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COMMENTARY

The Promise of Educational Neuroscience: Comment on Bowers (2016)

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Bowers (2016) argues that there are practical and principled problems with how educational neuroscience may contribute to education, including lack of direct influences on teaching in the classroom. Some of the arguments made are convincing, including the critique of unsubstantiated claims about the impact of educational neuroscience and the reminder that the primary outcomes of education are behavioral, such as skill in reading or mathematics. Bowers' analysis falls short in 3 major respects. First, educational neuroscience is a basic science that has made unique contributions to basic education research; it is not part of applied classroom instruction. Second, educational neuroscience contributes to ideas about education practices and policies beyond classroom curriculum that are important for helping vulnerable students. Third, educational neuroscience studies using neuroimaging have not only revealed for the first time the brain basis of neurodevelopmental differences that have profound influences on educational outcomes, but have also identified individual brain differences that predict which students learn more or learn less from various curricula. In several cases, the brain measures significantly improved or vastly outperformed conventional behavioral measures in predicting what works for individual children. These findings indicate that educational neuroscience, at a minimum, has provided novel insights into the possibilities of individualized education for students, rather than the current practice of learning through failure that a curriculum did not support a student. In the best approach to improving education, educational neuroscience ought to contribute to basic research addressing the needs of students and teachers.

Keywords: educational neuroscience, education, instruction, neuroscience, mind, brain, and education

Jeffrey Bowers argues that there are practical and principled problems with educational neuroscience (Bowers, 2016), the branch of human cognitive neuroscience that addresses brain function and structure associated with education. The practical problem is that claims made about the success and promise of educational neuroscience are inconsistent with any examples of neuroscience resulting in improved classroom teaching. Further, although neuroimaging offers correlates of processes related to education, such processes are easier to measure behaviorally, and neuroscience does not offer insights beyond behavioral measures (psychology). The principled problem is that neuroscience cannot determine whether remedial instruction should target underlying deficits or instead develop non-impaired compensatory skills. Further, the only relevant assessment is whether a child has learned as reflected in behavior.

Many of Bower's points are, I believe, correct. There has been an irrational exuberance at the intersection of two of the most inspirational efforts of the 21st century, improving education and discovering how the workings of the human brain endow the capacities of the human mind (Bruer, 1997; Willingham & Lloyd, 2007). The critical measures of education are behavioral—how well does a child learn to read or to calculate. Simply characterizing the brain correlates of mental operations or even the neuroplasticity associated with educational interventions (e.g., Eden et al., 2004; Olulade, Napoliello, & Eden, 2013; Temple et al., 2003) does not provide direct guidance on classroom teaching. Indeed, I have previously noted that cognitive neuroscience has not yet had practical influences on educational practices and policies, but I delineated what I consider the practical and humanitarian ways in which cognitive neuroscience may support improvements in education (and also treatment of neuropsychiatric disorders; Gabrieli, Ghosh, & Whitfield-Gabrieli, 2015). In contrast to Bowers, I believe that neuroscience is starting to make useful contributions to education, and that judicious prioritization of research directions can makes those contributions substantial.

Educational neuroscience is primarily a basic research enterprise that relates to educational theories (Willingham & Lloyd, 2007). The direct goal of educational neuroscience is not that it can "improve teaching in the classroom," although in the long run educational neuroscience ought to contribute to better educational outcomes for students. Further, I think that educational neuroscience is already contributing to ideas about indi-

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vidualized education and early identification of learning difficulties that ought to help advance educational outcomes. I believe that conceptualizing education as being simply about teaching in the classroom underestimates the major goal of education, which is to help all children flourish in their learning and in the educational advancement they will need to support a happy and productive adult life.

Educational Neuroscience as a Basic Science

Educational neuroscience ought to be considered as basic research neuroscience that addresses topics of importance in education, such as learning to read or to perform mathematical calculations. This is analogous to cognitive neuroscience or affective neuroscience or social–cognitive neuroscience research efforts that have addressed the functional brain organization of perception, learning and memory, thought, emotion, and social cognition. The immediate goal of such human cognitive neuroscience has not been the betterment of thought, emotion, or social interaction, but discovery of how the brain empowers such human endowments. We would not reject what we have learned from cognitive, affective, or social human neuroscience because the world does not seem smarter, happier, or more agreeable.

Educational neuroscience may be especially pertinent for the many children with brain differences that make educational progress difficult in the standard curriculum (e.g., dyslexia, attention-deficit-hyperactivity disorder [ADHD], autism, and dyscalculia). In the United States, an estimated one out of eight children receive special education (National Center for Education Services; U.S. Department of Education, Office of Special Education Programs, 2014). Understanding such variation in brain development, coupled with related variation in cognitive, affective, and social development, is a critical basic research goal. The immediate research goal has not been the development of novel teaching methods, but rather a deepening understanding of how brain differences relate to learning differences. It is hoped, of course, that understanding brain correlates of behaviors of interest, such as learning, will ultimately contribute to enhancing the betterment of lives. This is analogous to basic research that aims to understand neuropsychiatric disorders such as depression, anxiety, or schizophrenia. The immediate research goal has been the development of an understanding of the neurobiological basis of such neuropsychiatric disorders, with the hope that a deepening understanding will, in the long run, contribute to practical improvements for people.

There has been especially strong progress in understanding the brain basis of dyslexia, an unexplained difficulty in learning to read (summarized in Gabrieli, 2009). Until 1990, virtually nothing was known about what brain differences are associated with dyslexia. Now, we know that dyslexia is frequently associated with altered neurophysiological response to print in the left fusiform gyrus (around the putative visual word form area) and to sound-print association in left temporo-parietal cortex. We know that these brain differences are independent of IQ (Tanaka et al., 2011; Simos, Rezaie, Papanicolaou, & Flectcher, 2014) and of reading level (Hoeft, Meyler, et al., 2007), and they occur in children before school-based reading instruction (Raschle, Zuk, & Gaab, 2012). We know that the microstructural anatomy of the arcuate fasciculus, the white-matter pathway that connects the major language areas of the left hemisphere, differs in adults with dyslexia (Klingberg et al., 2000), differs in kindergarten children with language difficulties predictive of dyslexia before school-based reading instruction (Saygin et al., 2013), differs in infants at increased risk of dyslexia because of a family history of dyslexia (Langer et al., 2015), and predicts future reading difficulties in kindergartners (Myers et al., 2014). Without such specific neurobiological evidence, it would be impossible for the International Dyslexia Association and the National Institute of Child Health and Human Development to validly define dyslexia as “a specific learning disability that is neurobiological in origin.”

In basic research, convergence of evidence from multiple sources of experimentation is generally considered to be valuable in developing robust and valid theories. Neuroscience can contribute a convergent strand of evidence that inevitably intertwines with behavioral evidence because the brain is the organ of behavior. For example, a major step forward in understanding dyslexia was the discovery that the most common psychological cause for dyslexia is a deficit in auditory processing of language (Bradley & Bryant, 1978), specifically the explicit awareness of the discrete sounds (phonemes) of language that must be mapped onto letters or syllables (graphemes) in print. A fundamental question was whether the phonological difficulty reflected a poor quality of the phonetic representations of language or, instead, a difficulty in accessing and using intact phonetic representations. Convergent behavioral (reviewed in Ramus & Szenkovits, 2008) and neuroimaging evidence (Boets et al., 2013; Kovelman et al., 2012) has supported the view that this core deficit in dyslexia is more likely a difficulty in accessing phonetic representations rather than poor quality in the representations themselves. Another important theoretical issue in dyslexia is whether the reading difficulty is secondary to nonlinguistic processes involved in the perception of visual motion (e.g., Stein, 2001). This possibility was supported by neuroimaging evidence that adults with dyslexia exhibited reduced activation in visual areas responsive to motion (Eden et al., 1996). A subsequent study, however, provided compelling evidence that the altered brain response to such moving stimuli were more likely the consequence of reduced reading experience than the cause of reading difficulty (Olulade et al., 2013).

The above are examples of how the capacity of neuroimaging to reveal the inner workings of the black box of the mind can make unique contributions to education research. Behavioral studies are inherently limited to stimulus-response experimentation, and neuroimaging can visualize and quantify the neural correlates of the multiple processes that intervene between stimulus and response. The combination of behavioral and neuroscience evidence is more powerful than research that is limited to only one source of evidence.

Brain-behavior convergence also relate to policy in educational practice (a factor not considered by Bowers). For example, an important issue in education is the definition of a learning disorder because this definition triggers requirements and resources to help students (requirements that are often costly). For many years, dyslexia was defined by a discrepancy criterion, such that poor readers with a high IQ were diagnosed as dyslexic, whereas poor readers with a low IQ were not considered dyslexic. Neuroimaging evidence that functional brain differences are similar in poor readers regardless of IQ (Tanaka et al., 2011; Simos et al., 2014) converges with behavioral evidence (e.g., Siegel, 1989; Stuebing et al., 2002) in the conclusion that poor readers ought to receive remedial support regardless of IQ. Further, evidence that brain differences near birth are associated with long-term reading outcomes ought to motivate public policies (analogous to public health policies) that promote positive outcomes.

Individualized Education and Prediction

A fundamental and widely acknowledged challenge for improving education is recognizing that students differ one from one another in substantial ways and that an educational approach that is effective for one student may be ineffective for another student. Currently, these differences most often become apparent after educational failure. Better teaching and learning would occur if important student characteristics could be identified at the outset so that curriculum was individualized rather than being offered uniformly on a trial-and-error basis.

Science can offer ideas and methods to identify the characteristics that ought to or ought not to trigger alternative educational approaches for various students. For example, there is no good evidence for the intuitively appealing and widely held idea that students differ from one another in regards to being visual or verbal learners (Pashler, McDaniel, Rohrer, & Bjork, 2008). In contrast, several longitudinal neuroimaging studies have provided evidence that neuroimaging can predict variable response to education in mathematics and reading that, in the present state of knowledge, add to or exceed predictions from conventional behavioral measures.

In one study, third graders received a math tutoring program that encouraged students to shift from counting to fact retrieval as a basis for arithmetic (Supekar et al., 2013). Individual differences in how much students benefitted from the tutoring program did not correlate with baseline behavioral scores on tests of intelligence (IQ), working memory, or even mathematical abilities. Conversely, baseline neuroimaging measures correlated with future learning from this tutoring program (specifically, greater right hippocampal volume and resting-state intrinsic functional connectivity between right hippocampus and prefrontal and striatal regions correlated with greater learning).

Bowers writes that “behavioral findings suggest that the intervention was successful (children improved overall), but the authors do not provide any suggestions about how the biomarkers in each child might be used.” I believe this conclusion about overall improvement misses the most important and exciting discovery in this study—that *before* the educational intervention neuroimaging could provide evidence about individual differences as to which students would benefit more or less from this intervention. None of the behavioral measures predicted these individual educational benefits. The critical value of this finding for education is an existence proof that there are measures that can be used to determine whether or not a student will benefit from a specific math intervention. The students who benefit least may be better off with an alternative educational approach. Identifying objective measures that can guide students toward individually optimized educational curriculum is a major goal for improving education.

Two neuroimaging studies examining baseline brain measures as predictors of longitudinal outcomes in reading ability among students with dyslexia are considered by Bowers. In one study, we examined how children ages 8–12, identified by their teachers as struggling readers, fared from the beginning to the end of a school year in single-word decoding skills (the ability to read aloud pseudowords on the basis of phoneme-grapheme mapping rules; Hoeft, Ueno, et al., 2007). At the beginning of the school year, these children were evaluated with over a dozen behavioral measures of reading and reading-related skills, an functional magnetic resonance imaging (fMRI) task requiring rhyme judgments for pairs of printed words, and a voxel-based morphometry (VBM) analysis of anatomic gray and white matter densities. The beginning-of-the-year behavioral measures accounted for 65% of the variance in end-of-year scores, and the brain measures accounted for 57% of that variance. The *combination* of behavioral and brain measures accounted for a significantly better 81% of the variance, demonstrating enhanced forecasting of student reading skills across a school year.

Among children with dyslexia, there is considerable variation in the degree to which individual children do or do not compensate for reading difficulty by closing the gap between their actual and age-expected reading skills. A longitudinal study of older children (mean age of 14 years) examined how behavioral measures (17 tests of reading and reading-related skills), fMRI activation for a word-rhyming task, and diffusion tensor imaging (DTI) indices of white-matter organization predicted which children, over the next 2.5 years, would compensate or persist in their reading difficulty (Hoeft et al., 2011). None of the standard behavioral measures predicted future reading gains above chance, but the brain measures did predict future reading gains. In combination, greater activation in right prefrontal cortex (a region not typically engaged for reading single words at this age) and white-matter organization of the right superior longitudinal fasciculus predicted with 72% accuracy whether a child would be in the compensated or persistent group. Multivoxel pattern analysis (MVPA) of whole-brain fMRI activation, a data-driven pattern classification analysis, yielded over 90% accuracy in classifying whether a dyslexic child at baseline would belong to the compensating or persistent group 2.5 years later.

Bowers’ response to these studies is twofold. First, rather than highlighting the actual findings by which neuroimaging greatly enhanced the prediction of reading gains across students, he speculates that other kinds of nonimaging measures would have improved prediction, including “low-level visual and attentional factors” or inclusion of “one of the strongest predictors of future dyslexia into their regression studies, namely whether one (or both) of the parents dyslexic. If all these factors were entered in a regression then the unique contribution of imaging data would almost certainly be reduced.”

One can endlessly speculate whether other behavioral or brain measures could have altered the findings. In both studies, we measured many reading and reading-related language skills that likely represented the outcome (i.e., absorbed the variance) of how attentional or low-level visual factors influenced actual reading. In response to the strong speculation made that parental history of dyslexia would have eliminated the documented predictive value of neuroimaging, we have now examined parental history in the students with dyslexia in the 2011 study (Hoeft et al., 2011). We asked whether parental history of dyslexia differed between the students with dyslexia who made substantial progress in reading over 2.5 years versus the students with dyslexia who made less progress. Consistent with the known familial nature of dyslexia, about two-thirds of the students with dyslexia had at least one parent with dyslexia, but the percentages in the two groups were 50% (in the group that did not improve) and 57% (in the group that did improve; $\chi^2 > .6$). This finding is contrary to the speculation that long-term outcomes between equivalently poor readers would be strongly predicted by family history.

Why is prediction important? It is important because prediction helps identify which students with dyslexia respond substantially to current curriculum and which students do not respond to the same curriculum. The students who are predicted to fail to respond to standard curriculum ought to be offered alternative curricula, initially experimentally, to discover what form of instruction is helpful for them. Otherwise, we would perpetually assign these most vulnerable students to an ineffective curriculum and only learn of that ineffectiveness by the prolonged failure of that student.

In the three studies above, the brain measures predicted individual response to education better than many salient behavioral measures. In the end, there has to be a convergence of behavioral and brain measures for prediction because of the intertwined relations of brain and behavior. The neuroimaging studies indicate that such individualized prediction is possible, and perhaps that will motivate the development of new behavioral measures with predictive value. Furthermore, the neuroscience discoveries motivate new basic research questions. What mental process is indexed by the right prefrontal activation that predicted reading improvement? Would an intervention that enhances right prefrontal activation help students who now make relatively little progress in reading?

In response to evidence that neuroimaging measures significantly improve the prediction of which child will benefit from a curriculum, Bowers writes that "a significant increase in prediction is often of little practical utility whether a given child should be given remedial instruction." Of course, all children who need help should be given remedial instruction, but to offer a specific remedial instruction to a child who is unlikely to benefit from that instruction is an unsatisfactory approach to education.

A critical goal for prediction is the early identification of children who will struggle to read. Currently, we often wait for prolonged failure in a child's educational achievement to initiate intervention. In regards to reading, earlier (or preventive) interventions in kindergarten and first grade have yielded better immediate and long-term outcomes than later (or remedial) interventions in older grades (Torgesen, 2001), and by 5th grade these interventions are often ineffective (Torgesen et al., 2007). Further, there are spiraling negative consequences for poor reading. Reading is essential for engaging the entire curriculum, from social studies to science and mathematics. Poor reading in school is exacerbated by massive differences in out-of-school reading that not only enhance reading skills but also expands a child's vocabulary and world knowledge that, in turn, promote reading comprehension. During leisure reading outside of school in 5th grade, a good reader may read as many words in 2 days as a poor reader does in an entire year (Cunningham & Stanovich, 1998). Further, a young child who is struggling to read is prone to developing low self-esteem in relation to academic and other competencies. By all these accounts, earlier interventions are better.

A great obstacle for early intervention is identification of children at true risk for dyslexia. Over the years of primary school, a reading impairment becomes increasingly clear-cut as a child falls further behind his or her peers, but with the passage of time that child has less responsiveness to intervention and greater socioemotional setbacks. In early reading, however, the gap between the good and poor reader is smaller, and many children fluctuate developmentally. Researchers have developed behavioral measures of skills important for learning to read that can be used with prereading children (often a combination of tests of phonological awareness of spoken language, speed of naming, and letter knowledge). These measures correlate with later reading ability, but have limited sensitivity and specificity for early identification. It has been estimated that to identify all of the weakest 10% of beginning readers (the children who would benefit most from early intervention), current behavioral measures would identify 20% of children as being at high risk. The problem with inaccurate identification of at-risk children is that the most effective interventions are resource demanding, involving many hours of individualized or small-group instruction.

Neuroimaging has shown considerable promise in helping with early identification of children at true risk for developing dyslexia. Event-related potentials (ERPs) from scalp electrodes have shown functional brain differences in children with familial risk for dyslexia within hours of birth (Guttorm, Leppanen, Richardson, & Lyytinen, 2001) that correlated with language abilities through age 5 (Guttorm et al., 2005), and that, in another study, have been associated with dyslexia at age 8 (Molfese, 2000). In kindergarten, these brain measures correlate better than behavioral measures with reading level in 5th grade (Maurer et al., 2009). Neuroimaging measures of white matter also reveal anatomical differences in children at familial risk in infancy (Langer et al., 2015), and when measured in prereading children in kindergarten relate to weakness in reading-related language skills (Saygin et al., 2013) and to reading outcomes in 3rd grade (Myers et al., 2014). Although there are as yet few interventions that target preschool children, there is evidence using ERPs as an outcome in 4- to 7-month-old infants that active auditory exposure can modify prelinguistic acoustic mapping (the brain-measure outcomes are helpful for infants with who cannot yet talk or perform tasks; Benasich, Choudhury, Realpe-Bonilla, & Roesler, 2014).

Bowers correctly notes that the brain measures have not yet enhanced sensitivity or specificity for early identification of dyslexia (in part because such studies require years of longitudinal tracking of student outcomes). This analysis, however, ignores the larger problem—that nearly three decades of behavioral research has not improved the sensitivity or specificity of early identification to the degree that schools make early identification and immediate intervention a regular educational practice. Similarly, decades of psychological and education research have not yet found a way to know which struggling student is best served by which curricular intervention. It is simply unknown as to what mixture of behavioral and brain evidence will lead to accurate early identification or accurate selection of effective intervention for a child. If neuroimaging can sometimes outperform conventional behavioral measures in predicting educational outcomes in reading (Hoeft et al., 2011) or math (Supekar et al., 2013), then

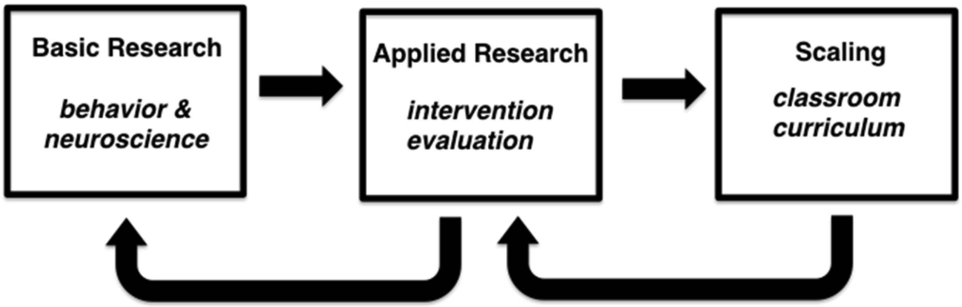

Figure 1. Proposed pipeline organization of educational neuroscience. Educational neuroscience combines with behavioral science to motivate experimental interventions, which, if effective, can be scaled to widespread classroom practice (top row of arrows). Consideration of educational needs inspire basic research directions and prioritize development of interventions (bottom row of arrows).

neuroimaging should be included in research approaches until education practices become more effective on behalf of children.

What Is Educational Neuroscience About and Where Does It Stand?

Bowers' critique raises important questions about the enterprise of educational neuroscience. He cites many examples of superficial claims that improved understanding of how the human brain works will inform teaching and learning, and I agree that many of these claims have not carefully considered how such a translation would or could actually occur. Indeed, I have raised a similar concern about such translation, and suggested that improved prediction of learning difficulties or variation in response to curriculum might constitute such a translational pathway (Gabrieli et al., 2015), but there are not many examples of attempts to precisely consider this challenge of translation. Further, the examples of prediction involve a relatively limited number of studies that need to be replicated and extended to broader samples.

Many of the best examples of possible translation from basic neuroscience research to educational practice come from the study of neurodevelopmental differences, especially dyslexia. It seems plausible that neuroscience may be most revealing for individuals with brain differences that make learning difficult because those brain differences are constraining their education. Students with dyslexia, ADHD, autism, and other brain differences that influence learning may comprise an estimated 15–20% of all students, but the impact of helping such students would have a broader consequence because schools must commit a disproportionate amount of resources to help these students. It is less clear at present how educational neuroscience would translate for more typical students, with perhaps a contribution toward individualized learning.

An Integrated Learning Perspective

In the United States, the relation between basic research and translation of that research into practice has been markedly different in medicine and education. In medicine, there is a strong relation between basic research, often performed in nonhumans, and critical clinical issues of treatment (albeit with many complexities in translation from bench to bedside). In education, there has been a separation of related lines of inquiry, with schools of education focusing on issues of education, and departments of psychology and neuroscience focusing on basic research about learning, language, and reading. If there were steady progress in improving educational practices, this state of affairs would be acceptable.

To the contrary, progress in education has been exceptionally difficult by almost every criterion. In regards to reading, longitudinal evidence indicates that, on average, children who are dyslexic in first grade remain as far behind in reading in 12th grade as they were in 1st grade (educational practices do not on average close the reading gap; Ferrer et al., 2015). In regards to many other educational outcomes, in the United States, the *income-achievement gap*—the difference in academic achievement between students from higher- and lower-income backgrounds—is substantial and growing (Reardon, 2011), and is evident from the beginning of school and culminates in wide disparities in high school and college completion (Duncan & Magnuson, 2011). Current educational practices are insufficient for many vulnerable students.

I believe, contrary to Bowers, that progress in education is best served by integrating basic research (including educational neuroscience, but also all other forms of relevant evidence) into a pipeline that extends from basic research to small-scale applications (such as randomized controlled trials or RCTs) to scalable classroom curriculum (see Figure 1). There are enough promising findings from educational neuroscience, including specific findings where neuroscience measures outperform current behavioral measures, that neuroscience ought to remain an integral component of basic research in education. To paraphrase George Bernard Shaw, I believe that Bowers sees the limitations of educational neuroscience and asks "Why," and I see the many needs of children and the promising and novel evidence that educational neuroscience is providing about brain differences that can transform individualized education, and say "Why not?"

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