



DEPARTMENT OF BIOLOGICAL SCIENCES

To: Dr. Maria Calzada, Dean of the College of Arts & Sciences  
From: Dr. Don Hauber, Chair  
Date: August 11, 2016  
Subj.: BioInquiry Course Proposal Appeal

On May 3, 2016 the Department of Biological Sciences voted to appeal the decision of the College Curriculum Committee to reject the BioInquiry course proposal – this after the committee considered a revised resubmission of the proposal. The key point of contention is that the course would be designated as a core course required of all biology majors. The committee insisted that the course be listed as an elective since they felt it was primarily for the less prepared students. Our department argues that this course is designed as a leader course for all biology majors. As identified in our proposal, this curriculum shift is a national trend and is supported empirically. Dr. Christopher Barney (Hope College) who was one of our external reviewers on our 5-year department review in 2013 commented similarly expressing dismay that the curriculum committee members failed to see the value of this course in preparing all students (see Appendix). Similar sentiments were expressed by Dr. Ellen Goldey (Florida Atlantic) who came to our campus in 2014 to present her research in classroom reform. What they both emphasize, and the main justification for our proposal which is derived from the Vision & Change document (AAAS, 2011) is expressed in the following passage that reflects on the impact of Vision & Change (McLaughlin & Metz, 2016):

It is worth noting that this revolution, this paradigm shift, is now happening across all levels of science (and biology) education. The Next Generation Science Standards (NGSS Lead States, 2013) move K–12 students from content regurgitation toward using evidence to support scientific reasoning, and toward solving societal problems (NRC, 2012; NGSS Lead States, 2013; Pruitt, 2014). Advanced Placement (AP) tests underwent sweeping changes starting in 2012 – again away from rote memorization of facts to critical thinking and application of knowledge (Drew, 2011). The Graduate Record Examination (GRE) test was significantly revised in 2011 to emphasize critical thinking, verbal comprehension, and analytical skills over rote memorization. And the American Association of Medical Colleges (AAMC), driven by concerns about the ability of STEM curricula to keep pace with the accelerating rate of scientific discovery (AAMC, 2009) has recently implemented “MCAT 2015” – a medical-school admissions test that emphasizes deep knowledge of critical scientific concepts, scientific reasoning, data analysis, and problem solving (AAMC, 2014). A common thread throughout all of these movements is a shift away from fact acquisition and toward problem solving and critical thinking, thus mimicking V&C.

The point here is that, rather than focusing heavily on content, the emphasis of the new testing is on scientific reasoning, data analysis, critical thinking, and problem solving – all learning outcomes in BioInquiry. In this leader course, students gain a working knowledge of core concepts in biology (such as evolution and information flow) and this content is applied in collaborative research projects and assignments that foster critical thinking and problem solving skills. This strategy provides a foundation that emphasizes scientific skills as well as content to be extended and utilized in subsequent courses.

Regarding retention, we compared the number of Biology majors remaining in the major after their first year – in other words, fall of their first year to fall of the following year. We wanted to see if the first

iteration of **BioInquiry\*** made any difference on retention in the major by comparing to the two prior years. The table below reports on these results:

Year	Started FY fall as Biol	Started SY fall as Biol	Percent retained in major
Fall 2013 Biol. First-Yr	54	26	48%
Fall 2014 Biol. First-Yr	49	23	46.9%
Fall 2015 Biol. First-Yr*	53	30	56%

While it is encouraging that retention has increased following the pilot run of BioInquiry, it is difficult to draw conclusions. We will continue to monitor this trend and provide the necessary support for incoming biology majors to succeed. My cursory examination of overall retention (students remaining at Loyola) for the FY Biology majors seems to have remained steady (~77%) for 2013-15, suggesting that BioInquiry has not yet had an impact on *overall* retention. However, since the 2015 cohort of students is likely better prepared for the core science classes they will take in their second year, we predict that BioInquiry may impact overall retention as students advance through the major.

#### Cited

American Association for the Advancement of Science. 2011. Vision and change in undergraduate biology education: a call to action. Washington, DC.

McLaughlin, J. and A. Metz. 2016. Vision & Change: Why it Matters. American Biology Teacher 78:456-462.



August 1, 2016

Dr. Donald Hauber  
Professor and Chair  
Department of Biological Sciences  
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Dear Dr. Hauber:

I hope this letter finds you and your colleagues doing well.

It was very nice to receive your e-mail and to learn of the many exciting developments in biology at Loyola University. As you might expect, I was pleased to see that your department found the review report that Dr. Ribble and I submitted useful and that the recommendations in the report were taken seriously. I applaud the significant amount of time, attention, and thinking the department has devoted to its curriculum and to designing ways in which to make the major in biology even more effective in training future teachers, conservationists, researchers, physicians, and others for whom an excellent background in biology is an imperative. Your work over the past three years is helping to much better match your curriculum, courses, and assessment activities to your department's mission statement than was true at the time of our visit. It was gratifying to also learn you are enjoying your new spaces in Monroe Hall, which were under construction during our visit.

On the other hand, I was surprised and disappointed to learn that the Loyola University Curriculum Committee did not approve the establishment of the department's proposed Introduction to Biological Inquiry course and its attendant modifications in your curriculum for the first two years of the biology major. It is evident from reading the course proposal you sent me that the proposal was very well informed by national reports on college-level biology education and published scholarship on STEM teaching and first-year experiences. In my view, your proposed introductory course incorporates many of the suggestions for instructional improvement found in those reports and is a much-needed response to the changing face of biology training in the United States. Although the concerns expressed by your curriculum committee should be considered seriously and addressed (as you are already doing), I believe those concerns are inappropriately outweighing the many educational (and retention) benefits that your new course promises to deliver.

More specifically, your course is aimed at students at the beginning of their potential biology majors, a critical time for students to learn about the excitement, rewards, and challenges of being a professional biologist or person whose career depends on a thorough understanding of biology. Your emphasis on helping the students better understand how biology works, while

improving critical thinking, inquiry, interpersonal, and communication skills is a combination that should serve your students and your program very well. At Hope, we have shifted the laboratories that accompany our two introductory courses to inquiry and project based experiences such that students carry out six multiweek projects during the academic year rather than weekly “labs”. In addition, we have initiated an alternative to the labs for interested students, which focuses for an entire year on a one project, the isolation and characterization of bacteriophages. This approach has been so successful that the college has recently initiated two other projects for first-year students as part of our Day<sub>1</sub> project. Your proposed course should accomplish many of the same goals as our bacteriophage courses.

I think the concerns of your curriculum committee that the Introduction to Biological Inquiry course will place your premedical students at a disadvantage are misplaced. Medical schools are moving away from a prescribed list of prerequisite courses and are moving to the demonstration of competencies (see Scientific Foundations for Future Physicians (Association of American Medical Colleges, 2009)) and the MCAT has changed to focus on testing these competencies (see attachment). Your new introductory course seems to be well suited for improvements in these important abilities for your pre-medical students. In addition, I do not understand why your curriculum committee decided that Introduction to BioInquiry was a remedial course. Although part of your rationale was to improve the educational experience for all your entering students and thus help less well prepared students succeed at a higher level, the course appears to me to be focused on the skills and habits of thinking that all biology majors need. The course is unlike courses the students will have had in high school and better reflects the actual working life of most biologists. In this regard, if there is a concern about having too many introductory courses for the most capable students (as indicated by high scores on the AP biology exam), I would suggest that those students be allowed to replace the Cells and Genetics course (subjects on which many high school AP courses focus) with the AP credit rather than your proposed introductory course. The learning that will occur in that course is equally, if not more, important for your best students as it is for students with less aptitude and/or preparation for college level biology. In addition, although it would cause a loss of the modeling and mentoring your better prepared students could offer your less prepared students, you might consider offering one of the sections of Introduction to BioInquiry as an honors section, with an increase in performance or activity expectations.

Finally, I think your assessment plan for the course is very good and using a SALG instrument should help you understand if the course is meeting the goals you have designed for it.

I wish you well as you continue your efforts to bring this course to fruition. Please express my greetings and appreciation to your faculty and staff colleagues.

Sincerely yours,

A handwritten signature in black ink that reads "Christopher C. Barney". The signature is written in a cursive, flowing style.

Christopher C. Barney  
T. Elliot Weier Professor of Biology

# Active learning increases student performance in science, engineering, and mathematics

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Edited\* by Bruce Alberts, University of California, San Francisco, CA, and approved April 15, 2014 (received for review October 8, 2013)

To test the hypothesis that lecturing maximizes learning and course performance, we metaanalyzed 225 studies that reported data on examination scores or failure rates when comparing student performance in undergraduate science, technology, engineering, and mathematics (STEM) courses under traditional lecturing versus active learning. The effect sizes indicate that on average, student performance on examinations and concept inventories increased by 0.47 SDs under active learning ( $n = 158$  studies), and that the odds ratio for failing was 1.95 under traditional lecturing ( $n = 67$  studies). These results indicate that average examination scores improved by about 6% in active learning sections, and that students in classes with traditional lecturing were 1.5 times more likely to fail than were students in classes with active learning. Heterogeneity analyses indicated that both results hold across the STEM disciplines, that active learning increases scores on concept inventories more than on course examinations, and that active learning appears effective across all class sizes—although the greatest effects are in small ( $n \leq 50$ ) classes. Trim and fill analyses and fail-safe  $n$  calculations suggest that the results are not due to publication bias. The results also appear robust to variation in the methodological rigor of the included studies, based on the quality of controls over student quality and instructor identity. This is the largest and most comprehensive metaanalysis of undergraduate STEM education published to date. The results raise questions about the continued use of traditional lecturing as a control in research studies, and support active learning as the preferred, empirically validated teaching practice in regular classrooms.

constructivism | undergraduate education | evidence-based teaching | scientific teaching

Lecturing has been the predominant mode of instruction since universities were founded in Western Europe over 900 y ago (1). Although theories of learning that emphasize the need for students to construct their own understanding have challenged the theoretical underpinnings of the traditional, instructor-focused, “teaching by telling” approach (2, 3), to date there has been no quantitative analysis of how constructivist versus exposition-centered methods impact student performance in undergraduate courses across the science, technology, engineering, and mathematics (STEM) disciplines. In the STEM classroom, should we ask or should we tell?

Addressing this question is essential if scientists are committed to teaching based on evidence rather than tradition (4). The answer could also be part of a solution to the “pipeline problem” that some countries are experiencing in STEM education: For example, the observation that less than 40% of US students who enter university with an interest in STEM, and just 20% of STEM-interested underrepresented minority students, finish with a STEM degree (5).

To test the efficacy of constructivist versus exposition-centered course designs, we focused on the design of class sessions—as opposed to laboratories, homework assignments, or other exercises. More specifically, we compared the results of experiments that documented student performance in courses with at least some active learning versus traditional lecturing, by metaanalyzing

225 studies in the published and unpublished literature. The active learning interventions varied widely in intensity and implementation, and included approaches as diverse as occasional group problem-solving, worksheets or tutorials completed during class, use of personal response systems with or without peer instruction, and studio or workshop course designs. We followed guidelines for best practice in quantitative reviews (*SI Materials and Methods*), and evaluated student performance using two outcome variables: (i) scores on identical or formally equivalent examinations, concept inventories, or other assessments; or (ii) failure rates, usually measured as the percentage of students receiving a D or F grade or withdrawing from the course in question (DFW rate).

The analysis, then, focused on two related questions. Does active learning boost examination scores? Does it lower failure rates?

## Results

The overall mean effect size for performance on identical or equivalent examinations, concept inventories, and other assessments was a weighted standardized mean difference of 0.47 ( $Z = 9.781$ ,  $P \ll 0.001$ )—meaning that on average, student performance increased by just under half a SD with active learning compared with lecturing. The overall mean effect size for failure rate was an odds ratio of 1.95 ( $Z = 10.4$ ,  $P \ll 0.001$ ). This odds ratio is equivalent to a risk ratio of 1.5, meaning that on average, students in traditional lecture courses are 1.5 times more likely to fail than students in courses with active learning. Average failure rates were 21.8% under active learning but 33.8% under traditional lecturing—a difference that represents a 55% increase (Fig. 1 and Fig. S1).

## Significance

The President's Council of Advisors on Science and Technology has called for a 33% increase in the number of science, technology, engineering, and mathematics (STEM) bachelor's degrees completed per year and recommended adoption of empirically validated teaching practices as critical to achieving that goal. The studies analyzed here document that active learning leads to increases in examination performance that would raise average grades by a half a letter, and that failure rates under traditional lecturing increase by 55% over the rates observed under active learning. The analysis supports theory claiming that calls to increase the number of students receiving STEM degrees could be answered, at least in part, by abandoning traditional lecturing in favor of active learning.

Author contributions: S.F. and M.P.W. designed research; S.F., M.M., M.K.S., N.O., H.J., and M.P.W. performed research; S.F. and S.L.E. analyzed data; and S.F., S.L.E., M.M., M.K.S., N.O., H.J., and M.P.W. wrote the paper.

The authors declare no conflict of interest.

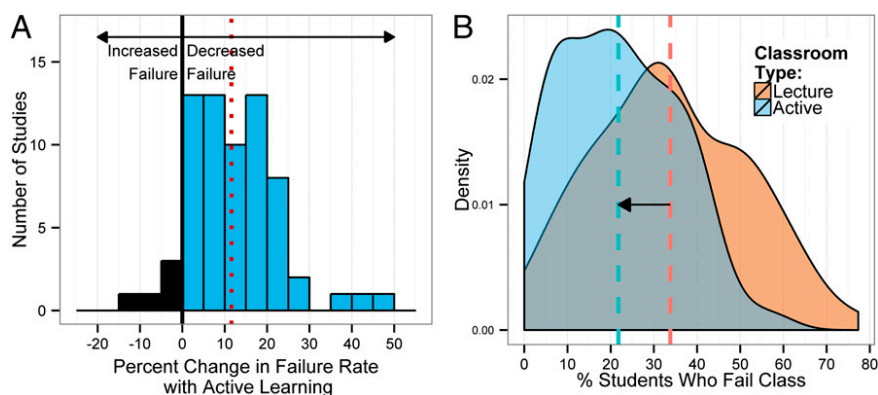
\*This Direct Submission article had a prearranged editor.

Freely available online through the PNAS open access option.

See Commentary on page 8319.

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This article contains supporting information online at [www.pnas.org/lookup/suppl/doi:10.1073/pnas.1319030111/-DCSupplemental](http://www.pnas.org/lookup/suppl/doi:10.1073/pnas.1319030111/-DCSupplemental).



**Fig. 1.** Changes in failure rate. (A) Data plotted as percent change in failure rate in the same course, under active learning versus lecturing. The mean change (12%) is indicated by the dashed vertical line. (B) Kernel density plots of failure rates under active learning and under lecturing. The mean failure rates under each classroom type (21.8% and 33.8%) are shown by dashed vertical lines.

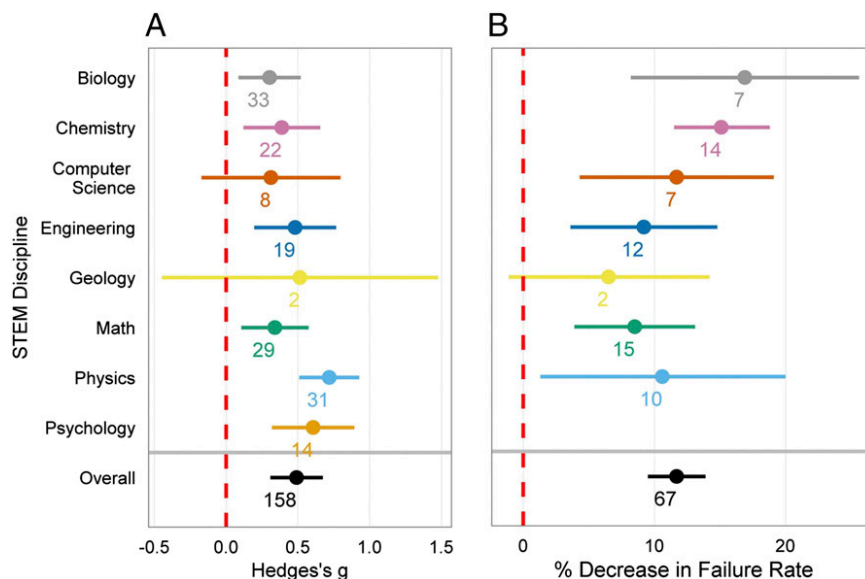
Heterogeneity analyses indicated no statistically significant variation among experiments based on the STEM discipline of the course in question, with respect to either examination scores (Fig. 2A;  $Q = 910.537$ ,  $df = 7$ ,  $P = 0.160$ ) or failure rates (Fig. 2B;  $Q = 11.73$ ,  $df = 6$ ,  $P = 0.068$ ). In every discipline with more than 10 experiments that met the admission criteria for the meta-analysis, average effect sizes were statistically significant for either examination scores or failure rates or both (Fig. 2, Figs. S2 and S3, and Tables S1A and S2A). Thus, the data indicate that active learning increases student performance across the STEM disciplines.

For the data on examinations and other assessments, a heterogeneity analysis indicated that average effect sizes were lower when the outcome variable was an instructor-written course examination as opposed to performance on a concept inventory (Fig. 3A and Table S1B;  $Q = 10.731$ ,  $df = 1$ ,  $P < 0.001$ ). Although student achievement was higher under active learning for both types of assessments, we hypothesize that the difference in gains for examinations versus concept inventories may be due to the two types of assessments testing qualitatively different cognitive skills. This explanation is consistent with previous research

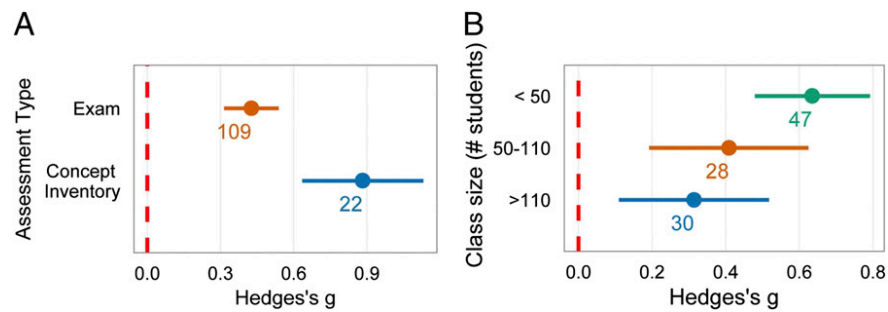
indicating that active learning has a greater impact on student mastery of higher- versus lower-level cognitive skills (6–9), and the recognition that most concept inventories are designed to diagnose known misconceptions, in contrast to course examinations that emphasize content mastery or the ability to solve quantitative problems (10). Most concept inventories also undergo testing for validity, reliability, and readability.

Heterogeneity analyses indicated significant variation in terms of course size, with active learning having the highest impact on courses with 50 or fewer students (Fig. 3B and Table S1C;  $Q = 6.726$ ,  $df = 2$ ,  $P = 0.035$ ; Fig. S4). Effect sizes were statistically significant for all three categories of class size, however, indicating that active learning benefitted students in medium (51–110 students) or large (>110 students) class sizes as well.

When we metaanalyzed the data by course type and course level, we found no statistically significant difference in active learning's effect size when comparing (i) courses for majors versus nonmajors ( $Q = 0.045$ ,  $df = 1$ ,  $P = 0.883$ ; Table S1D), or (ii) introductory versus upper-division courses ( $Q = 0.046$ ,  $df = 1$ ,  $P = 0.829$ ; Tables S1E and S2D).



**Fig. 2.** Effect sizes by discipline. (A) Data on examination scores, concept inventories, or other assessments. (B) Data on failure rates. Numbers below data points indicate the number of independent studies; horizontal lines are 95% confidence intervals.



**Fig. 3.** Heterogeneity analyses for data on examination scores, concept inventories, or other assessments. (A) By assessment type—concept inventories versus examinations. (B) By class size. Numbers below data points indicate the number of independent studies; horizontal lines are 95% confidence intervals.

To evaluate how confident practitioners can be about these conclusions, we performed two types of analyses to assess whether the results were compromised by publication bias, i.e., the tendency for studies with low effect sizes to remain unpublished. We calculated fail-safe numbers indicating how many missing studies with an effect size of 0 would have to be published to reduce the overall effect sizes of 0.47 for examination performance and 1.95 for failure rate to preset levels that would be considered small or moderate—in this case, 0.20 and 1.1, respectively. The fail-safe numbers were high: 114 studies on examination performance and 438 studies on failure rate (*SI Materials and Methods*). Analyses of funnel plots (Fig. S5) also support a lack of publication bias (*SI Materials and Methods*).

To assess criticisms that the literature on undergraduate STEM education is difficult to interpret because of methodological shortcomings (e.g., ref. 11), we looked for heterogeneity in effect sizes for the examination score data, based on whether experiments did or did not meet our most stringent criteria for student and instructor equivalence. We created four categories to characterize the quality of the controls over student equivalence in the active learning versus lecture treatments (*SI Materials and Methods*), and found that there was no heterogeneity based on methodological quality ( $Q = 2.097$ ,  $df = 3$ ,  $P = 0.553$ ): Experiments where students were assigned to treatments at random produced results that were indistinguishable from three types of quasirandomized designs (Table 1). Analyzing variation with respect to controls over instructor identity also produced no evidence of heterogeneity ( $Q = 0.007$ ,  $df = 1$ ,  $P = 0.934$ ): More poorly controlled studies, with different instructors in the two treatment groups or with no data provided on instructor equivalence, gave equivalent results to studies with identical or randomized instructors in the two treatments (Table 1). Thus, the overall effect size for examination data appears robust to variation in the methodological rigor of published studies.

## Discussion

The data reported here indicate that active learning increases examination performance by just under half a SD and that lecturing increases failure rates by 55%. The heterogeneity analyses indicate that (i) these increases in achievement hold across all of the STEM disciplines and occur in all class sizes, course types, and course levels; and (ii) active learning is particularly beneficial in small classes and at increasing performance on concept inventories.

Although this is the largest and most comprehensive meta-analysis of the undergraduate STEM education literature to date, the weighted, grand mean effect size of 0.47 reported here is almost identical to the weighted, grand-mean effect sizes of 0.50 and 0.51 published in earlier metaanalyses of how alternatives to traditional lecturing impact undergraduate course performance in subsets of STEM disciplines (11, 12). Thus, our results are consistent with previous work by other investigators.

The grand mean effect sizes reported here are subject to important qualifications, however. For example, because struggling students are more likely to drop courses than high-achieving students, the reductions in withdrawal rates under active learning that are documented here should depress average scores on assessments—meaning that the effect size of 0.47 for examination and concept inventory scores may underestimate active learning's actual impact in the studies performed to date (*SI Materials and Methods*). In contrast, it is not clear whether effect sizes of this magnitude would be observed if active learning approaches were to become universal. The instructors who implemented active learning in these studies did so as volunteers. It is an open question whether student performance would increase as much if all faculty were required to implement active learning approaches.

Assuming that other instructors implement active learning and achieve the average effect size documented here, what would

**Table 1.** Comparing effect sizes estimated from well-controlled versus less-well-controlled studies

Type of control	<i>n</i>	Hedges's <i>g</i>	SE	95% confidence interval	
				Lower limit	Upper limit
For student equivalence					
Quasirandom—no data on student equivalence	39	0.467	0.102	0.268	0.666
Quasirandom—no statistical difference in prescores on assessment used for effect size	51	0.534	0.089	0.359	0.709
Quasirandom—no statistical difference on metrics of academic ability/preparedness	51	0.362	0.092	0.181	0.542
Randomized assignment or crossover design	16	0.514	0.098	0.322	0.706
For instructor equivalence					
No data, or different instructors	59	0.472	0.081	0.313	0.631
Identical instructor, randomized assignment, or ≥3 instructors in each treatment	99	0.492	0.071	0.347	0.580

a shift of 0.47 SDs in examination and concept inventory scores mean to their students?

- i) Students performing in the 50th percentile of a class based on traditional lecturing would, under active learning, move to the 68th percentile of that class (13)—meaning that instead of scoring better than 50% of the students in the class, the same individual taught with active learning would score better than 68% of the students being lectured to.
- ii) According to an analysis of examination scores in three introductory STEM courses (*SI Materials and Methods*), a change of 0.47 SDs would produce an increase of about 6% in average examination scores and would translate to a 0.3 point increase in average final grade. On a letter-based system, medians in the courses analyzed would rise from a B– to a B or from a B to a B+.

The result for undergraduate STEM courses can also be compared with the impact of educational interventions at the precollege level. A recent review of educational interventions in the K–12 literature reports a mean effect size of 0.39 when impacts are measured with researcher-developed tests, analogous to the examination scores analyzed here, and a mean effect size of 0.24 for narrow-scope standardized tests, analogous to the concept inventories analyzed here (14). Thus, the effect size of active learning at the undergraduate level appears greater than the effect sizes of educational innovations in the K–12 setting, where effect sizes of 0.20 or even smaller may be considered of policy interest (14).

There are also at least two ways to view an odds ratio of 1.95 for the risk of failing a STEM course:

- i) If the experiments analyzed here had been conducted as randomized controlled trials of medical interventions, they may have been stopped for benefit—meaning that enrolling patients in the control condition might be discontinued because the treatment being tested was clearly more beneficial. For example, a recent analysis of 143 randomized controlled medical trials that were stopped for benefit found that they had a median relative risk of 0.52, with a range of 0.22 to 0.66 (15). In addition, best-practice directives suggest that data management committees may allow such studies to stop for benefit if interim analyses have large sample sizes and *P* values under 0.001 (16). Both criteria were met for failure rates in the education studies we analyzed: The average relative risk was 0.64 and the *P* value on the overall odds ratio was  $\ll$  0.001. Any analogy with biomedical trials is qualified, however, by the lack of randomized designs in studies that included data on failure rates.
- ii) There were 29,300 students in the 67 lecturing treatments with data on failure rates. Given that the raw failure rate in this sample averaged 33.8% under traditional lecturing and 21.8% under active learning, the data suggest that 3,516 fewer students would have failed these STEM courses under active learning. Based on conservative assumptions (*SI Materials and Methods*), this translates into over US\$3,500,000 in saved tuition dollars for the study population, had all students been exposed to active learning. If active learning were implemented widely, the total tuition dollars saved would be orders of magnitude larger, given that there were 21 million students enrolled in US colleges and universities alone in 2010, and that about a third of these students intended to major in STEM fields as entering freshmen (17, 18).

Finally, increased grades and fewer failures should make a significant impact on the pipeline problem. For example, the 2012 President's Council of Advisors on Science and Technology report calls for an additional one million STEM majors in the United States in the next decade—requiring a 33% increase

from the current annual total—and notes that simply increasing the current STEM retention rate of 40% to 50% would meet three-quarters of that goal (5). According to a recent cohort study from the National Center for Education Statistics (19), there are gaps of 0.5 and 0.4 in the STEM-course grade point averages (GPAs) of first-year bachelor's and associate's degree students, respectively, who end up leaving versus persisting in STEM programs. A 0.3 “bump” in average grades with active learning would get the “leavers” close to the current performance level of “persisters.” Other analyses of students who leave STEM majors indicate that increased passing rates, higher grades, and increased engagement in courses all play a positive role in retention (20–22).

In addition to providing evidence that active learning can improve undergraduate STEM education, the results reported here have important implications for future research. The studies we metaanalyzed represent the first-generation of work on undergraduate STEM education, where researchers contrasted a diverse array of active learning approaches and intensities with traditional lecturing. Given our results, it is reasonable to raise concerns about the continued use of traditional lecturing as a control in future experiments. Instead, it may be more productive to focus on what we call “second-generation research”: using advances in educational psychology and cognitive science to inspire changes in course design (23, 24), then testing hypotheses about which type of active learning is most appropriate and efficient for certain topics or student populations (25). Second-generation research could also explore which aspects of instructor behavior are most important for achieving the greatest gains with active learning, and elaborate on recent work indicating that underprepared and underrepresented students may benefit most from active methods. In addition, it will be important to address questions about the intensity of active learning: Is more always better? Although the time devoted to active learning was highly variable in the studies analyzed here, ranging from just 10–15% of class time being devoted to clicker questions to lecture-free “studio” environments, we were not able to evaluate the relationship between the intensity (or type) of active learning and student performance, due to lack of data (*SI Materials and Methods*).

As research continues, we predict that course designs inspired by second-generation studies will result in additional gains in student achievement, especially when the types of active learning interventions analyzed here—which focused solely on in-class innovations—are combined with required exercises that are completed outside of formal class sessions (26).

Finally, the data suggest that STEM instructors may begin to question the continued use of traditional lecturing in everyday practice, especially in light of recent work indicating that active learning confers disproportionate benefits for STEM students from disadvantaged backgrounds and for female students in male-dominated fields (27, 28). Although traditional lecturing has dominated undergraduate instruction for most of a millennium and continues to have strong advocates (29), current evidence suggests that a constructivist “ask, don't tell” approach may lead to strong increases in student performance—amplifying recent calls from policy makers and researchers to support faculty who are transforming their undergraduate STEM courses (5, 30).

## Materials and Methods

To create a working definition of active learning, we collected written definitions from 338 audience members, before biology departmental seminars on active learning, at universities throughout the United States and Canada. We then coded elements in the responses to create the following consensus definition:

Active learning engages students in the process of learning through activities and/or discussion in class, as opposed to passively listening



to an expert. It emphasizes higher-order thinking and often involves group work. (See also ref. 31, p. iii).

Following Bligh (32), we defined traditional lecturing as "...continuous exposition by the teacher." Under this definition, student activity was assumed to be limited to taking notes and/or asking occasional and unprompted questions of the instructor.

**Literature Search.** We searched the gray literature, primarily in the form of unpublished dissertations and conference proceedings, in addition to peer-reviewed sources (33, 34) for studies that compared student performance in undergraduate STEM courses under traditional lecturing versus active learning. We used four approaches (35) to find papers for consideration: hand-searching every issue in 55 STEM education journals from June 1, 1998 to January 1, 2010 (Table S3), searching seven online databases using an array of terms, mining reviews and bibliographies (SI Materials and Methods), and "snowballing" from references in papers admitted to the study (SI Materials and Methods). We had no starting time limit for admission to the study; the ending cutoff for consideration was completion or publication before January 1, 2010.

**Criteria for Admission.** As recommended (36), the criteria for admission to the coding and final data analysis phases of the study were established at the onset of the work and were not altered. We coded studies that (i) contrasted traditional lecturing with any active learning intervention, with total class time devoted to each approach not differing by more than 30 min/wk; (ii) occurred in the context of a regularly scheduled course for undergraduates; (iii) were largely or solely limited to changes in the conduct of the regularly scheduled class or recitation sessions; (iv) involved a course in astronomy, biology, chemistry, computer science, engineering, geology, mathematics, natural resources or environmental science, nutrition or food science, physics, psychology, or statistics; and (v) included data on some aspect of student academic performance.

Note that criterion *i* yielded papers representing a wide array of active learning activities, including vaguely defined "cooperative group activities in class," in-class worksheets, clickers, problem-based learning (PBL), and studio classrooms, with intensities ranging from 10% to 100% of class time (SI Materials and Methods). Thus, this study's intent was to evaluate the average effect of any active learning type and intensity contrasted with traditional lecturing.

The literature search yielded 642 papers that appeared to meet these five criteria and were subsequently coded by at least one of the authors.

**Coding.** All 642 papers were coded by one of the authors (S.F.) and 398 were coded independently by at least one other member of the author team (M.M., M.S., M.P.W., N.O., or H.J.). The 244 "easy rejects" were 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; a post hoc analysis suggested that the easy rejects were justified (SI Materials and Methods).

The two coders met to review each of the remaining 398 papers and reach consensus (37, 38) on

- i) The five criteria listed above for admission to the study;
- ii) Examination equivalence—meaning that the assessment given to students in the lecturing and active learning treatment groups had to be identical, equivalent as judged by at least one third-party observer recruited by the authors of the study in question but blind to the hypothesis being tested, or comprising questions drawn at random from a common test bank;
- iii) Student equivalence—specifically whether the experiment was based on randomization or quasirandomization among treatments and, if quasirandom, whether students in the lecture and active learning treatments were statistically indistinguishable in terms of (a) prior general academic performance (usually measured by college GPA at the time of entering the course, Scholastic Aptitude Test, or American College Testing scores), or (b) pretests directly relevant to the topic in question;
- iv) Instructor equivalence—meaning whether the instructors in the lecture and active learning treatments were identical, randomly assigned, or consisted of a group of three or more in each treatment; and
- v) Data that could be used for computing an effect size.

To reduce or eliminate pseudoreplication, the coders also annotated the effect size data using preestablished criteria to identify and report effect sizes only from studies that represented independent courses and populations reported. If the data reported were from iterations of the same course at the same institution, we combined data recorded for more than

one control and/or treatment group from the same experiment. We also combined data from multiple outcomes from the same study (e.g., a series of equivalent midterm examinations) (SI Materials and Methods). Coders also extracted data on class size, course type, course level, and type of active learning, when available.

Criteria *iii* and *iv* were meant to assess methodological quality in the final datasets, which comprised 158 independent comparisons with data on student examination performance and 67 independent comparisons with data on failure rates. The data analyzed and references to the corresponding papers are archived in Table S4.

**Data Analysis.** Before analyzing the data, we inspected the distribution of class sizes in the study and binned this variable as small, medium, and large (SI Materials and Methods). We also used established protocols (38, 39) to combine data from multiple treatments/controls and/or data from multiple outcomes, and thus produce a single pairwise comparison from each independent course and student population in the study (SI Materials and Methods).

The data we analyzed came from two types of studies: (i) randomized trials, where each student was randomly placed in a treatment; and (ii) quasirandom designs where students self-sorted into classes, blind to the treatment at the time of registering for the class. It is important to note that in the quasirandom experiments, students were assigned to treatment as a group, meaning that they are not statistically independent samples. This leads to statistical problems: The number of independent data points in each treatment is not equal to the number of students (40). The element of nonindependence in quasirandom designs can cause variance calculations to underestimate the actual variance, leading to overestimates for significance levels and for the weight that each study is assigned (41). To correct for this element of nonindependence in quasirandom studies, we used a cluster adjustment calculator in Microsoft Excel based on methods developed by Hedges (40) and implemented in several recent metaanalyses (42, 43). Adjusting for clustering in our data required an estimate of the intraclass correlation coefficient (ICC). None of our studies reported ICCs, however, and to our knowledge, no studies have reported an ICC in college-level STEM courses. Thus, to obtain an estimate for the ICC, we turned to the K–12 literature. A recent paper reviewed ICCs for academic achievement in mathematics and reading for a national sample of K–12 students (44). We used the mean ICC reported for mathematics (0.22) as a conservative estimate of the ICC in college-level STEM classrooms. Note that although the cluster correction has a large influence on the variance for each study, it does not influence the effect size point estimate substantially.

We computed effect sizes and conducted the metaanalysis in the Comprehensive Meta-Analysis software package (45). All reported *P* values are two-tailed, unless noted.

We used a random effects model (46, 47) to compare effect sizes. The random effect size model was appropriate because conditions that could affect learning gains varied among studies in the analysis, including the (i) type (e.g., PBL versus clickers), intensity (percentage of class time devoted to constructivist activities), and implementation (e.g., graded or ungraded) of active learning; (ii) student population; (iii) course level and discipline; and (iv) type, cognitive level, and timing—relative to the active learning exercise—of examinations or other assessments.

We calculated effect sizes as (i) the weighted standardized mean difference as Hedges' *g* (48) for data on examination scores, and (ii) the log-odds for data on failure rates. For ease of interpretation, we then converted log-odds values to odds ratio, risk ratio, or relative risk (49).

To evaluate the influence of publication bias on the results, we assessed funnel plots visually (50) and statistically (51), applied Duval and Tweedie's trim and fill method (51), and calculated fail-safe *N*s (45).

**Additional Results.** We did not insist that assessments be identical or formally equivalent if studies reported only data on failure rates. To evaluate the hypothesis that differences in failure rates recorded under traditional lecturing and active learning were due to changes in the difficulty of examinations and other course assessments, we evaluated 11 studies where failure rate data were based on comparisons in which most or all examination questions were identical. The average odds ratio for these 11 studies was  $1.97 \pm 0.36$  (SE)—almost exactly the effect size calculated from the entire dataset.

Although we did not metaanalyze the data using "vote-counting" approaches, it is informative to note that of the studies reporting statistical tests of examination score data, 94 reported significant gains under active learning whereas only 41 did not (Table S4A).

Additional results from the analyses on publication bias are reported in Supporting Information.

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