Psychological Science in the Public Interest
14(1) 1–3

DOI: 10.1177/1529100612454415
http://pspi.sagepub.com

Applying Cognitive Psychology to Education: Translational Educational Science

Henry L. Roediger, III
Washington University in St. Louis

The scientific study of human learning and memory is now more than 125 years old. Psychologists have conducted thousands of experiments, correlational analyses, and field studies during this time, in addition to other research conducted by those from neighboring fields. A huge knowledge base has been carefully built up over the decades.

Given this backdrop, we may ask ourselves: What great changes in education have resulted from this huge research base? How has the scientific study of learning and memory changed practices in education from those of, say, a century ago? Have we succeeded in building a translational educational science to rival medical science (in which biological knowledge is translated into medical practice) or types of engineering (in which, e.g., basic knowledge in chemistry is translated into products through chemical engineering)?

The answer, I am afraid, is rather mixed. Psychologists and psychological research have influenced educational practice, but in fits and starts. After all, some of the great founders of American psychology—William James, Edward L. Thorndike, John Dewey, and others—are also revered as important figures in the history of education. And some psychological research and ideas have made their way into education—for instance, computer-based cognitive tutors for some specific topics have been developed in recent years—and in years past, such practices as teaching machines, programmed learning, and, in higher education, the Keller Plan were all important. These older practices have not been sustained. Was that because they failed or because of a lack of systematic research showing they were effective? At any rate, in 2012, we cannot point to a well-developed translational educational science in which research about learning and memory, thinking and reasoning, and related topics is moved from the lab into controlled field trials (like clinical trials in medicine) and the tested techniques, if they succeed, are introduced into broad educational practice. We are just not there yet, and one question that arises is how we could achieve a translational educational science.

Of course, educational practices have changed over the years, and they are changing all the time. Some changes are based on research, but changes have also been introduced by educational theorists with persuasive (but untested) arguments, by some new fad sweeping through the educational system, or by the marketing of some new way to teach *X* (where *X* might be most anything). The list of highly marketed products without a research base seems overwhelming. Further, the list of people and groups attempting to shape education is great—teachers, principals, state boards of education, parent groups, legislators, textbook publishers, and more. Despite (or perhaps because of) the din surrounding education from competing groups, ineffective practices in education, ones discredited by research, hang on.

I have taught undergraduate courses in cognitive psychology, learning and memory, and introductory psychology (among others) for many years. At some point in these particular courses, I usually ask students to raise their hands if they were required to memorize, verbatim, poems or prose passages (e.g., the Gettysburg Address) during their years in school. Many students, sometimes most students, raise their hands in affirmation. I then go on to ask if their teachers told them why they required this practice. Some students have no idea, but most students who articulate a response say something like the following: "Our teacher told us that remembering information is kind of like lifting weights. If we learn to memorize a long poem and we succeed through many repetitions, then this practice will help us in learning other information in our school courses." In short, memory is like a muscle, and if you use it to memorize a poem, it will become stronger in learning other materials.

This view is the venerable theory of formal discipline, the idea that various cognitive functions are faculties that can be generally enhanced by practice. Thus, practicing memorizing can increase the faculty of memory, and such practice will generalize widely: Learning any other material will be easier if one has memorized poetry (or anything else). It is a fine theory, and it dominated education for many years in the late 1800s and early 1900s, which is one reason why rote learning was so favored during that period. However, research has shown repeatedly that, like many plausible-sounding

Corresponding Author:
Henry L. Roediger, III, Department of Psychology, CB 1125, Washington University, One Brookings Dr., St. Louis, MO 63130-4899
E-mail: roediger@wustl.edu

educational ideas, the fundamental idea behind the doctrine of formal discipline is dead wrong.

In what may have been the only novel experiment he ever carried out on learning and memory, William James (1890, pp. 666–668) tested this intriguing idea and found no evidence for it. In a series of papers published in 1901, Thorndike and Woodworth (1901) performed more tests of the idea and came to the same conclusion. They remarked that "careful tests of one individual and a group of students confirmed Professor James' result" (Thorndike & Woodworth, 1901, p. 251). Every study since then has continued to validate the point: Practicing memorizing one type of material (e.g., lists of words) may improve performance on memorizing similar lists (the phenomenon of learning to learn), but the benefits of such practice will not generalize to other materials. Yet students arriving at college in 2012 still have been required to memorize poetry and other materials by teachers who told them it would help to strengthen their memories.

Now, I have nothing against memorizing poetry. My having memorized *The Raven* in ninth-grade English did me no harm, even if it did not cause me to have superior ability to learn other material. At the time, I certainly felt as if it had done me some good (cognitive dissonance at work?).

The larger question confronting us is what to make of the resistance of some educational practices to change on the basis of psychological research. The idea of formal discipline was declared dead over 100 years ago, but it hangs on, at least for some teachers, and is passed on to new generations of students (some of whom later become teachers). The field of education seems particularly susceptible to the allure of plausible but untested ideas and fads (especially ones that are lucrative for their inventors). One could write an interesting history of ideas based on either plausible theory or somewhat flimsy research—the various methods of teaching math, reading, foreign languages, and on and on—that have come and gone over the years. And, like formal discipline, once an idea takes hold, it is hard to root out. A previous report in *Psychological Science in the Public Interest* was devoted to questioning the orthodoxy of assessing learning styles and teaching to students' preferred styles of learning (Pashler, McDaniel, Rohrer, & Bjork, 2009), an idea supported by, at best, only indirect evidence and, at worst, no rigorous experimental evidence.

In an ideal world, cognitive and educational psychologists would have created a translational educational science that would be eagerly adopted by education schools and educators who would want to improve education on the basis of the latest research findings. The situation in education should be like that in medicine (ideally), a field in which new discoveries from the lab in work with animals are tested in small-scale studies with humans and then in clinical trials with larger numbers of people. If the therapeutic practices pass these tests, they are introduced into clinical practice (with results continuing to be monitored for such issues as side effects). Medical practice has greatly improved since the 1940s, when the idea of clinical trials in medicine began to take hold and replace doctors' heavy reliance on their own experiences and word of mouth.

This translation situation does not yet exist in education despite more than a century of relevant psychological research by educational, cognitive, and social psychologists (and related research by sociologists, educators, economists, and others). Certainly, some groups are working toward this goal. The U.S. Department of Education's Institute of Education Sciences (IES) has the mission "to provide rigorous and relevant evidence on which to ground education practice and policy and share this information broadly" (the quote comes from the IES Web site). The IES's yearly budget is $200 million, which sounds like a lot but is a drop in the bucket for U.S. spending on education (well under 1%). Private foundations, particularly the James S. McDonnell Foundation and the Spencer Foundation, have also funded research on education with the hope that it will eventually be translated into practice in schools.

The present report on improving students' learning with effective techniques falls squarely within this tradition of considering implications of rigorous research for educational practice. The authors examined 10 techniques that are relatively low in cost, can be used in many settings, and (in some cases) are already widely used by students and/or teachers. Of course, many more techniques for learning exist, but often they apply to only one subject (e.g., the best way to learn algebra) without wide generalizability.

To evaluate each technique, the authors asked whether its benefits would generalize across four dimensions: learning conditions (e.g., studying alone vs. studying in a group), student qualities (e.g., age or ability), materials (e.g., scientific concepts, historical facts, mathematical problems), and the criterion tasks on which learning is measured (e.g., essay tests that require transfer of learning, multiple-choice tests). The authors also asked whether strategies for learning shown to work in laboratory settings had also been shown to be effective in actual classroom settings, and, critically, they discuss implementation issues—how hard would it be to use the strategy? Finally, they provide an overall evaluation of the technique's utility.

The authors' assessment revealed five techniques to be effective, albeit to varying degrees, for learning. These techniques received high or at least reasonably positive ratings in terms of utility. Five other techniques were deemed less useful (although assessments of the utility of some might change with further evidence). Considering the bad news first, two techniques that students frequently report using for studying—highlighting (or underlining) text and rereading text—were judged to be ineffective. This is troubling, and one can hope that teachers will get the word out that much more effective study strategies exist than highlighting, underlining, and rereading. The other strategies that lack evidence for their general utility are imagery use for text-based learning, the keyword mnemonic, and (surprisingly, to me) summarization. In some cases, there is simply no evidence for effectiveness, and in other cases, the strategy has been shown to work in some situations but not in others (i.e., not to generalize across the dimensions of interest).

The good news is that five techniques were revealed to be effective and to show some generalizability across types of materials, students, learning conditions, and criterion tasks. Further, classroom evidence exists for most of these techniques, and they are easy to implement. One of the most powerful techniques is distributing practice on tasks. This technique has long been known to be a powerful enhancer of learning (after all, Ebbinghaus [1885/1964] first showed the benefit of distributed practice over massed practice). Despite the fact that spaced or distributed practice could have been implemented a century ago (see Dempster, 1989), an examination of most any textbook today for teaching, say, elementary mathematics will show practice problems grouped by type: Students practice addition problems, then turn to subtraction problems, then go on to multiplication problems, and so on. Learning can occur quickly under massed-practice conditions, so it seems like an efficient way to teach, but hundreds of studies have shown that distributed practice leads to more durable learning. Certainly some blocked practice might be necessary for initial learning, but then practice schedules for problems should be intermixed.

Retrieval practice (or testing) is another powerful and general strategy for learning: If students practice retrieving information, they can keep it in an accessible state (at their mental fingertips, as it were) and can then retrieve and use the information both for answering direct questions and for transferring the knowledge to related situations. This was another of the techniques shown to be most effective in the authors' survey.

Three other techniques—interleaved practice (in which bouts of study or practice for one topic are interleaved among study or practice for other topics), elaborative interrogation (whereby students ask themselves why the information they are reading is true), and self-explanation (whereby students explain some procedure or process to themselves)—seem promising in some situations, but lacked the general utility of distributed practice and retrieval practice via testing. Still, these techniques were shown to be reasonably effective in experimental studies.

I have provided only a quick snapshot here of the great scholarly achievement provided in this report. The authors have constructed their paper to be modular, so that if readers want to learn about, say, the pros and cons of summarization, they can dip into the manuscript at the appropriate spot. Of course, as with all active areas of research, new findings may modify the recommendations provided in the report, but one positive feature of Dunlosky et al.'s thorough review of these 10 techniques is that it pinpoints many areas where new research is needed.

Another general issue for future consideration is evidence about combinations of techniques. Students (and teachers) usually employ more than one strategy for learning (or teaching). If distributed practice and interleaving are combined (as they usually are) and students also practice retrieval, how good can their performance become? Research on questions about synergistic effects among learning techniques mostly lies in the future for now, but this issue will be critically important to enhance performance in schools.

The techniques that the authors discuss, even the most effective ones, are not educational panaceas, of course. They all assume a motivated learner, a student who wants to learn (even if only to do well on a test). If children come to school hungry, without proper books and materials, lacking parents who support learning, then even the best study strategies in the world will not overcome their deficits. Thus, these methods and strategies should properly be seen as one set of techniques that will prove useful in education, but no one would claim that students' adoption of these methods would overcome all (or even the main) educational problems in our schools. Still, the effective learning methods are important and should be implemented wherever possible. Similarly, students should learn that study methods such as highlighting and simple rereading are ineffective, and they should be encouraged to use better techniques, such as retrieval practice via testing (or self-testing).

Will the recommendations of this report be translated into action by the educational system? Will we come to have a translational educational science in the years ahead? Every reader of this report can help in this cause by widely circulating the findings. We can hope that the recommendations in this report make their way into education schools, textbooks, and, eventually, the hands of teachers and students.

Declaration of Conflicting Interests

The author declared no conflicts of interest with respect to his authorship or the publication of this article.

References

Dempster, F. N. (1989). The spacing effect: A case study in the failure to apply the results of psychological research. *American Psychologist*, *43*, 627–634.

Ebbinghaus, H. E. (1964). *Memory: A contribution to experimental psychology*. New York, NY: Dover. (Original work published 1885)

James, W. (1890). *Principles of psychology* (Vol. 1). New York, NY: Dover.

Pashler, H., McDaniel, M. A., Rohrer, D., & Bjork, R. A. (2009). Learning styles: Concepts and evidence. *Psychological Science in the Public Interest*, *9*, 105–119.

Thorndike, E. L., & Woodworth, R. S. (1901). The influence of improvement in one mental function upon the efficiency of other functions (I). *Psychological Review*, *8*, 247–261.