CBE—Life Sciences Education
Vol. 5, 104–110, Summer 2006

Feature

Points of View: On the Implications of Neuroscience Research for Science Teaching and Learning: Are There Any?

A Skeptical Theme and Variations: The Primacy of Psychology in the Science of Learning

John T. Bruer

James S. McDonnell Foundation, St. Louis, MO 63117

NOTE FROM THE EDITOR

Points of View is a series designed to address issues faced by many people within the life sciences educational realm. We present differing points of view back to back on a given topic to stimulate thought and dialogue.

The focus of all contributed features and research articles in *Issues in Neuroscience Education* is the teaching and learning of neuroscience, from elementary school to graduate school audiences. However, neuroscience is unique as a branch of biology in that it includes the study of neuronal and brain mechanisms that may underlie learning. To highlight this unique position of neuroscience, we have chosen to focus this issue's *Points of View* on how research findings in the field of neuroscience may or may not have implications for the teaching and learning of science in general. We invited authors to address the following questions:

- What are the current implications of neuroscience research, if any, for how to improve K–25+ science teaching and learning in schools and universities?
- To what extent will neuroscience research into biological mechanisms of learning, memory, attention, and other brain functions inform educational practices and science teaching in the future?

INTRODUCTION

I am notorious for my skepticism about what neuroscience can currently offer to education. My skepticism derives from several concerns, but a common theme runs through all of them: attempts to link neuroscience with education pay insufficient attention to psychology. In what follows, I will present four variations on this theme. First, for those who are committed to developing a science-based pedagogy and solving existing instructional problems, cognitive psychology offers a mother-lode of still largely untapped knowledge. Second, attempts to link developmental neurobiology to brain development and education ignore, or are inconsistent with, what cognitive psychology tells us about teaching and learning. Third, cognitive neuroscience is the brain-based discipline that is most likely to generate educationally relevant insights, but cognitive neuroscience presupposes cognitive psychology and, to date, rarely constrains existing cognitive models. And fourth, the methods of cellular and molecular neuroscience are powerful, but it is not always clear that the concepts of learning and memory used by neuroscientists are the same as those used by psychologists, let alone by classroom teachers.

DOI: 10.1187/cbe.06–03–0153
Address correspondence to: John T. Bruer (bruer@jsmf.org).

Cognitive Psychology: A Basic Science of Learning

My notoriety as a neuroscience and education skeptic derives primarily from my 1997 article "Education and the Brain: A Bridge Too Far" (Bruer, 1997). I argued that cognitive psychology, not neuroscience, is the strongest current candidate for a basic science of learning. The psychological research that appears most relevant to improved educational practice is research on human problem solving and expertise, a research program initiated by Newell and Simon (1972), and research on memory and knowledge organization as reviewed by Brown *et al.* (1983). Research in this tradition attempts to explain problem solving and learning by formulating and testing cognitive models that assume the existence of mental representations and functions. In the educational context, such models can describe novice behavior in a problem-solving domain, as well as expert behavior. Such models can guide learning by suggesting instructional interventions that might transform novice models into more expert models (Bruer, 1993).

In 1986, at the James S. McDonnell Foundation, I initiated a program, Cognitive Studies for Educational Practice (CSEP), that funded applications of cognitive principles to K–12 instruction (McGilly, 1994). That program provided research support for collaborations between cognitive psychologists and classroom teachers to develop research-based methods for solving recognized instructional problems, that is, classroom problems teachers identified as significant barriers to learning. Among the issues teachers identified were students' inability to read with understanding, to master elementary arithmetic, to understand fractions, to make the transition from arithmetic to algebra, to transfer scientific understanding from classroom to real-world problems, to learn American history, and to write a coherent essay. The psychologist–teacher teams developed instructional methods based on cognitive research to address these problems and tested their interventions in classrooms, often with educationally significant results. Research funded under the CSEP program, along with other cognitively inspired educational research, provide the substance for a series of reports published by the National Research Council (NRC). Neuroscience does not figure prominently in these reports even though brain scientists served on the boards and advisory groups that prepared these documents.

For example, *How Students Learn* (NRC, 2005) makes no mention of current or future implications of neuroscience for educational practice. Another NRC study, *Knowing What Students Know* (NRC, 2001), discusses the implications of brain science in a six-page appendix within the 315-page report. The report dismisses the folk-theoretic views about brain lateralization and learning, the advisability of extrapolating from effects of environmental enrichment on rodent brain educational practice, and the educational significance of critical periods. While concluding that applications of brain science to general education are currently limited, the report points to special education as a promising area for future applications, citing Michael Merzenich and Paula Tallal's work on dyslexia (p. 109). The NRC study *How People Learn: Brain, Mind, Experience, and School* (NRC, 1999), a 300-page book, has one 10-page chapter on the brain. The study modestly concludes that brain research has established that structural changes in the brain encode learning and acknowledges that in the future, neuroscience might provide some practical benefits to educators.

So, I am not alone in my skepticism, or at least my reservations, about the relevance of current neuroscience to educational practice. There is a substantial research community within psychology—whose work focuses on teaching, learning, real-world instructional problems, and solutions to these problems—that shares my concerns. Given the problems that confront teachers in the classroom (as illustrated by the list above), as a practical matter, many of us see neuroscience as having currently little to contribute toward solving those problems. What is frustrating to educational researchers and others committed to developing a science of learning is that educators' current fascination with synapses and brain images causes them to overlook a substantial body of psychological and behavioral research that could have immediate impact in the classroom.

Synapses, Critical Periods, and Developmental Neurobiology

What accounts for the current fascination with synapses, brain images, and learning? Although educators have always been intrigued by brain science (witness the long history of right- vs. left-brain learning in education and the media), it was the concerted effort of policy advocates in the mid-1990s, arguing for an expanded Head Start Program, that brought brain science and education to the covers of *Time* and *Newsweek* (Bruer, 1999). A policy argument intended to advance the legislative cause of the educationally disadvantaged, once it hit the newsstands, resonated with middle-class parents throughout the industrialized world and provided grounds for purveyors of educational materials and advice to develop and promote "brain-based curricula" and "brain-compatible learning" programs.

The brain and early childhood education argument was based on three well-established results from developmental neurobiology. First, in early childhood there are periods of rapid developmental synaptogenesis, followed by synaptic pruning. Second, there are critical periods in development when normal experience is required for normal development. Third, rodent studies have shown that rearing the animals in complex environments has demonstrable effects on brain structure. These results are spun into the following story: periods of peak synaptic density or high resting brain metabolic rate are the periods when children learn everything most easily, when experiences hardwire the brain for life, and when learning results in life-long neural changes. According to this literature, the period of peak synaptic density is *the* critical period for brain development. The more synapses that are used during this period, the more will be retained into adulthood, and more synapses equal higher intelligence. Parenting and education are exercises in synaptic conservation, where more and earlier experience saves neural connections, builds better brains, and maximizes intelligence. Depending on the author, the policy recommendation, or the educational program being peddled this critical period can be birth to three, birth to 10, or three to 12 years of age.

Of course, on closer consideration, the findings of developmental neurobiology do not cohere with this popular story. Synapse elimination is essential for normal brain development, and elimination is primarily under genetic, not environmental control. Critical periods do not always neatly correlate with peak synaptic density or resting brain metabolism, and critical period effects appear to be confined to acquiring species-general traits (e.g., vision, first language) and do not generalize to culturally specific learning, the kinds of learning that occur in school and in an individual's daily life. Rearing rodents in complex environments affects brain structure throughout the life span, not just early in life when critical periods typically occur.

More significantly, the popular "brain and education story" does not cohere with what experimental psychologists know about learning over the life span. Specifically, the proponents of the story ignore decades of psychological research that shows that rate and ease of subject matter learning depend more on prior background knowledge than on biological maturation and chronological age. The NRC reports abundantly document this finding.

Of course, this argument is aided and abetted by possibly overzealous neuroscientists who want to see their work as having implications for solving societal problems or who, concluding their published articles, speculate about the direction of further research. I have provided examples of this difficulty elsewhere (Bruer, 2002). In the present context, one example will suffice.

In spring 2000, the popular press and brain-based educators became enthralled by the claim that the teenage brain was not yet mature. The hard science behind this claim was a paper reporting the results of a magnetic resonance imaging (MRI) study showing an increase in cortical gray matter in regions of the adolescent brain. The authors of this study speculated that the observed changes might be related to a second wave of synaptic overproduction: "It may herald a critical stage in development when the environment or activities of the teenager may guide selective synapse elimination during adolescence" (Giedd *et al.,* 1999). The original study's speculation about a possible second wave of synaptogenesis was highlighted in a U.S. National Institute of Mental Health press release (see NIH Publication No. 01–4929NIMH Press release, Child and adolescent mental health information at http://www.nimh.nih.gov/publicat/childmenu.cfm). The press release, while acknowledging that the cause of the changes in gray matter were not yet known, said that changes in the teenage brain may be due to the same "use-it-or-lose-it" principle that governs early development of the visual system. Synapses that get exercised are retained, whereas those that are not wither. Among brain enthusiasts, possible late synaptogenesis would extend the biologically privileged learning period into the adolescent years. The press release speculated about the implications of the researchers' speculation.

This message became the scientific centerpiece for the May 2, 2000, White House Conference on Teenagers. On National Public Radio's (NPR) *Morning Edition* coverage of the conference, "use it or lose it" was presented as science's best guess of what was happening in the teenage brain: "If children are using their brain at this point for academics or sports or music or video games that is what their brain will be hardwired or optimized for" (www.npr.org/opt/collections/torched/me/data_me/seg_73624.htm provides the audio of the segment). This is now a third-order speculation.

The "spun" version also resonated with educators. The morning the NPR story ran I received the following e-mail from a teacher: "I heard this incredible piece on NPR this morning the abstract for which I will reproduce below. This has unbelievable developmental implications—helps explain why junior high school kids don't learn anything! If the pruning of the brain actually happens twice, this also helps explain the incredible leap in learning rates of adolescents (once the pruning begins, not during the explosion of cell growth)."

Third-order speculation should not be the process by which neuroscience is perceived to have implications for education.

How can neuroscientists help educators now? It is possible that neuroscientists are not aware of how their work and their forward-looking, speculative hypotheses are perceived and interpreted within the educational and lay communities. They should think critically about how their research is presented to educators and the public and should avoid even the most innocent speculation about the practical significance of basic research. They should remind the interested public that we are just at the beginning of our scientific inquiry into how neural structures implement mental functions and how mental functions guide behavior. We neuroscience and education skeptics make this recommendation: "Neuroscience has advanced to the point where it is time to think critically about the form in which research information is made available to educators so that it is interpreted appropriately for practice—identifying which research findings are ready for implementation and which are not" (NRC, 1999, p. 114). We would also add that speculations about practical applications are not research information.

Cognitive Models Matter in Cognitive Neuroscience Too

In my 1997 article, I argued that, although cognitive psychology provides the best current and midterm future foundation for a science of learning, if neuroscience were to become relevant to education, it was most likely to be via its subdiscipline of cognitive neuroscience. But even here, the road to better learning is probably not as independent of psychological and behavioral science as some cognitive neuroscientists and educators might believe.

One of the impediments to cross-disciplinary dialogues about the relevance of science, or a science, to learning is a clear understanding of what the basic assumptions and methods of the various disciplines are and the levels of analysis at which they operate. Traditionally, psychology is best understood as a discipline that studies individual behavior. For most of its history, it has been a behavioral science in at least two senses. First, psychologists attempt to develop theories that explain behavior. Second, the data psychologists collect to frame and test their theories have been behavioral data—reaction times, eye movements, and number of items successfully remembered from a list. In North America, from the turn of the century until the mid-1950s, behaviorism dominated psychology. Behaviorists believed that only observable entities should be allowed into psychological theories and that all human behavior could be accounted for by chains of stimuli and responses, with no need to posit unobservable mental functions and concepts. In the mid-1950s, North American psychology underwent a "cognitive revolution," wherein psychologists recognized that any adequate theory of human behavior required positing mental constructs and functions that were not directly observable (see Bruer, 1993). Psychology became a science of mind. Psychologists viewed the human mind as a computing device that contained both programs and data structures. Cognitive psychologists still used behavioral data, but now used it to frame and test hypotheses about what programs and data structures enabled human behavior. Cognitive neuroscience emerged as a new discipline in the early 1980s. Cognitive neuroscientists work at the interface of biological and behavioral science. Using both behavioral and biological measures of brain activity (single-cell recording, evoked response potentials, and brain-imaging technologies), cognitive neuroscientists attempt to discover the neural hardware that runs the mental software posited by cognitive psychological research. As we will see, one of the key issues in developing a coherent science of learning is being clear about these various levels of analysis and how these levels interact.

Compared with speculations about synapse formation, contemplating the educational implications of cognitive neuroscience is a welcome step in the right direction. The reason for this is that cognitive neuroscience and an applied science of learning meet at an appropriate level of analysis. Cognitive neuroscience presupposes cognitive models. Furthermore, our current applied science of learning has established how cognitive models can contribute to improved teaching and learning, as we see in the NRC reports. Unlike cognitive psychology, which leaves educators and the public cold, cognitive neuroscience has boundless, albeit superficial, popular appeal. This popularity springs from its chief research tool: brain imaging. Cognitive neuroscientists present their data in colorful images, where highly active brain areas appear as bright patches within the brain.

Cognitive neuroscience is a hybrid discipline, a melding of cognitive psychology, systems neuroscience, and computational modeling. From the outset, the goal of cognitive neuroscience has been to identify neural structures that implement cognitive functions. In its initial decade, evidence for localization claims came primarily from lesion or electroencephalograph studies. One might say that cognitive neuroscience ceased to be a "boutique discipline" with the development of positron emission tomography and later functional MRI technology.

These advances in brain imaging technology allowed cognitive neuroscientists to gather data on brain activations in normal subjects. In 1988, Posner *et al.* (1988) articulated a research strategy for cognitive neuroscientific, brain-imaging studies. The first sentence of their abstract stated: "The human brain localizes mental operations of the kind posited by cognitive theories." Performance studies typical of psychological research, they argued, provide at best indirect and inconclusive evidence about localization of cognitive processes. Imaging studies provided a new source of direct evidence that allowed cognitive neuroscientists to test hypotheses about the localization of cognitive processes. That first sentence formulated the working hypothesis of cognitive neuroscientific brain-imaging research. The utility and power of that hypothesis is evident in the progress cognitive neuroscience has made to date.

That the working hypothesis remains central to cognitive neuroscience is evident in recent discussions of criteria that imaging studies should fulfill to be considered publishable. The editors of *Nature Neuroscience* (Editorial, 2001) suggested that among these criteria are the requirement that the study be hypothesis driven; that the study allow scientists to ask questions about basic cognitive processes, rather than identifying networks of brain regions activated by a series of tasks; and that the study include rigorous behavioral designs that ensure that the authors have isolated the cognitive process of interest.

Imaging studies allow cognitive neuroscientists to localize the processes, functions, and representations that cognitive psychologists have identified using methods of experimental psychology, cognitive constructs that have prior experimental support in behavioral and performance data. Cognitive neuroscience thus presupposes cognitive psychology. Cognitive neuroscience makes advances by providing better localizations of cognitive functions, but one could certainly argue that fundamental progress in cognitive neuroscience depends not only on the ability to identify cognitive processes and localize them, but also on the ability to analyze further these processes into their subcomponents at even higher levels of resolution. If the challenge is to understand at deeper levels the actual mental operations implemented in brain areas, then the cognitive models used in imaging studies must be continually refined (Posner and Raichle, 1994). Cognitive models are as fundamental to cognitive neuroscience as they are to our applied science of learning.

Reading instruction and treating dyslexia are areas of considerable educational import. Imaging studies on reading and dyslexia are cited as examples where neuroscience, in the form of cognitive neuroscience, is also thought to have implications for education. Among cognitive neuroscientists and educators, the imaging studies published by the Shaywitz Laboratory at Yale University are probably the best known. In a series of studies, these researchers have shown that there is a functional disruption of brain organization in adult dyslexics (Shaywitz *et al.,* 1998), that similar disruptions are evident in posterior brain systems of child dyslexics (Shaywitz *et al.,* 2002), and that a phonologically or phonics-based reading intervention results in the development of left occipitotemporal brain systems required for skilled reading (Shaywitz *et al.,* 2004).

These imaging studies, as the authors acknowledge in their cited references, presuppose a long history of psychological research on reading and an even longer one in clinical neurology on dyslexia. The earliest theories of word reading and dyslexia derived from clinical neurological case studies in the late 19th century. These theories posited the existence of a sensory language center and a motor language center in the brain connected by a transmission pathway (Geschwind, 1979). Damage to either of the areas or the pathway resulted in different forms of dyslexia. Until the early 1980s, neuropsychologists' primary concern was correlating disorders in cognitive functions, like dyslexia, with specific brain lesions. In 1982 Coltheart (1982) argued for a different approach. Neuropsychologists should engage in *model building*. Their goal should be to interpret patterns of impaired and preserved cognitive functions (dyslexia) in terms of an explicit model of the normal operation of those functions (word recognition). This approach led to the emergence of cognitive neuropsychology, wherein cognitive models of normal cognitive function provided a theoretical foundation for explaining cognitive deficits (Shallice, 1988). Research on reading and dyslexia progressed on the basis of cognitive models of word recognition.

These cognitive models posit modules or computational processes that identify some visual stimuli as legal strings of letters according to the spelling rules of a language (orthographic representations), convert these strings of visually presented letters into sound patterns of the language (phonological representations), associate meanings with these sound patterns (semantic representations), and generate the motor programs needed to pronounce the words. Specific cognitive models differ in how these modules might be interconnected and about how the modules are implemented computationally (Coltheart *et al.,* 1993; Harm and Seidenberg, 2004).

This cognitive research has supported a growing consensus that phonological processing is fundamental to skilled reading and that phonological processing deficits account for the most prevalent form of reading disability, phonological dyslexia. A fundamental assumption that has guided this research over the past 25 years is that theories of dyslexia should be grounded in our understanding of the psychological mechanisms, i.e., the cognitive models, that support word recognition.

One motivation of the 1998 Shaywitz study was to determine why previous imaging studies that had attempted to identify a neural signature of phonological dyslexia had been inconclusive. Their solution was to develop a task hierarchy for an imaging study that would systematically tap components of the prevailing cognitive model of word recognition. These models predict that the most reliable indicator of phonological dyslexia is a reduced capacity to read pseudo- or nonwords (Castles and Coltheart, 1993; Stanovich *et al.*, 1997). According to the theory, pronouncing such orthographically legal, but fictive, words (e.g., "mard" in English) places the highest demand on phonological processing. In the imaging studies, brain activations of dyslexics versus typical readers differ most from the control condition in the pseudoword condition (Shaywitz *et al.*, 1998, 2002). The introductory sections of the three articles by Shaywitz *et al.* are exceptional in their clear statement of how their imaging studies assume and derive from cognitive models of word recognition. These studies adhere to the working hypothesis of cognitive neuroscience, as articulated by Raichle and coworkers and discussed above.

Of these studies, educators are most intrigued by the finding that a phonologically based reading intervention changes the functional anatomy of the brain in problem readers (Shaywitz *et al.*, 2004). As a reading intervention, the Shaywitz *et al.* study adapted an instructional program designed to teach phonological skills that had previously been evaluated using reading-relevant performance (i.e., behavioral) measures (Blachman *et al.*, 1999). The behavioral results reported in the Shaywitz imaging study replicate Blachman's earlier behavioral study. The phonologically based program significantly improved reading fluency in disabled readers. In addition, the imaging study also found that after intervention, the reading-disabled children showed changes in brain activation patterns in areas previously associated with skilled reading (on the basis of previous lesion and imaging studies) both at the end of the intervention and one year later.

This is an impressive result, but what is the *educational* implication? The reading intervention itself is based solely on psychological and behavioral research. We also know from behavioral measures that the intervention improves reading in disabled readers. We know in fact that among 96 methodologically sound, published studies of systematic phonics instruction, the intervention used in the study by Shaywitz *et al.* ranks approximately tenth in mean overall effect size, fourth in word decoding effect size, and seventh in nonword reading effect size (Ehri *et al.*, 2001). We also know based on a meta-analysis of these 96 studies that systematic phonics instruction has a moderate effect on reading outcomes, that effects are greater if instruction begins before first grade, that such instruction helps low- and middle-socioeconomic-status readers, and that it helped students at risk for reading disability (Ehri *et al.*, 2001). We know that systematic phonics instruction works, that the program used in the imaging study is one of the stronger exemplars of such instruction, and that it results in development of neural circuits associated with skilled reading. Based on both the imaging study and previous behavioral research, our educational recommendation would most likely be that systematic phonics instruction should be implemented in beginning reading programs.

Suppose that the imaging study had shown no interpretable change in brain activations previously associated with skilled reading after intervention. How would our educational recommendation change? Given what we know about the educational impact of phonologically mediated reading instruction, based on behavioral measures of reading performance and the basic psychological research that supports this approach to reading instruction, our recommendation would change not at all. Given the vast body of experimental evidence that supports the importance of phonological awareness in reading, a "negative" imaging result would most likely be interpreted as showing that the imaging technology was not sufficiently sensitive to register the predicted postintervention changes in brain activation. The Shaywitz *et al.* result is of interest in as much as it is consistent with current cognitive models of reading and dyslexia—cognitive models the study assumes in its experimental design and tasks. But a negative imaging result in this case would not have implications for educational practice because the cognitive model is too well supported by psychological and behavioral studies.

For education, cognitive models matter more than identifying the brain areas that implement those models. Likewise, as Posner and Raichle argued (see above), using imaging studies to advance our understanding of the brain depends fundamentally on improving and refining our cognitive models. Cognitive neuroscience and imaging studies in particular, *could* provide insights that might help us refine our cognitive models. A few cognitive neuroscientists have started to discuss how brain activation patterns might provide constraints on our cognitive theorizing (Fiez and Petersen 1998; Fiez *et al.*, 1999). However, imaging studies that go beyond establishing localization claims are complex, subtle, and quite rare within current cognitive neuroscience. They require an appreciation for the subtleties of competing cognitive models and the ability to interpret imaging results in the light of all relevant behavioral, neuropsychological, and imaging data available. Making such inferences goes well beyond correlating brain areas with cognitive functions and observing how activation patterns change after learning occurs. However, it is this more subtle and refined form of cognitive neuroscience that can build bridges from systems neuroscience to cognitive psychology. Once we can make this connection, we can explore possible new connections from the resulting cognitive models to the science of learning and educational practice.

Some will find the conclusions I draw about the implications of cognitive neuroscience and brain imaging for an improved science of learning discouraging. However, rather than enthusiastically speculating about the contributions that cognitive neuroscience and brain imaging are about to bring to instruction, we should be dismayed that cognitive models of reading and other subject domains, now nearly four decades old, have had such little impact in the classroom. Rather than suggest that schools of education are remiss in ignoring the neuroscience of reading, we should be concerned that educators remain unaware of the impact that cognitive psychological research has had and can have on educational practice.

Cellular and Molecular Neuroscience: Action at a Distance

The goal of education is learning. Learning is a memory process. Cellular and molecular neuroscientists are attempting to elucidate the molecular mechanisms underlying learning and memory by explaining memory processes at the synaptic level. In this reductive program, neuroscience is attempting to explain changes in behaviors that accrue through learning in terms of changes in synaptic plasticity, or change. The leading candidate mechanisms for synaptic plasticity underlying learning and memory are long-term potentiation (LTP) and depression (LTD). LTP and LTD are the processes of stimulating a dendritic spine repeatedly, leaving it more or less responsive, respectively, to new input of the same type. What might be the educational implications of neuroscientific research at this level for teaching and learning?

Answering this question requires that we first answer another question: Does LTP cause observed changes in memory and behavior? Is it a causal mechanism for learning?

Typically in these discussions about possible causal mechanisms, neuroscientists attempt to analyze causal claims using necessity-sufficiency accounts of causal relations (Buonomano and Merzenich, 1998; Shors and Matzel, 1997). So, one must establish that LTP is both a necessary and sufficient condition for the occurrence of long-term memory. Buonomano and Merzenich (1998) present criteria that must be satisfied to establish the existence of a causal link between concepts like LTP and changes in long-term memory.

LTP is a necessary condition for long-term memory, if, whenever there is a demonstrable change in behavior that can be attributed to the formation of a trace in long-term memory, LTP can be shown to occur in the appropriate neural circuit. LTP is a sufficient condition for memory formation if, whenever LTP occurs, there is a demonstrable change in behavior that can be attributed to the formation of a trace in long-term memory. For a somewhat different case, Buonomano and Merzenich (1998) detail the difficulties one confronts in attempting to establish empirically the existence of causal mechanisms at the synaptic level.

Shors and Matzel (1997), based on their review of the literature, concluded that LTP did not meet the criteria for providing a causal explanation of memory. To make a long argument very short, they documented instances where changes in memory occur without LTP and where LTP occurs without changes in memory. That is, they documented that LTP is neither necessary nor sufficient for memory change. Part of the problem resides in ambiguities over the definition of LTP. Another difficulty arises with experimental use of genetic and pharmacological interventions to block LTP that can have general, rather than specific, effects on the organism, making experimental interpretations difficult.

However, Shors and Matzel cite a more fundamental problem. They report that between 1974 and 1997, more than 1300 articles appeared that had "LTP" in their title. Of these, fewer than 80 described any behavioral manipulation relevant to assessing changes in memory. Furthermore, the articles that contained behavioral manipulations tended to provide evidence against the hypothesis that LTP is a memory mechanism. Thus, the claim that LTP is a molecular mechanism for learning and memory may be more of a dogma of neuroscientific memory research than a hypothesis that is being rigorously tested.

If so, cellular and molecular neuroscientific research on the causal mechanisms underlying memory and learning may represent a consistent set of claims under the dogmatic assumption, but in fact may be unconnected to memory phenomena as assessed by performance and behavioral data. If so, this undermines the relevance of the cellular and molecular results to learning and memory as studied by psychologists. And it is, after all, the psychological concept of memory that is most relevant to teaching and learning. If so, the implications of cellular and molecular neuroscience for teaching and learning are limited now, and will remain so, as long as neuroscientific research remains conceptually disconnected from psychology. Currently, LTP represents synaptic action at a considerable distance from memory and learning.

LTP is but a single example, but it is illustrative of a problem that arises whenever we attempt to link research across levels of analysis that range from molecules to behavior (Roediger *et al.,* 2006). Are concepts like memory, learning, attention, retrieval, and plasticity as understood by neuroscientists the same concepts as understood by psychologists? Or are they only using the same words to designate different phenomena? If research at the synaptic level is to have implications for psychology and education, we need conceptual clarity at every interface between each level of analysis, from behavioral to molecular. Clearing away the semantic underbrush may be an important first step in outlining a research program wherein neuroscience, cognitive neuroscience, and psychology can have eventual implications for teaching and learning.

CONCLUSION

So I remain skeptical about the implications of neuroscience for education currently and into the near future. Maybe I should say the *direct* implications of neuroscience for education. I do believe that eventually we will be able to bridge neuroscience at its various levels of analysis with education, but I am convinced that all of these bridges will have a least one pier on the island of psychology.

REFERENCES

Blachman, B., Tangel, D. M., Ball, E. W., Black, R., and McGraw, C. K. (1999). Developing phonological awareness and word recognition skills: a two-year intervention with low-income, inner-city children. Reading and Writing: An Interdisciplinary Journal *11*, 239–273.

Brown, A. L., Bransford, J. D., Ferrara, R. A., and Campione, J. C. (1983). Learning, remembering, and understanding. In: Handbook of Child Psychology, Vol. 3, Cognitive Development, ed. P. H. Mussen, New York: Wiley, 78–161.

Bruer, J. T. (2002). Avoiding the pediatrician's error: how neuroscientists can help educators (and themselves), Nat. Neurosci. *5*, S1031–S1033.

Bruer, J. T. (1999). The Myth of the First Three Years: A New Understanding of Early Brain Development and Lifelong Learning, New York: Free Press.

Bruer, J. T. (1997). Education and the brain: a bridge too far. Educ. Res. *26*(8), 1–13.

Bruer, J. T. (1993). Schools for Thought: A Science of Learning in the Classroom, Cambridge MA: MIT Press.

Buonomano, D. V., and Merzenich, M. M. (1998). Cortical plasticity: from synapses to maps. Annu. Rev. Neurosci. *21,* 149–186.

Castles, A., and Coltheart, M. (1993). Varieties of developmental dyslexia. Cognition *47*, 149–180.

Coltheart, M. (1982). The psycholinguistic analysis of acquired dyslexia: some illustrations. Phil. Trans. R. Soc. Lond. B. *298*, 151–164.

Coltheart, M. *et al.* (1993). Models of reading aloud: dual-route and parallel-distributed-processing approaches. Psychol. Rev. *100*, 589–608.

Editorial (2001). Analyzing functional imaging studies. Nat. Neurosci. *4*, 333.

Ehri, L. C., Nunes, S. R., Stahl, S. A., and Willows, D. M. (2001). Systematic phonics instruction helps students learn to read: Evidence from the National Reading Panel's meta-analysis. Rev. Educ. Res. *71*, 393–447.

Fiez, J. A., Balota, D. A., Raichle, M. E., and Petersen, S. E. (1999). Effects of lexicality, frequency, and spelling-to-sound consistency of the functional anatomy of reading. Neuron *24*, 205–218.

Fiez, J. A., and Petersen, S. E. (1998). Neuroimaging studies of word reading. Proc. Nat. Acad. Sci. USA *95*, 914–921.

Geschwind, N. (1979). Specializations of the human brain. Sci. Am. *241*, 158–168.

Giedd, J. N., Blumenthal, J., Jeffries, N. O., Castellanos, F. X., Liu, H., Zijdenbos, A., Paus, T., Evans, A. C., and Rapoport, J. L. (1999). Brain development during childhood and adolescence: a longitudinal MRI study. Nat. Neurosci. *2*, 861–863.

Harm, M. W., and Seidenberg, M. S. (2004). Computing the meanings of words in reading: cooperative division of labor between visual and phonological processes. Psychol. Rev. *111*, 662–720.

McGilly, K. (ed.) (1994). Classroom Lessons: Integrating Cognitive Theory and Classroom Practice, Cambridge, MA: MIT Press.

National Research Council (2005). How Students Learn: History, Mathematics, and Science in the Classroom. Committee on How People Learn, A Targeted Report for Teachers, M.S. Donovan and J.D. Bransford, eds., Division of Behavioral and Social Sciences and Education, Washington, D.C. The National Academies Press.

National Research Council (2001). Knowing What Students Know: The Science and Design of Educational Assessment. Committee on the Foundations of Assessment, J. Pelligrino, N. Chudowsky, and R. Glaser, eds., Board on Testing and Assessment, Center for Education, Division of Behavioral and Social Sciences and Education, Washington, DC: The National Academies Press.

National Research Council (1999). How People Learn: Brain, mind, experience, and school. J. D. Bransford, A. L Brown, and R. R. Cocking, eds., Committee on Developments in the Science of Learning, Commission on Behavioral and Social Sciences and Education, Washington, DC: The National Academies Press.

Newell, A., and Simon, H. A. (1972). Human Problem Solving, Englewood Cliffs, NJ: Prentice-Hall.

Posner, M. I., Petersen, S. E., Fox, P. T., and Raichle, M. E. (1988). Localization of cognitive operations in the human brain. Science *240*, 1627–1631.

Posner, M. I., and Raichle, M. (1994). Images of Mind, New York: Scientific American Library.

Roediger, H. R., Dudai, Y., and Fitzpatrick, S. M. (2006). Science of Memory: Concepts, Oxford, United Kingdom: Oxford University Press.

Shallice, T. (1988). From Neuropsychology to Mental Structure, New York: Cambridge University Press.

Shaywitz, S. E. *et al.* (1998). Functional disruption in the organization of the brain for reading in dyslexia. Proc. Natl. Acad. Sci. USA *95*, 2636–2641.

Shaywitz, B. A. *et al.* (2002). Disruption of posterior brain systems for reading in children with developmental dyslexia. Biol. Psychiat. *52*, 101–110.

Shaywitz. B. A. *et al.* (2004). Development of left occipitotemporal systems for skilled reading in children after a phonologically-based intervention. Biol. Psychiat. *55*, 926–933.

Shors, T. J., and Matzel, L. D. (1997). Long-Term Potentiation: What's Learning Got to Do With It? Behav. Brain Sci. *20*, 597–655.

Stanovich, K. E., Siegel, L. S., and Gottardo, A. (1997). Converging evidence for phonological and surface subtypes of reading disability. J. Educ. Psychol. *89*,114–127.