EMOTIONAL INTELLIGENCE:

ON DEFINITIONS, NEUROSCIENCE, AND MARSHMALLOWS

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Commentary on focal article by Cary Cherniss

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In his timely article, Cherniss offers his vision for the future of "Emotional Intelligence" (EI). However, his goal of clarifying the concept by distinguishing definitions from models and his support for "Emotional and Social Competence" (ESC) models will, in our opinion, not make the field advance. To be upfront, we agree that emotions are important for effective decision-making, leadership, performance and the like; however, at this time, EI and ESC have not yet demonstrated incremental validity over and above IQ and personality tests in meta-analyses (Harms & Credé, 2009; Van Rooy & Viswesvaran, 2004).

If there is a future for EI, we see it in the ability model of Mayer, Salovey and associates (e.g, Mayer, Caruso, & Salovey, 2000), which detractors and supporters agree holds the most promise (Antonakis, Ashkanasy, & Dasborough, 2009; Zeidner, Roberts, & Matthews, 2008). With their use of quasi-objective scoring measures, the ability model grounds EI in existing frameworks of intelligence, thus differentiating itself from ESC models and their self-rated trait inventories. In fact, we do not see the value of ESC models: They overlap too much with current personality models to offer anything new for science and practice (Zeidner, et al., 2008).

In this commentary we raise three concerns we have with Cherniss's suggestions for ESC models: (1) there are important conceptual problems in both the definition of ESC and the distinction of ESC from EI; (2) Cherniss's interpretation of neuroscience findings as supporting the constructs of EI and ESC is outdated, and (3) his interpretation of the famous marshmallow experiment as indicating the existence of ESCs is flawed. Building on the promise of ability models, we conclude by providing suggestions to improve research in EI.

1. Definitional Problems

Cherniss attempts to distinguish ESCs from EI. For EI, he adopts the ability model's definition as an individual-difference *ability* applied to perception and expression of emotion, use of emotion in thought and reasoning, and self- and other-emotional regulation. For ESCs, he uses a definition by Boyatzis stating that ESC's are EI-related qualities "of the person that lead(s) to or causes effective or superior performance." This latter definition is problematic on three grounds.

First, the definition of ESC is defined by its outcome (i.e., performance) and its cause (i.e., EI). Thus, ESC does not exist in the absence of EI or performance. As MacKenzie (2003, p. 325) noted, defining a concept by its causes and its outcomes "is not helpful because this definition does not specify the 'nature' of . . . the construct. Defining a construct in this manner also makes it impossible to empirically test the proposed theoretical linkages between the construct and the specific antecedents and consequences mentioned, because these relationships are assumed to be true by definition." Cherniss's approach seems to confuse defining a construct with embedding it into a nomological network. The latter is an important aspect of construct validation but does not substitute for a definition that establishes the unique properties of a construct independent of its relationships to other variables.

Second, his conceptual distinction between EI and ESC is unclear: He suggests that EI is an ability and ESC is a competency. Cherniss, however, also refers to competencies as abilities. This confusion about abilities and competencies is not surprising. Boyatzis (2008), whose definition of competency Cherniss also cites, defined an "emotional intelligence competency" as "an ability to recognize, understand, and use emotional information about oneself … that leads to or causes effective or superior performance" (p. 228). So what is ESC: An ability or a competency? If it is the former, ESC or EI is redundant. If it is the latter, ESC remains undefined.

Third, in delineating models and definitions of EI, Cherniss stated that EI provides "a useful catalogue of the personal qualities, *other than cognitive intelligence*, that most strongly aid adaptation" [italics ours]. Thus, EI models are defined by exclusion--a "laundry list of virtually every positive quality of character *except* for cognitive intelligence" (Zeidner, et al., 2008, pp. 64-65). Such reasoning does not help the field advance because it does not prospectively define the

EI construct; that is, this definition does not specify what exactly is included in the construct. It opens the door to pass off established constructs such as personality traits as EI. Conceptual confusion and imprecision undermines construct validation. As Matthews, Zeidner, and Roberts (2002, p. 45) mentioned, “a test should not be labelled a measure of EI when really it is a measure of some, other well-established personality-trait or related individual-difference variable. . . . If this practice were repeated throughout the scientific community, thousands of new (but redundant) tests would flood the market each year.” This redundancy is currently evidenced in the fact that EI tests, whether ability- or ESC-based, do not reliably predict incremental variance in performance outcomes (Harms & Credé, 2009; Van Rooy & Viswesvaran, 2004).

2. Neuroscience Findings Support IQ and not EI

Cherniss uses dated findings by Damasio (1994) to highlight cases similar to that of the famous Elliot case--briefly, Elliot (and related cases) had damage to the prefrontal cortex yet apparently showed normal intelligence; however, he could not make decisions because part of his emotional decision-making circuitry (in the ventromedial prefrontal cortex, or VM) was damaged. Damasio used this case and others to suggest that individuals with brain-damage might exhibit normal intelligence but that, because of damage to emotional circuitry, these individuals could not make decisions. Cherniss, as have many EI advocates, employs these clinical cases as evidence for EI, suggesting that the emotional component of decision-making *is* EI.

Part of the argument here, with which we agree, is that the brain uses emotional memories associated with the decision-making task at hand (“emotional somatic markers”), which are automatically called up and help to guide decision-making (see Letter 3 in Antonakis, et al., 2009). That is, with experience, individuals associate good and bad “feelings” with certain good and bad outcomes. These feelings (i.e., emotional somatic markers) or intuitions are then used prospectively to aid decision-making by providing individuals with hunches as to whether a choice is good or bad (these hunches can be reliably measured via galvanic skin responses). This paradigm has been tested in the context of a decision-making task that involves what is known as the Iowa or Bechara Gambling task (Bechara & Damasio, 2005). This task involves choosing cards from four decks, two of which are bad (i.e., they have high immediate rewards but higher future losses) and two of which are good (i.e., they have lower immediate rewards but future losses are less severe than those of the bad decks). Contrary to normal participants (without brain damage), brain-damaged participants do not show any pre-decision physiological (i.e., emotional) responses prior to choosing from a bad deck and consistently go for the bad decks.

The somatic marker hypothesis has received much empirical support (Bechara & Damasio, 2005). Yet, as of today there is *no research* linking performance on an EI test with performance on the gambling task. More importantly, research has moved on since Damasio's (1994) initial interpretation of the Elliot case. Current evidence suggests that: (1) working memory, which resides in the aforementioned brain region too and which correlates very strongly with fluid intelligence, r = .85 (Oberauer, Schulze, Wilhelm, & Süss, 2005), is also needed in effective decision making and works in conjunction with other neural circuitry requiring somatic markers; (2) damage to the ventromedial region *actually damages fluid intelligence too* and this result has been known since 1996; (3) IQ predicts performance on the gambling task; and (4) the "Elliot" brain area (i.e., the VM) is reliably associated with IQ as modern scanning techniques show (see Letter 3 in Antonakis, et al., 2009).

Even Bechara, Damasio, Tranel, and Anderson (1998) noted, in reinterpreting the functions of the VM, that "Our initial prediction that we would find a complete double dissociation between decision making and working memory relative to . . . prefrontal cortex, however, has to be revised. . . . "decision making seems to be influenced by the intactness or impairment of working memory; i.e., the subject's decision making is affected by having an abnormal working memory" (p. 434). Bechara and Martin (2004) added "that working memory and decision making are asymmetrically dependent. Working memory is not dependent on the intactness of decision making. . . . On the other hand, the integrity of decision making seems to be dependent on the intactness of working memory—that is, the participant's decision making is affected by having an abnormal working memory" (p. 160).

In summary, a modern interpretation of the Elliot case leads us to view it as supportive of the combined roles of IQ, working memory, and somatic markers for decision making (using the limbic system and other brain regions associated with IQ). Somatic markers and EI are not isomorphic, nor is there evidence linking somatic marker functioning to EI. If anything, 15 years of research subsequent to the Elliot case show that the implicit emotional signaling system is undergirded by working memory (and IQ) and not vice-versa; hence, Bechara and Martin's (2004) reference to asymmetrical dependence.

3. The Marshmallows Studies Don't Support EI either

As further support for ESC, Cherniss cites the "marshmallows" studies by Shoda, Mischel, and Peake (1990) who "found that the children who were able to resist temptation had a total SAT score that was 210 points higher on average than those children who were unable to wait." Conceptual and empirical problems, however, mar Cherniss's use of this study.

Conceptually, why is delay of gratification is an ESC? Cherniss's commentary does not address this question. Delay of gratification can be defined as the ability to wait for something that one desires. Thus, it is plausible to suggest that delay of gratification has both cognitive (calculation of future payoffs, risk preference) and emotional aspects. Delay of gratification might also reflect the ability to predict socially-desirable behavior expected by parents and teachers; it might also reflect aspects of personality (patience, impulsiveness).

Empirically, past and current research provides support for the cognitive aspects of delay of gratification. First, a recent meta-analysis by Shamosh and Gray (2008) reported a $\rho = .23$ between IQ and delay of gratification; this correlation would have been higher had it been corrected for range restriction and unreliability. Further, a neuroscience study by Shamosh et al. (2008) found an $r = .40$ between IQ and delay of gratification and this association was related to activity in known IQ brain regions. Third, in a *random* sample of about 1,000 adults and using an incentive-based design with real monetary stakes, Dohmen, Falk, Huffman, and Sunde (in press) found that IQ predicted delay of gratification after controlling for various personality traits, participant income, and other demographic and economic preference factors. In a similar study, Burks, Carpenter, Goette, and Rustichini (2009) showed that IQ predicted both short-term and long-term delay of gratification in a sample of about 1,000 trainee truckers, again including a rigorous set of control variables. Finally, with regard to the emotional aspect of delay of gratification, research indicates that personality correlates with it (Ostaszewski, 1996).

In addition to the above listed conceptual and empirical problems of labelling delay of gratification as an ESC, digging a bit deeper into the Shoda et al. (1990) marshmallow study reveals several problems. Delay of gratification correlated significantly with *only* four out of eleven measures of a coping and cognitive competence scale (California Child Q-Set) when the effect of SAT was partialled-out (which was used to show that coping is independent of intelligence). Four out of eleven is not cause for conclusive inference, particularly when the sample size reported by Shoda et al. (1990) was only 33. With such a small sample and multiple tests, the significance of the correlations could be due to chance. When controlling for *family-wise error* (e.g., Bonferroni correction), and setting the overall Type I error to be .05 in a sample of 33, the correlation coefficient must be .49 or greater to achieve significance. How many of the correlations listed by Shoda et al. (1990, see p. 982) were this high? Not one--and this without controlling for personality traits.

We have similar concerns with Cherniss's interpretation of the study by Duckworth and Seligman (2005) as showing that "self discipline predicted grades twice as well as IQ scores in a sample of 8th graders." Again, there is no conceptual justification: Why does Cherniss label self-discipline as an ESC? Furthermore, a closer look at the study by Duckworth and Seligman reveals that the interpretation by Cherniss is not justified, as explained in next paragraph.

The measure of self-discipline was an index of (a) self and other ratings (e.g., parents and teachers) and (b) a behavioral (objective) measure (a marshmallow-style test). Parents and teachers obviously knew the grades of the children and would thus probably rate children with higher grades as more disciplined (e.g., to maintain cognitive consistency). In addition, we were able to obtain the summary data from Duckworth and Seligman. Our reanalysis of these data showed that the average correlation between the objective measure of self-discipline and the five self/other ratings of self-discipline was only .14, raising concerns about the validity of the index of self-discipline. Using only the behavioral measure of self-discipline and the Otis-Lennon IQ test scores (constraining its validity to .62, see Guilmette, Kennedy, & Queally, 2001) we found that self discipline predicted grades ($\beta = .29$, $p < .001$) but so did IQ ($\beta = .36$, $p < .001$). Furthermore, Duckworth and Seligman reported that the observed correlation between IQ and grades suffered from range restriction (i.e., instead of $r = .32$ the correlation should be $r = .49$), suggesting that the effect of IQ is an underestimation.

The above-listed conceptual and empirical concerns should be taken into account in future EI research. At the conceptual level, it will be helpful to explicitly justify why delay of gratification and self-discipline are ESCs. Next, as part of the construct validation process, measures of EI must predict delay of gratification and self-discipline beyond established predictors (i.e., IQ and personality). Finally, even if delay of gratification and self-discipline were indeed ESCs, to establish their relevance for predicting performance it would be good practice to control for IQ and personality, which are also predictors of performance in work settings. Notably, in the Shoda et al. (1990) and Duckworth and Seligman (2005) studies, the estimates for the predictive power of delay of gratification and self-discipline for performance would have been even weaker had these studies included personality traits as control variables.

4. Conclusion: The Future of Emotional Intelligence

We think that the arguments and evidence that Cherniss presents are not strong. Many of the key papers he cites cannot be clearly interpreted to support his reasoning. In conjunction with no evidence from meta-analyses for incremental validity of EI/ESC above and beyond IQ and personality traits, the future for EI does not bode well, or does it?

As we mentioned before, the only way we see EI moving forward is to commit firmly to the ability definition and its consequences--then there is no need and possibly no room for ESCs. Other consequences further include the design of better ability tests and not broader trait-like ESC tests. One interesting avenue to for ability tests it to is examine EI scores in terms of a congruence model; at this time, answers are scored according to their difference from what the majority of respondents (“consensus” scoring) or experts answered. To ensure that findings based on these scores are not ambiguous, EI researchers should consider the widely-used statistical guidelines provided by Edwards (1995). Perhaps the use of difference scores has been one reason for poor results and confounded interpretations.

Another interesting avenue would be to design tests that have objectively-correct answers, by, for example, having the participants’ emotions--who are portrayed in EI test materials--measured via physiological or other biological means. Interestingly, work currently being done at the interface of psychophysics and cognitive psychology can enable computer algorithms to accurately rate human facial expressions (Sorci et al., in press); future work in this area could be extended to dynamic testing situations.

Yet another alternative is to start from basic research in neurosciences to first determine whether physiological markers or certain brain regions not linked to IQ can predict performance on validated social-psychological tests like nonverbal decoding ability or interpersonal sensitivity (Hall, 1978; see Hall, Andrzejewski, & Yopchick, 2009). It is unfortunate that much of the research done in EI has wholly ignored the rich history of research in related areas of social psychology. Building on research programs in these areas might prove to be fruitful. In summary, there is much that could be done to take EI to the next level. The foundations of this transformation must be built on the firm theoretical and empirical grounding of ability tests.

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