistry, and 18 in physics. An increase of 0.47 SDs would raise total examination points by about 23 in biology, 28.7 in chemistry, and 51 in physics, and raise final grades an average of 0.30 in biology, 0.28 in chemistry, and 0.34 in physics. In a letter-based grading system, these increases would move course medians or averages from a B− to a B in biology and chemistry, and from a B to a B+ in physics.

Our estimate of US$3,500,000 in lost tuition and course fees was based on an analysis of income from introductory biology courses at UW. Tuition and fees at this institution are about average for its peer comparison group of 10 other large US public universities. At UW, each course in the introductory biology sequence represents one-third of a normal course load in a term. Based on tuition rates per credit hour for the 2012–2013 academic year, published percentages of students receiving full financial aid and students paying in-state versus out-of-state tuition, we estimated that an average student pays $1,000 in tuition and fees per five-credit STEM course. The resulting estimated total of US $5,000,000 in lost tuition and fees due to failure would be much higher if all of the studies analyzed here included DFWs, or if the estimate were based on tuition at private institutions. It would be lower, however, if it were based on courses that lack laboratory sessions and/or are awarded fewer credit hours.

Additional Results. The data on failure rate reported in Fig. 4 can also be visualized, on a study-by-study basis, as shown in Fig. S1.

Assessing publication bias. Publication bias may be the most serious threat to the validity of a metaanalysis. Specifically, it is possible for a metaanalysis to report an inflated value for the overall effect size because the literature search failed to discover studies with low effect sizes (41). Several analyses support the hypothesis that the so-called file-drawer effect is real: Studies with low effect sizes or that failed to show statistical significance are less likely to be published, and thus substantially harder for the metaanalyst to find (2).

We performed two analyses to evaluate the impact of publication bias on this study: (*i*) assessing funnel plots and (*ii*) calculating a fail-safe *N*.

Assessing funnel plots. Plots of effect sizes versus SE (or sample size) are called funnel plots because in the absence of publication bias, they should be symmetrical about the overall mean and flare out at the bottom. The flare is due to increased variation in effect size estimates in studies with a large SE (or small sample size). Publication bias produces a marked asymmetry in a funnel plot—a "bite" out of the lower left of the plot (45). Visual inspection of the funnel plots for assessment and failure rate data indicates some asymmetry in the data for scores, due to five data points with unusually large effect sizes and/or SEs (Fig. S5*A*), but little-to-no asymmetry in the data for failure rate (Fig. S5*B*).

Statistical analyses (46) are consistent with this conclusion—indicating asymmetry in the funnel plot for assessment data but no asymmetry in the funnel plot for failure rate data. Specifically, both nonparametric and parametric tests indicate a statistically significant association between effect size and SE for the examination score data (Kendall's Tau with continuity correction 0.11, one-tailed $P = 0.02$; Egger's regression intercept 0.55, one-tailed $P = 0.00032$) but no association for the failure rate data (Kendall's Tau with continuity correction 0.14, one-tailed $P = 0.10$; Egger's regression intercept −0.43, one-tailed $P = 0.37$).

However, Duval and Tweedie's trim and fill method (46) for adjusting for publication bias indicates that the degree of asymmetry observed in this study has virtually no impact on the estimates of mean overall effect size: Under trim and fill, the random effects estimate for the standardized mean difference is 0.47 (95% confidence interval 0.37–0.56) for the assessment data and an odds ratio of 1.94 (95% confidence interval 1.71–2.20) for the failure rate data. A sensitivity analysis focusing on extreme values for examination scores also failed to discern an effect (see *Other sensitivity analyses*). Thus, there is no indication that publication bias has affected the effect size estimates reported here.

***Calculating a fail-safe* N.** The fail-safe *N* is the number of studies with effect size 0 that would have to be added to the study to reduce the observed effect size to a predetermined value that experts would consider inconsequential. Conservatively, we set the inconsequential value to a standardized mean difference of 0.20—still considered a pedagogically significant effect size in the K–12 literature (47)—for the examination score data and an odds ratio of 1.1 for the failure rate data. Orwin's fail-safe *N* is 114 for the assessment data and 438 for the failure rate data.

Like the trim and fill analyses, the large fail-safe *N*s suggest that the effect sizes reported here are not inflated by publication bias. To bring the effect sizes reported here down to values that might be insignificant to students and instructors, there would have to be an unreasonably large number of undetected studies with 0 effect.

Other sensitivity analyses: Assessing the influence of extreme values. The funnel plots of examination and concept inventory data suggest that there are several studies with extreme effect size values. To quantify this impression, we calculated the lower fence and upper fence for effect size values in the study, based on the interquartile range. The interquartile range was 0.59, producing a lower fence of −0.76 and an upper fence of 1.60. The two studies below the lower fence and the 10 studies above the upper fence can be considered outlying values. To evaluate the impact of these outliers on the metaanalysis, we recalculated the overall effect size with the 12 studies removed and found that it was 0.45, with a 95% confidence interval bounded by 0.38 and 0.51, meaning that it was statistically indistinguishable from the value computed with the outliers.

To investigate the outlying values further, we analyzed the characteristics of each study that generated an extreme value. Both of the outliers with low effect sizes were from studies that tested problem-based learning (PBL) as an active learning technique but involved extremely small sample sizes (totals of 16 and 25 students). The outliers with large effect sizes tested PBL, flipped classrooms with in-class activities, studio models, peer instruction, or vaguely defined interactive engagement with large numbers of students participating. One unifying feature of these high-effect-size studies appeared to be the high intensity of the active learning component. The percentage of class time devoted to active learning versus lecturing was reported as 25 (one study), 33 (one study), and 100 (seven studies), with one study missing data. This observation suggests that varying the intensity of active learning may be a productive experimental design for second-generation, discipline-based education research.

To evaluate the failure-rate data's sensitivity to values from individual studies, we ran a one-study-removed analysis—meaning that we recalculated the overall effect size, as the log-odds, with each of the 67 studies removed. In the 67 recalculations, the overall log-odds ranged from 1.49 to 1.60 with a mean of 1.55, suggesting that no one study had a disproportionate impact on the overall estimate.

Heterogeneity analyses: Assessment data. Table S1 *A–C* provides details on the heterogeneity analyses summarized in the *Results*–specifically, metaanalyzing the assessment data by discipline, assessment type (concept inventories versus examinations), and class size.

In addition, we evaluated how coders characterized the active learning interventions in the metaanalysis, and found that there was no heterogeneity in effect sizes for examination data based on the type of active learning intervention used ($Q = 11.173$, df = 7, $P = 0.131$).

For several reasons, however, we urge further work on the question of heterogeneity in effect sizes based on the type of active learning intervention. The sample sizes in our analysis are highly unbalanced, with some types of interventions (e.g., interactive demonstrations and case histories) poorly represented. In addition, a large suite of experiments was coded simply as "worksheets" because authors referred to "cooperative in-class exercises," "group problem solving," or "in-class tutorials." Coding was imprecise in these and many other cases because at present there is no consensus about what various active learning types actually entail in practice. For example, papers routinely use the term "problem-based learning" for what one author considers eight distinct course designs (48). Similar issues can arise in implementation of general strategies such as "cooperative group learning." The problem was exacerbated because authors rarely provided details on the intensity, duration, questioning level, group composition, and tasks involved in active learning interventions. Further, few studies noted whether assigned exercises had direct consequences for students—specifically, whether they were ungraded or graded, graded for participation or correctness, and for what percentage of total course grade.

We include the data on heterogeneity by intervention type here only to urge further work on the issue, as discipline-based education research begins its second generation. Progress will depend on the research community creating an objective and repeatable classification of intervention type—preferably grounded in theory and empirical work from cognitive science and educational psychology. To understand heterogeneity in effect sizes, we need a reliable taxonomy of active learning types.

Heterogeneity analyses: Failure rate data. Table S2*A* provides details on the heterogeneity analysis for failure rate data summarized in the *Results*–specifically, metaanalyzing the failure rate data by discipline. In addition, we metaanalyzed the failure rate data by the type of failure metric used, class size, and course level.

There was evidence for heterogeneity based on the type of failure metric used ($Q = 27.60$, df = 7, $P < 0.0001$; Table S2*B*), although the result should be interpreted cautiously because five of the categories are represented by tiny samples. Because it is the most comprehensive and interpretable metric of failure, we urge future investigators to use DFW as the sole metric for quantifying failure rates in STEM courses. Given these caveats, it is interesting to note that the analysis (Table S2*B*) indicates that the log-odds of students withdrawing from a course are much higher under lecturing. If these students were performing poorly before withdrawing but would be retained in active learning sections, it suggests that the overall effect size of 0.47 on assessment performance may be conservative, as noted in the *Discussion*.

The analysis based on class size just misses statistical significance ($Q = 5.91$, df = 2, $P = 0.052$). The data in Table S2*B* are consistent with the result for the examination scores (assessment) data. Active learning appears to work slightly better in classes with 50 or fewer students, but is effective in lowering failure rates in any class size. As with the assessment data, there is no impact on failure rate based on using active learning in introductory versus upper division classes ($Q = 0.71$, df = 1, $P = 0.40$).

The effect sizes and moderator variables for each independent study analyzed here are provided in Table S4 (49–234).

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Fig. S1. Changes in failure rate. Data points on failure rates under traditional lecturing and active learning from the same study—and thus from the same course and comparable student populations—are connected by a line. The overall means for each treatment are indicated by the green bars.

Fig. S1

Fig. S2. Forest plots for data on examinations, concept inventories, and other assessments, organized by discipline. Horizontal lines indicate 95% confidence intervals; detailed data are given in Table S4*A*.

Fig. S2

Fig. S3. Forest plots for data on failure rates, organized by discipline. The dashed, red, vertical line indicates the odds ratio indicating no effect. Horizontal lines indicate 95% confidence intervals; detailed data are given in Table S4*B*.

Fig. S3

Fig. S4. Distribution of class sizes in the study.

Fig. S4

Fig. S5. Funnel plots for evaluating publication bias. (*A*) Funnel plot for examination score data (n = 160). (*B*) Funnel plot for failure rate data (n = 67). Note that the log odds ratio is plotted here.

Fig. S5

Other Supporting Information Files

Table S1 (DOCX)
Table S2 (DOCX)
Table S3 (DOCX)
Table S4 (DOCX)