

Inadequate Evidence for Multiple Intelligences, Mozart Effect, and Emotional Intelligence Theories

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I (Waterhouse, 2006) argued that, because multiple intelligences, the Mozart effect, and emotional intelligence theories have inadequate empirical support and are not consistent with cognitive neuroscience findings, these theories should not be applied in education. Proponents countered that their theories had sufficient empirical support, were consistent with cognitive neuroscience findings, and should be applied in education (Cherniss, Extein, Goleman, & Weissberg, 2006; Gardner & Moran, 2006; Rauscher & Hinton, 2006). However, Gardner and Moran offered no validating evidence for multiple intelligences, Rauscher and Hinton concluded that “listening-to-Mozart” studies should be disregarded, and Cherniss, Extein, Goleman, and Weissberg agreed that emotional intelligence lacked a unitary empirically supported construct. My reply addresses theory proponents’ specific criticisms of my review and reasserts my original claims.

In “Multiple Intelligences, the Mozart Effect, and Emotional Intelligence: A Critical Review” (Waterhouse, 2006), I argued that “MI theory has no validating data ... the Mozart effect theory has more negative than positive findings, and EI theory lacks a unitary empirically supported construct.” I also argued that these theories’ brain system claims were not consistent with relevant cognitive neuroscience findings and concluded that until these theories have garnered reasonable evidentiary support they should not be applied in education. Theory proponents counterargued that their theories were well supported by both behavioral research and cognitive neuroscience findings and should continue to be applied in education (Cherniss, Extein, Goleman, & Weissberg, 2006; Gardner & Moran, 2006; Rauscher & Hinton, 2006).

Gardner and Moran (2006) affirmed the importance of empirical evidence for multiple intelligences (MI) theory, stating that “Theories such as evolution or plate tectonics or MI develop through the continuing accumulation of evidence.” They claimed that abundant empirical evidence for MI theory existed in the studies Gardner relied on to develop his theory, but this claim conflates theory generation and theory validation. They also claimed that evidence for cognitive systems such as reasoning and natural kind categorization of-

ferred support for MI theory, but they provided no proof for this assertion.

Rauscher and Hinton (2006) conceded that “listening-to-Mozart” studies do have too many negative findings to warrant being applied to the classroom. They argued instead that skills developed in playing a music instrument transfer to spatial skills, and thus music instruction studies have findings that are important for education. However, the concept of transfer lacks adequate empirical support (Barnett & Ceci, 2002; Mayer, 2004; Perruchet & Vinter, 2002).

Cherniss et al. (2006) agreed that “conflicting constructs continue to characterize EI theory,” but they viewed these conflicts as a sign of vitality. Cherniss et al. also argued that EI had significant predictive validity but they provided limited evidence to support this claim.

In the discussions that follow I address theory proponents’ criticisms of my review and offer arguments to explain problems I found in their criticisms.

MI THEORY LACKS VALIDATING EMPIRICAL EVIDENCE

Gardner and Moran (2006) asserted that I erred in claiming that MI theory lacked empirical support, that I misconstrued the conceptual basis of MI, that I misunderstood the definitions of several intelligences, and that I had a naïve view of science that limited my ability to value Gardner’s MI theory.

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The following discussions respond to these criticisms and outline two important evidence problems that Gardner and Moran failed to address.

Gardner and Moran's Proposed Evidence Does Not Validate MI Theory

Gardner and Moran (2006) offered four evidentiary claims for MI theory. First, they claimed that MI theory was empirically validated by the fact that "Gardner combined the empirical findings of hundreds of studies from a variety of disciplines" to develop MI theory. However, theory validation is a process distinct from theory generation. The studies Gardner read that led him to hypothesize that there might be MI may serve to warrant the reasonableness of his hypothesis, but the studies he read cannot validate the existence of MI.

Second, Gardner and Moran (2006) argued that MI subcomponents were supported by empirical evidence for "many specific neural systems ... like theory of mind, recognition of natural kinds, understanding of self, understanding of others," and by evidence for "systems of numerical, linguistic, and causal reasoning." Gardner and Moran further argued that "modules identified by evolutionary psychologists, contrary to Waterhouse's argument that they refute MI theory, actually align very well with Gardner's intelligences and their subcomponents." However, Gardner and Moran did not supply a crucial specification: Which multiple intelligence is supported by what evidence for which neural system or adapted cognition module? For example, Astuti, Solomon, and Carey (2004) reported findings for studies of natural kind conceptualization in Madagascar. Natural kind conceptualization includes, among other things, seeing objects, animals, and other humans; labeling objects and individuals; grouping objects and individuals; and conceptualizing categories of groupings. Consequently, Astuti et al.'s evidence for natural kind categorization "aligns with" at least four intelligences: the visual spatial intelligence, the naturalistic intelligence, the linguistic intelligence, and the interpersonal intelligence.

Examination of individual adapted cognition modules and cognitive systems revealed that their specific behavioral components aligned with more than one multiple intelligence (Waterhouse, 2006), thus cutting across the boundaries of Gardner's intelligences. Consequently this evidence does not provide empirical support for the intelligences but, conversely, argues against the framework of MI.

A related problem is that neither Gardner nor any of his adherents has defined a set of testable psychological subcomponents for each of the intelligences (Allix, 2000). Gardner (2004) asserted that because his "basic paradigm clashes with that of psychometrics" (p. 214), and because testing "results may well be misused," he will not define testable subcomponents for the intelligences. Without such subcomponents, the intelligences are defined only by general descriptions (Gardner, 1983, 1999, 2004), and the generality of these descriptions has prevented researchers from

conducting studies to explore the validity of the intelligences (Allix, 2000).

Gardner and Moran's (2006) third evidentiary claim was that Gardner, Feldman, and Krechevsky (1998) reported empirical evidence for multiple intelligence profiles in pre-school children. However, this 1998 report is inadequate as support for MI theory. No formal data analysis was presented, and the half-page discussion of findings is too brief and too vague to be used by other researchers:

Assessments did identify distinctive profiles for a majority of children ... every child exhibited at least one strength ... there was little correlation between the children's performances on the different activities ... because of the small sample size (39 subjects), our results must be regarded as tentative. (Gardner et al., 1998, p. 27)

Fourth, Gardner and Moran (2006) claimed that MI theory will ultimately accrue evidence comparable to that for evolutionary theory and plate tectonics theory. Even though only 23 years have elapsed since Gardner first proposed MI (1983), Gardner and Moran's claim can already be seen to be mistaken. Unlike MI theory, evolutionary theory and plate tectonics theory accrued important validating empirical findings quite soon after they were proposed. In the 23 years following Darwin's publication of *Origin of Species* in 1859 (Appleman, 2000), other scientists presented an array of fossil and faunal evidence in support of Darwin's theory (Bowler, 1986). In the 23 years following the emergence of plate tectonics theory in the 1960s, data collected from ocean floor mapping, magnetic rock record measurement, radiometric dating of the Earth's magnetic pole reversal history, and precise location of earthquake sites provided strong validating empirical evidence for plate tectonics theory (Oreskes, 2003). MI theory has accrued no such validating empirical evidence in the 23 years since it was proposed.

The Multiple Levels of MI

Gardner and Moran (2006) argued that I misconstrued the intelligences as skills because I failed to "encompass the several levels on which MI theory examines intelligences." Gardner and Moran proposed (a) the finer level of neurological subcomponents of each intelligence, (b) the middle road level of the intelligences, and (c) the broader level of skills that use the intelligences "to produce proficient and/or expert behavior."

Contrary to Gardner and Moran's (2006) claim, I did understand these levels. My review outlined cognitive systems of the same explanatory scope (middle road level) as MI whose findings countered the nature and boundaries of MI (Waterhouse, 2006). In their response Gardner and Moran redefined the "What is it?" and "Where is it?" cognitive systems down from middle road level to finer level neurological subcomponents of the intelligences. They also redefined

Kahneman's decision-making processes up from middle road level to broader level skills that would deploy Gardner's intelligences. These down-a-level and up-a-level redefinitions had the effect of side-stepping the evidence against MI theory that these systems provide. Moreover, Gardner and Moran's redefinitions were inaccurate.

What is it and where is it processing streams could not be MI subcomponents. The brain's primary visual cortex sends visual information along both the ventral (what is it) and dorsal (where is it) processing streams, wherein neurons further downstream learn to respond to increasingly organized sets of features (Deco & Rolls, 2005). These two streams begin with shared visual information, but the dorsal stream moves to incorporate body-in-space processing, and the ventral stream moves to incorporate auditory processing. Thus, each stream's mixed content processing precludes it from being construed as a subcomponent of any individual multiple intelligence.

Kahneman's systems could not be domain skills deploying the intelligences. Briefly, in Kahneman's (2003) prospect theory, Systems I and II govern decision making by predicting utility value (Will this decision be good or bad?). System I is spontaneous but rigid and is probably based in limbic and basal ganglia neural circuits, whereas System II is effortful but flexible and is probably based in frontal lobe functions (Trepel, Fox, & Poldrack, 2005). System II operates at the concept level, and System I operates both at the percept and concept level (Kahneman, 2003). Because the percept level is the same as Gardner and Moran's finer level, and the concept level is the same as Gardner and Moran's middle level, therefore neither System I nor System II could be interpreted as broader level skills deploying the intelligences.

Understanding Definitions of Specific Intelligences

Gardner and Moran (2006) were correct in stating that I believed Gardner (2004) had proposed two additional types of intelligence. Although Gardner may have intended something different, nonetheless both the content and parallel structure of his published text argue that there are two MI profiles, each yielding a separate form of intelligence. The text posits that individuals with high IQs have "a mental searchlight" (intelligence), whereas individuals with jagged MI profiles have "a laser-form of intelligence" (Gardner, 2004, p. 217).

Gardner and Moran (2006) argued that I was wrong to state that intrapersonal and interpersonal intelligences were combined into a personal intelligence. However, Gardner (1983, 1999) did treat these two intelligences as part of a larger whole of "personal intelligence" (1983, chapter 10; 1999, p. 43). For example, Gardner (1983) stated that "it is

important not to gloss over differences between the personal and other forms of intelligence" (p. 240).

Gardner and Moran (2006) further argued that I was wrong to include empathy for natural things as part of the naturalist intelligence. However, Gardner (1999) proposed that the naturalist intelligence involved biophilia, wherein the naturalist "may well possess the talent of caring for, taming, or interacting subtly with various living creatures" (p. 49). The word empathy does not seem to me to be wide of the mark as a summary incorporating "biophilia" and "caring for" and "interacting subtly."

My View of Science is Based on the Practice of Science

Gardner and Moran (2006) argued that I had a "naïve view of science" that prevented me from being able to "acknowledge or understand the enterprise in which Gardner has been engaged." My view of science is exemplified in my practice of science. For example, our research group synthesized neuroscience and behavioral findings on attachment to theorize that abnormalities in the neurohormone oxytocin might contribute to attachment impairments found in autism (Modahl, Fein, Waterhouse, & Newton, 1992; Waterhouse, Fein, & Modahl, 1996). Our group then conducted empirical studies of this hypothesis in which we did find evidence for abnormalities in oxytocin in autistic individuals (Green et al., 2001; Modahl et al., 1998). Since that time other researchers have conducted a wide range of related research, and genetic evidence has linked an oxytocin receptor gene with autism (Wu et al., 2005).

In fact, Gardner and Moran (2006) argued that my view of science is naïve because I do not view Gardner's synthesis of research findings as validating evidence for MI theory. Syntheses are important because they can summarize the current state of research, can identify studies that should be conducted, and can yield new theories. However, if a new theory, such as MI theory, is generated by the synthesis of existing findings, then that new theory requires empirical validation.

MI Theory Problems That Gardner and Moran Failed to Address

Gardner and Moran's (2006) response failed to address two problems for MI theory outlined in my review. Gardner claimed that the successful application of MI theory in education provided empirical support for MI theory (Gardner, 2004, p. 214; Gardner & Connell, 2000, p. 292). However, applying MI cannot provide evidence to validate the intelligences because the act of applying MI theory *assumes* the validity of the intelligences. Gardner and Moran offered no response to this problem.

Second, Gardner (1999) asserted that MI theory depends on each intelligence having its own neural processing circuit, arguing that if "musical and spatial processing were identi-

cally represented” in neural circuits “that fact would suggest the presence of one intelligence, and not two separate intelligences” (p. 99). My review outlined evidence for shared neural circuits for the processing of many different types of content (Waterhouse, 2006). Gardner and Moran (2006) offered no response to this evidence.

In summary, Gardner and Moran (2006) provided no validating research evidence for MI theory, and they sidestepped the problem that neuroscience findings for other cognitive systems cut across MI boundaries. They were mistaken in their claim that a theory based on a synthesis of research requires no empirical validation, and they did not address the problem that application research cannot validate MI theory. Finally, although Gardner (1999) claimed that MI theory would be nullified if the neural processing circuits for different contents were found to be shared (p. 99), Gardner and Moran offered no response to evidence that neural processing circuits for different contents are shared.

THE MOZART EFFECT AND MUSIC LESSON TRANSFER

Rauscher and Hinton (2006) argued that I misconstrued the concept of transfer and misrepresented the contents of a review article by Schellenberg (2003). Their major criticism, however, was that I was wrong to lump listening-to-Mozart studies together with music instruction studies because “instruction studies, unlike the listening studies, have profound implications for educational practice. The following sections address these criticisms.

Transfer From Instrument Practice to Spatial Skills is “For Free” Learning

I argued that Rauscher’s (2002) claim that music will lead to improvement in spatial cognition (p. 276) meant that spatial skill improvement occurred for free. Rauscher and Hinton countered that because I lumped listening and lesson studies together I failed to understand that improved spatial cognition transferred via music lessons was not for free but was, instead, effortful, because children expended effort in practicing their instruments. Contrary to Rauscher and Hinton’s claim, however, transfer is not effortful, it is for free learning. For example, if a student practiced the violin daily, and without any practice in origami paper folding, showed enhanced origami folding skills, then no matter how effortful the violin practice was, the paper folding skill improvement is for free because no effort was expended in practicing origami.

More important, Barnett and Ceci (2002) reviewed research on transfer and concluded that, despite 100 years of research, no clear evidence has emerged, and many researchers believe there is no experimental design that can determine whether or not transfer exists (p. 634). Mayer (2004) also reviewed transfer research and concluded that

there was no evidence for general learning transfer, and no evidence for specific skill transfer, but there was some evidence for specific transfer of general knowledge (pp. 217–218). Perruchet and Vinter (2002) reported that “totally negative results are certainly the most frequent outcome” in transfer research (p. 319).

Schellenberg’s (2003) Claims Concerning Music Lesson Effects

Rauscher and Hinton (2006) argued that I misrepresented Schellenberg (2003) by citing his article as support for the claim that transfer from music to spatial skill had not been demonstrated. Rauscher and Hinton argued that Schellenberg’s (2003) statement that “positive transfer effects to nonmusical domains, such as language, mathematics, or spatial reasoning could be similarly unique for individuals who take music lessons” (p. 444) meant that transfer from music to spatial skills had been demonstrated. However, Schellenberg’s statement (2003, p. 444) was hypothetical, as framed by this preceding statement: “If we suspend our disbelief, however, and assume that music education affects abilities ... how could we account for this influence?” (p. 443). In fact, Schellenberg (2003) proposed that “music lessons ... may confer benefits by providing close and extended contact with an adult other than a parent or teacher” and that “similar effects should be evident with ... chess and drawing” lessons (p. 444).

Music Instruction Studies Do Not Have “Profound Implications” for Education

Rauscher and Hinton (2006) are to be commended as scientists for their forthright review of the evidence for their own and others listening-to-Mozart studies. They concluded that “Given the contradictory findings of the studies on children, we agree with Waterhouse that educational practice should not be influenced by this area of research.”

Rauscher and Hinton (2006) argued that music instruction studies, by contrast, have clearly demonstrated that skills developed in playing a musical instrument do enhance spatial skills. They cited two published studies (Rauscher et al., 1997; Rauscher & Zupan, 2000) and a review (Hetland, 2000). Rauscher et al. (1997) reported that, unlike the control group, 34 young children given piano keyboard lessons showed spatial reasoning improvement that lasted for 1 day, and Rauscher and Zupan (2000) reported that 34 young children given 8 months of keyboard lessons did significantly better in creating an object from pieces than did controls. Hetland (2000) reviewed 15 studies of music instruction’s association with improved spatial skills. However, only 6 of the 15 studies were published, and 2 of these 6 were the Rauscher studies discussed previously (Rauscher et al., 1997; Rauscher & Zupan, 2000). Moreover, 1 of the remaining 4 published studies was not directly relevant because it

used music and spatial training to enhance math skills (Graziano, Peterson, & Shaw, 1999). Of the 3 remaining published studies Hetland reviewed, only 1 reported an effect with a p value of .05 or below (Costa-Giomi, 1999).

Thus, Rauscher and Hinton (2006) claimed “profound implications for educational practice” based on three published studies that linked music instruction to spatial skill enhancement—two of which are from Rauscher’s own group. These studies are promising, but insufficient at present to hold “profound implications” for education.

All Forms of Music’s Effect on Spatial Skills Should Be Considered Together

Rauscher and Hinton (2006) stated that “Waterhouse’s conflating listening studies with the music instruction studies will lead to greater misinterpretation of the research by educators, politicians, and laypeople.” I have no wish to add to misinterpretation of current findings, but I believe it is of value to try to establish a comprehensive understanding of all reported music effects on spatial cognition.

Rauscher and Hinton (2006) proposed three brain mechanisms for music’s ability to cause improved spatial cognition: transfer, cortical arousal, and synaptic plasticity. Rauscher and Hinton argued that music lessons provided spatial skill transfer and cortical arousal, each of which separately contributed to spatial skill improvement. They also proposed that because rats exposed to a Mozart sonata demonstrated improved maze learning and exhibited changes in brain synapses (Chikahisa et al., 2006), therefore, synaptic changes could be the cause of skill transfer.

Although current research findings do not support the notion of transfer, neuroscience findings do suggest functional connections among synaptic change, cortical arousal, repetition, and overlapping neural circuits for different forms of content. Brief repetition and cortical arousal confer short-term enhancement of neural circuit activity, but long-term motor skills, perceptual skills, and content memories depend on synaptic and other neural changes that occur when there has been extended repetition of in the circuitry underwriting those skills and memories and when there has been associated cortical arousal (Phelps, 2006; Squire & Kandel, 2000). If there are general cross-content processing neural circuits, formed by generalist genes (Kovas & Plomin, 2006), there may be cross-content enhancement of memory.

Consequently, from what is known, the following speculative model could be proposed. Short-term exposure to music provides general cortical arousal, as well as some specific (priming) repetition of shared or overlapping circuits for music and spatial processing, which together may contribute to a brief enhancement of spatial skills. Longer term exposure to music, whether through extensive auditory exposure only (as in the rat studies) or through extensive auditory exposure as part of instrument practice, provides repeated and extended cortical arousal and extensive repetition of the firing of

shared and overlapping neural circuits for music and spatial skills. These extended effects, in turn, cause changes in gene expression, and these changes in gene expression may lead to reorganization of synaptic structures (and other forms of remodeling of neural circuits). The structural changes may support durative enhanced spatial skills.

This speculative model is consistent with neuroscience findings and offers a coherent account of results from the different types of music effect studies. It also provides an explanation for the fact that spatial skills are not the only cognitive skills be found to be enhanced by music experience (Schellenberg, 2004). Equally important, the model accounts for spatial skill enhancement through music exposure or music instruction without invoking the unsupported notion of transfer, and without resorting to a claim for a novel, previously undiscovered cognitive process.

CONFLICTING CONSTRUCTS, FINDINGS, AND CLAIMS FOR EMOTIONAL INTELLIGENCE (EI)

Cherniss et al. (2006) argued that I was wrong to view multiple conflicting EI measures and constructs as a problem, wrong to argue that EI has limited predictive validity, wrong to assert that Goleman claimed that EI accounts for more than 80% of success, wrong to propose that EI was unlikely to have a discrete neural system, and wrong to argue that EI should not be applied in education. The following sections address these five criticisms.

Lack of a Validated Unitary EI Construct Remains a Problem

Cherniss et al. (2006) argued that the many conflicting EI constructs are not a stumbling block for EI research. However, the competing EI constructs demonstrate that EI is poorly understood and make generalization across studies extremely difficult. Van Rooy and Viswesvaran (2004) reported that studies of EI “have not used the same, or even a few of the same, measures of EI” (p. 74). Moreover, efforts to reconcile measures have been unsuccessful. For example, Gignac, Palmer, Manocha, and Stough (2005) reported that a confirmatory factor analysis could not even reconcile an offspring measure of EI with its parent measure. Goldenberg, Matheson, and Mantler (2006) could not demonstrate convergence of two measures of EI, the Mayer-Salovey-Caruso Emotional Intelligence Test (MSCEIT) and SREIS, in a community sample of 223 individuals. Correlations between scores from the two EI measures for their three comparable subscales (perceiving emotion, $r = -.03$; using emotions, $r = -.02$; managing emotions, $r = .04$) were essentially zero (Goldenberg, Matheson, & Mantler, 2006, p. 39).

Murphy (2006) reviewed the state of research on measures of EI and concluded that not only are existing measures

of EI inconsistent with one another but current constructs of EI for which there are, as yet, no measures are so conceptually unclear that these constructs will not be able to be translated into measures.

EI Has Limited Predictive Validity

Cherniss et al. (2006) argued that, contrary to my review, five published studies reported that EI has significant predictive validity for a variety of life outcomes. However, these five studies do not provide strong support for EI. One study assumed that attitudes, job skill, and leadership factors that differentiated better collection agents were subserved by EI (Bachman, Stein, Campbell, & Sitarenios, 2000), and another reported only modest correlations for EI and leadership (Rosete, & Ciarrochi, 2005). Lopes, Salovey, and Straus (2003) expressed doubt about EI, concluding that “it is unclear to what extent we are truly assessing skill, rather than conformity or adjustment to social norms” (p. 655). Lopes, Salovey, Côté, and Beers (2005) reported that only one of four self-report EI subscales, emotional regulation, was associated with social adaptation (p. 5) and concluded that EI skills “are likely to have only a modest impact on the quality of social interactions” (p. 4). Moreover, the fifth study was a meta-analysis of EI studies that revealed that EI did not have predictive validity beyond that found for general intelligence, but general intelligence did “significantly predict performance beyond that explained by EI” (Van Rooy and Viswesvaran, 2004, p. 87). Van Rooy and Viswesvaran (2004) concluded that “the claims that EI can be a more important predictor than cognitive ability (e.g., Goleman, 1995) are apparently more rhetoric than fact” (p. 87).

Van Rooy and Viswesvaran’s (2004) meta-analysis determined that the correlation between EI and work performance was .24 and between EI and academic performance was .10 (p. 86). Thus, EI predicted only 1% of the variance in academic performance and only 8% of job performance variance. Similarly, Bastian, Burns, and Nettelbeck (2005) reported that only 6% of the variance in life skills could be predicted by EI (p. 1143).

Cherniss et al. (2006) cited Judge, Colbert, and Ilies (2004) to argue that “IQ and other tests of cognitive ability account for no more than about 25 percent of the variance in outcomes.” However, Deary, Strand, Smith, and Fernandes (2006) reported that intelligence scores predicted 48% of the variance of performance on General Certificate of Secondary Education exams, and Rindermann and Neubauer (2004) similarly found that intelligence scores predicted 43% of the variance in academic achievement. Schmidt and Hunter’s (1998) meta-analysis found that general intelligence “g” is the most valid predictor of job performance, and Gottfredson (1997) reviewed meta-analyses of the predictive validity of intelligence measures for job performance and reported a range of predictive validity from 23% to 65%. Thus, contrary to Cherniss et al.’s claim, studies have reported that general

intelligence accounts for more than 25% of the variance in academic and job performance.

As noted by Cherniss et al. (2006), Van Rooy and Viswesvaran (2004) found that EI had incremental predictive validity in relation to personality factors (p. 86). However, the EI basis for this increment is unclear, and the increment is small. Gannon and Ranzijn (2005) found that EI added only 1.3% beyond the 34.2% of variance in life satisfaction accounted for by personality. Personality dimensions, in general, have been reported to have high predictive validity for job performance. Hogan and Holland (2003), for example, found that emotional stability predicted 43%, extraversion 35%, agreeableness 34%, conscientiousness 43%, and openness to experience 34% of variation in job performance.

No Ambiguity in Goleman’s Claim That EI Accounts for More Than 80% of Success

Cherniss et al. (2006) offered no rebuttal of my claim that Goleman’s 80% figure is a subjective judgment mistakenly presented as “recent studies” (Waterhouse, 2006). Goleman examined a list of 21 job skills that he got from an unpublished privately commissioned study (Goleman, 1998, p. 31) and decided that 18 of the 21 skills were EI skills; thus, as 18 equals 85.7% of 21, he judged that EI explained more than 80% of life success (Pool, 1997, p. 12) or more than 80% of job skill competencies of superior workers (Goleman, 1998, p. 320).

In place of a direct rebuttal, Cherniss et al. (2006) suggested that I had misunderstood the ambiguities in Goleman’s work. However, Pool’s (1997) lecture report and Goleman’s (1998) published statements are not ambiguous. Pool did state that Goleman told members of the Association for Supervision and Curriculum Development that “IQ predicts only a small part of career performance—ranging from 4 to 20 percent. But recent studies have shown that emotional intelligence predicts about 80 percent of a person’s success in life” (p. 12). Goleman (1998) did claim that “IQ alone at best leaves 75 percent of job success unexplained, and at worst 96 percent” (p. 19), and Goleman (1998) did claim that “more than 80 percent of general competencies that set apart superior from average performers depend on emotional intelligence” (p. 320).

No Evidence for Neural Circuits for EI

Although at present no research has identified neural bases for EI, Cherniss et al. (2006) argued that EI and IQ neural circuits are separate, that EI depends on subcortical systems and IQ on prefrontal cortex, and that EI includes discrete brain systems for mindsight (recognizing that others have their own thoughts) and for face recognition.

Cherniss et al.’s (2006) brain claims for EI ignore the fact that behavioral studies have consistently reported significant correlations between EI and IQ and between EI and personality (Schulte, Ree, & Carretta, 2004; Van Rooy & Viswesvaran,

2004). Consequently, theorizing about the brain circuits for EI should include consideration of the evidence for structural and neurochemical brain bases for general intelligence (Shaw et al., 2006) and for personality (Paris, 2005). Moreover, instead of considering only mindsight and face recognition, speculative models should address the full range of existing evidence for neural bases for EI component behaviors such as attachment, empathy, face and emotion recognition, emotional sensation, emotional expression, the mirror neuron system, language skills, personality components, working memory, long-term memory, reasoning, decision making, and others (Waterhouse, 2006).

EI Is Not a Basis for Moral Conduct

Cherniss et al. (2006) claimed that programs such as social emotional learning (SEL) could be used to “enhance positive youth development and mental health, reduce substance use and antisocial behavior, and improve educational outcomes.” However, because no one yet knows what EI represents, beyond general mental ability and personality components already identified as part of EI, and because there is no empirically validated unitary construct of EI (Murphy, 2006), therefore it remains premature to apply EI to education. Furthermore, a review has suggested that there is insufficient evidence for the beneficial effects of SEL programs (Kristjansson, 2006).

Another problem of significance is that EI training has been implied to be moral education. For example, Cherniss et al. (2006) argued that EI/SEL training can reduce discipline problems as well as make students more caring and responsible. However, in fact, nothing in any EI construct precludes someone with high EI from being an immoral person. Kristjansson (2006) analyzed whether or not components of EI reflected moral principles, and he concluded that “EI lacks moral depth and does not exclude the possibility that a calculated Machiavellian personality can be deemed emotionally intelligent” (p. 17).

In summary, none of Cherniss et al.’s (2006) five criticisms survived close examination. Researchers do not yet know what the conflicting measures for EI are actually measuring. The five studies published in academic journals that Cherniss et al. outlined as evidence for EI did not provide strong empirical support for EI, and one of the five, a meta-analysis of EI studies (Van Rooy & Viswesvaran, 2004), found that EI predicted only 1% of the variance in academic performance and only 8% of the variance in workplace performance. Goleman did claim that EI predicted 80% of life and work performance. No research has, as yet, provided evidence for the possibility that there are unique brain circuits for the two core domains of EI. Finally, as EI components contain no moral principles, proponents should desist from implying that EI school programs can provide moral education.

CONCLUSION: PERSISTING WITHOUT ADEQUATE EVIDENCE

Although Gardner and Moran (2006), Rauscher and Hinton (2006), and Cherniss et al. (2006) claimed that there was a wealth of empirical support for their theories, Gardner and Moran offered no research evidence to validate MI, Rauscher and Hinton included only three published music instruction studies with significant positive findings for spatial skill enhancement, and Cherniss et al. provided five published studies whose findings did not provide strong support for the predictive validity of EI.

Despite their inadequate empirical bases, these theories have wide currency and, unfortunately, may continue to be applied in education because they tell “good news” stories. Gardner’s MI theory tells us the story that we each have eight forms of intelligence, so there is likely to be one in which we can shine. Rauscher’s music transfer theory offers spatial skill improvement through music lessons—a cognitive bonus for keeping music in the curriculum. Goleman’s EI theory tells the story that job and life success depends much more on our EI than our IQ, with the good news that we can increase our EI.

Tilly (2006) argued that there are four modes of explanation: conventions (accepted reasons for events and actions), stories (simple cause and effect accounts), codes (sets of rules such as legal judgments), and technical accounts (systematic discipline-based empirical explanations). Gardner and Moran (2006), Rauscher and Hinton (2006), and Cherniss et al. (2006) argued that MI, the music instruction effect, and EI were validated technical accounts of brain systems. In the absence of adequate validating empirical support, and in the absence of concord with neuroscience findings, these three theories are not validated technical accounts. Therefore, at present, despite their appeal, they should not be applied in education.

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