# ON WHY "EMOTIONAL INTELLIGENCE" WILL NOT PREDICT LEADERSHIP EFFECTIVENESS BEYOND IQ OR THE "BIG FIVE": AN EXTENSION AND REJOINDER

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*Emotional intelligence (EI) has been embraced by many practitioners and academicians without clear empirical support for the construct. In this rejoinder and extension of an earlier comment, I highlight the importance of using methodologically defensible scientific criteria for conducting or evaluating research. I review literature demonstrating that EI models are beset with problems concerning their validity and show that support for the EI construct may be based more on tangential speculation than on empirical findings. Although I find some common positions with EI researchers such as Prati et al., I underline contradictions and inconsistencies which may cast doubt on the necessity of EI for understanding and predicting leadership effectiveness.*

My earlier critique of Prati et al's (2003a) article, proposing that "emotional intelligence" (EI) is an indispensable condition for effective leadership was motivated by several reasons. The most important reason is that too many individuals, including academicians and practitioners, have been captivated, even hoodwinked, by the apparent "magic" of EI and are oblivious to the fact that many of the claims made by EI proponents regarding the apparent necessity of EI for leadership or organizational performance are unsubstantiated, exaggerated, misrepresented, or simply false (Antonakis, 2003; Matthews, Zeidner, & Roberts, 2002; Zaccaro & Horn, 2003; Zeidner, Matthews, & Roberts, 2004).

This article extends my discussion of Prati et al's (2003a) article and provides my response to Prati et al's (2003b) reply. I will highlight some common positions we hold and differences we have. In addition, I extend this discussion by providing some guidelines—traditionally used in psychometric testing—that are useful for guiding or evaluating research concerning the utility of psychological constructs in organizational settings. There are some proponents of EI, in the academic and consulting arena, who may be ignorant of established scientific guidelines for conducting or evalu-

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*Organizational Analysis*, Vol 12, No. 2, 2004, pp. 171–182 ISSN 1551-7470

ating empirical research. Unfortunately, without using these guidelines, conclusions made by EI researchers may mislead readers who are not trained to critically evaluate the claims that are made. Thus, my motivation in laying these guidelines is to ensure that future research—which should inform practice and hopefully improve the effectiveness of organizations—is based on science and data that are methodologically defensible. Finally, I will demonstrate that many of the fundamental positions regarding the apparent necessity of EI for organizational leadership are flawed.

## GENERAL REFLECTIONS

In general, I welcomed Prati et al's (2003b) response to my critique, which has helped to clarify their positions and resolve some misunderstandings. Contrary to their original work (Prati et al., 2003a), Prati et al. (2003b) have now decided to jettison popular notions of EI *à la* Goleman and colleagues (Goleman, 1995, 1998a, 1998b; Goleman, Boyatzis, & McKee, 2002) from their theoretical boat. However, while they still offer justification for using the quite "broad" Goleman model, which includes almost every individual-difference variable that is not IQ (for an interesting critique of this model see Sternberg, 1999) they now recognize the value of the focused definition of EI as proposed by Peter Salovey and associates (cf. Mathews, et al., 2002).

In all likelihood, the resulting decision in this climate would not only be unsatisfying to all involved but would also fail to address the organizational issues contained in the conflict. Other researchers need to begin questioning some of the popular claims surrounding EI. Unfortunately the business world has been inordinately influenced by these popular works (see Zaccaro & Horn, 2003). The influence of the popular claims has probably been for the worse. For example, I invite readers to undertake an Internet search using the terms "emotional intelligence" and "leadership" to see what damage is potentially being caused. Consulting companies galore offer panoplies of EI tests that can ostensibly differentiate 60% to 90% of exemplary leaders from average performers (or alternatively, that 60%-90% of effectiveness in leadership is attributed to EI). These claims, which are unclear from a statistical perspective (i.e., does EI actually predict up to 90% of the variance in leadership emergence or effectiveness?) are highly misleading and not backed up by any scientific data published in credible sources. Yet, many of these consulting companies use Goleman's work to support their claims, also noting that EI is apparently twice as important as IQ or technical skills for leadership effectiveness (see Goleman, 1998a)—a nebulous claim that has never been peer-reviewed and published in a credible, empirical, scientific journal.

The situation concerning popular notions of EI and their influence on the business world is troublesome from a scientific, economic, and ethical point of view. It is unconscionable that organizations might be basing their hiring, promotion, or retention decisions wholly or in part on EI models—models that simply do not have enough scientific backing to be used in industrial settings. Thus, it is imperative that future research be conducted using rigorous tests to determine whether EI *really* matters. Current scientific evidence suggests that EI does not yet offer anything new beyond that which we know about "g" (i.e., general intelligence or IQ) or personality (the "big five" personality factors). Also, EI does not predict work performance very well—and certainly much less than does "g" or other personality factors as I discuss below—contrary to the hyperbolic claims of EI proponents. As soberly stated by Zeidner, Matthews, and Roberts (2004, p. 393):

> Despite the important role attributed to a wide array of emotional competencies in the workplace, there is currently only a modicum of research supporting the meaningful role attributed to EI (and nested emotional com-

petencies) in determining occupational success. Many of the popular claims, presented in the literature regarding the role of EI in determining work success and well being, are rather misleading in that they seem to present scientific studies supporting their claims, while in fact failing to do so. In short, despite some rather fantastic claims to the contrary, the guiding principle appears presently as 'caveat emptor' [i.e., let the buyer beware].

## BACK TO BASICS: WHAT MAKES A MEASURE VALID?

This section introduces some principles derived from basic guidelines used in psychometrics, which may be of use in evaluating EI research and EI claims. As I have mentioned elsewhere (Antonakis et al., 2004), there is no authority akin to the U.S. Food and Drug Administration that ensures that products (i.e., books, training programs, selection tools, etc.) that apparently measure, predict, or develop leadership do what they claim to do. The onus is upon the consumer to determine whether a particular tool does what its developers claims it can.

Before one can propose that a construct (e.g., EI) offers something different or is somehow better than constructs that have an established history (e.g., "g" and the "big five") it is important that the EI proponents demonstrate that EI is reliable and valid (for detailed accounts regarding validity and reliability see Carmines & Zeller, 1979; Kerlinger, 1986; Nunnally & Bernstein, 1994). In general, reliability refers to the extent to which a test's indicators are internally cohesive and measures a construct consistently (Carmines & Zeller, 1979). A measure can be reliable, but could be reliably measuring the wrong thing—the measure could be consistently off target. Thus, a reliable measure does not imply that the measure is valid. Validity—which is "the extent to which any measuring instrument measures what it is intended to measure" (Carmines & Zeller, 1979, p. 17)—when demonstrated, suggests that the measure does what it should do (i.e., is on target, consistently). Validity therefore assures that a measure is reliable. Validity is typically assessed as follows:

1. *Construct validity.* Do indicators of a construct represent the construct consistent with theoretical formulations (i.e., does the construct have valid indicators)? Construct validity is typically tested using confirmatory factor analysis to assess whether the indicators of the constructs (i.e., unobserved or latent variables) constituting the theory relate to their constructs as specified a priori.
2. *Criterion validity.* Do measures of EI predict or explain variance in outcome or dependent measures (e.g., leadership effectiveness, work performance, etc.)? When the independent (i.e., EI) and outcomes measures are gathered at the same time one assesses what is termed *concurrent validity.* When the outcomes measures are gathered in the future then one assesses what is termed *predictive validity.* Establishing criterion validity using regression-type statistical techniques is vital for demonstrating the utility of a psychological measure.
3. *Discriminant validity.* Are measures of EI weakly correlated or unrelated to competing constructs (e.g., "g" or the "big five")? Again, if EI measures correlate strongly with measures of "g" or personality then what are they uniquely assessing? Are they simply old wine in new bottles?
4. *Convergent validity.* Are different EI tests strongly correlated with each other? Established measures of "g" tend to correlate strongly among each other. If they did not, then all the measures would be in doubt, as is currently the case with EI.

5. *Incremental validity.* Do measures of EI explain unique variance in dependent outcomes beyond the variance that this accounted for by competing constructs? This test is undertaken using regression-based statistical procedures in which competing variables (e.g., "g" and the "big five") are entered first into a regression model as control variables (i.e., model I). In the next step (i.e., model II), EI is entered as a predictor to determine if EI predicts significant and unique variance in the dependent outcome beyond that predicted by the control variables (note: the difference in variance predicted by the two models can be tested for statistical significance using an F-test). Incremental validity is the most difficult and the most important test to pass and can be considered a litmus test of validity. If EI measures cannot demonstrate incremental validity then they add nothing to the predictive power of "g[1]" or the "big five." EI is thus redundant or inutile, as current evidence suggests.

In addition to the above, and as highlighted in my previous critique (Antonakis, 2003), researchers should avoid gathering leader self measures (which suffer from social desirability bias and so forth, see Conway & Huffcutt, 1997; Harris & Schaubroeck, 1988; Podsakoff & Organ, 1986). Furthermore, followers should not complete both leader and outcome measures, because the resulting correlations will be inflated (as raters will strive to maintain cognitive consistency and so forth, see Avolio, Yammarino, & Bass, 1991; Podsakoff, MacKenzie, Lee, & Podsakoff, 2003). Finally, the EI measures used must be measures designed to tap into EI and not into apparently related constructs (e.g., empathy, self-monitoring, etc.).

Ideally, the target leaders should complete measures of "g", EI, and personality, and followers/peers/bosses the leadership measures on the respective target leader (or the leader measures should be gathered in some other manner, e.g., ratings by expert observers, ratings based on historical or archival data, etc.). The resulting correlations between the independent (i.e., individual difference measures) and the dependent (leadership style) variables will thus not be artificially inflated but accurate, assuming that ratings of leader style can be justifiably aggregated if multiple raters have been used (see Antonakis et al., 2004). Furthermore, practicing leaders in real contexts should be used instead of students because the dynamics of student-student relations might differ from leader-follower relations. Finally, samples should be of a respectable size.

I have yet to find *one* study that has followed the above generally accepted psychometric testing guidelines and has showed that EI matters for leadership effectiveness. Any research suggesting that EI matters but which has not followed these guidelines or which has exclusively used a qualitative mode of inquiry must be interpreted with extreme caution (see Antonakis et al., 2004 for weaknesses regarding the qualitative approach). It is known that EI measures are plagued with problems regarding their validity and reliability (see Davies, Stankov, Roberts, 1998; Matthews et al., 2002; MacCann, Matthews, Zeidner, Roberts, 2003; Roberts, Zeidner, & Matthews, 2001; Zeidner et al., 2004; Zeidner,.Matthew, & Roberts, 2001). It is obvious, therefore, that EI tests cannot yet be trusted to measure what they are supposed to measure. These issues need to be resolved before EI tests are used in industrial settings and before EI proponents prematurely trumpet EI's worth in the workplace.

## BACK TO "G" AND THE "BIG FIVE"

By demonstrating that EI measures are valid, EI proponents can rightly claim that EI matters in occupational settings. Unfortunately for them, this is not the case. Prati et al (2003b) attributed the

lack of positive findings to EI being "a relatively new field of investigation [and that] there are unfortunately a small number of studies related to leadership effectiveness" (p. 364). Whereas this may be the case, then why are they not gathering data to demonstrate that EI matters for leadership? It has now been almost 15 years since Salovey and Mayer (1989-1990) wrote their germinal piece on EI and almost 90 years since Thorndike (1920) discussed social intelligence, which EI proponents claim to be the older "cousin" of EI. This lack of empirical evidence, however, has not stopped the creation of theories regarding the importance of EI for leadership.

The state of empirical evidence for the relationship between EI and leadership remains weak after 15 years. In comparison, Bass's (1985) theory of transformational leadership demonstrated very positive findings by the year 2000 (i.e., 15 years later), in an equally contentious scientific field of study. Therefore, other constructs have shown empirical support within the same 15 year time period. Thus, being the "new kid on the block" does not absolve one from being a "good kid on the block."

Prati et al. (2003b), questioned the studies that I cited which showed that "g", the "big five," or that implicit motives matter, by stating, "curiously, 'leader effectiveness' was not specifically addressed" (p. 364). Readers may refer to the studies by Judge, Bono, Ilies, & Gerhardt (2002) and Spangler and House (1991), which did use effectiveness measures as outcomes (when we talk "outcomes" in leadership we typically refer to effectiveness, performance, satisfaction, motivation, etc.). Prati et al. might have overlooked these findings. I acknowledge that the study by Lord, De Vader, & Alliger (1986) used measures of emergence and not effectiveness, which I made clear when citing that study (see Antonakis, 2003, p. 356). For a review of studies showing "g" to be related to leadership effectiveness readers may refer to Zaccaro, Kemp, and Bader (2004).[2] Furthermore, Zaccaro et al., who are sympathetic to alternative perspectives of intelligence (including emotional intelligence), noted, "additional research is necessary to identify the unique contributions of emotional intelligence beyond other conceptually similar constructs [e.g., social intelligence]" (p. 117).

On another note, Prati et al. (2003b) claimed that the dissertations by Buford (2002), Collins (2001) and Schulte (2002), which did not bode well for the EI construct, used "self-report measures based on Goleman's mixed model" (p. 364). These dissertations, however, were in general quite well designed (according to the criteria based on psychometric guidelines I listed previously) and in fact, two of the three studies used the ability-based measure that Prati et al. (2003b) claimed has the most potential.

For example, Schulte (2002) who used the Mayer, Salovey, Caruso Emotional Intelligence Test (MSCEIT), found that the MSCEIT did not predict incremental variation in transformational leadership when controlling for a measure of "g" (Wonderlic Personnel Test) and the "big five" (NEO-PI). Buford (2002), who used the EQ360 (a self-report measure) developed by Bar-On, and controlled for the "big five" using the NEO-PI, concluded that EI "likely reflects well-researched, already defined personality traits" (p. 77). Collins (2001), who used practicing executives as subjects, measured EI using the MSCEIT as well as the OPQ32-EI (a self-report measure), and controlling for a measure of "g" (Watson-Glaser Cognitive Thinking Assessment) and the "big five" (NEO-PI) failed to produce the stellar results suggested by Prati et al.

More worrisome for the positions taken by many EI researchers, are recent studies that highlight weaknesses in the ability-based EI model. Schulte, Ree, and Carretta (in press) found that the MSCEIT, the ability-based measure proposed by Prati et al. (2003) was *strongly predicted* (i.e., multiple correlation of .81) by "g", agreeableness (a dimension of personality measured by the NEO-PI),[3] and gender (see also Day & Carroll, 2004, and O'Connor & Little, 2003, which do not

demonstrate positive findings for the MSCEIT). Schulte et al. concluded, "if EI can be largely predicted . . . from other well-known constructs, its uniqueness and expected incremental utility for predicting human performance may be limited."

In a recent meta-analysis, the MEIS (Multifactor Emotional Intelligence Scale)—an ability based EI measure that is the precursor to the MSCEIT—was found to be weakly predictive of various performance measures, including work performance (Van Rooy & Viswesvaran, 2004). The meta-analytic ρ (i.e., estimated population correlation) was .19, which suggests that EI and measures of performance share only 3.61% variance (interestingly, personality- or trait-based measures that Prati et al. now jettisoned faired a bit better but still quite dismally). This result compares poorly to the criterion validity of "g", which has been demonstrated to be .51 and .62 in the United States and the European Union respectively (see Salgado et al., 2003a, 2003b; Schmidt & Hunter, 1998). Furthermore, this correlation between "g" and work performance generally improves with increasing job complexity and hierarchical job level (see also Gottfredson, 1997).

Even more worrisome in the above meta-analysis was that EI measures failed to add incremental validity to "g" (EI added 2% variance to measures of "g" whereas "g" added 31% to measures of EI). Thus, when I originally stated that "g" is EI's nemesis I believe I was not exaggerating. If EI proponents want to show that EI matters, they need to demonstrate that EI goes beyond "g" and the "big five" and that EI predicts non-pitiful amounts in outcome measures. If EI cannot even predict simple work performance what hope is there for it to predict a more complex outcome like leadership effectiveness (which "g" and the "big five" already predict)? As van Rooy and Viswesvaran (2004) conclude: "the claims that EI can be a more important predictor [for performance] than cognitive ability . . . are apparently more rhetoric than fact."

In reference to "g," Prati et al. (2003b) stated, "The opinion of many scientists, who see the potential of the emotional intelligence construct, is that general intelligence has a tremendous impact in most contexts, but, like emotional intelligence, its importance is relevant to situational context" (p. 364). Prati et al., therefore, acknowledged that "g" matters. In any case, apart from Salovey and colleagues and a small contingent of serious scholars such as Prati et al., there appears to be few EI researchers who both propose that EI is important and who acknowledge that "g" matters. In any case, meta-analytic studies (with sample sizes in the thousands) that demonstrate that "g" does not matter in certain contexts as Prati et al. suggest, would be necessary to counter the meta-analytic results of Salgado et al. (2003a, 2003b) and Schmidt and Hunter (1998).

## ARE HIGH LEVELS OF EMOTIONAL APPRAISING ABILITY NECESSARY FOR LEADERSHIP EFFECTIVENESS?

In my first critique of Prati et al. (2003a), I expressly questioned whether "high" levels of emotional appraising ability are needed. I did not profess that leaders should totally ignore the emotional states of others and I argued that normal individuals are perfectly capable of demonstrating the emotional appraising/social skills that are necessary for effective leadership. I am against the notion that one needs inordinate levels of emotional appraising ability, which is contrary the position of Prati et al. that higher leader EI is associated with higher leader charisma/performance.

Adding some support to my speculation that EI does not matter for leadership effectiveness, Feyerherm and Rice (2002) found that elements of leaders' EI—as measured by the MEIS—are *negatively* related to the leaders' team performance. They concluded by stating: "Writers, consult-

ants, trainers and managers themselves should be cautious about making broad claims about the benefits of the leader's EI for team performance" (p. 359) (note: this study is limited because no justification was provided to aggregate individual data to the team level of analysis). I still maintain that a great charismatic/transformational leader generates affective links with his/her followers because of vision, moral conviction, a high need power (with a high responsibility disposition), courage, and confidence (see Antonakis & House, 2002; Shamir, House, & Arthur, 1993). There is no theoretical reason why the leader has to be high on EI to foster the charismatic effect, as I highlighted in my previous critique (Antonakis, 2003).

I maintain that leaders should not be too sensitive to and unduly influenced by the emotional states of others,[4] especially at top leader levels. I likened emotional or social gauging ability in terms of learning and applying scripts—a form of schematic knowledge that is tacitly held (Antonakis, 2003). A schema is "a cognitive structure that represents knowledge about a concept or type of stimulus, including its attributes and the relations among those attributes" (Fiske & Taylor, 1991, p. 98). Scripts are "expectations about the order as well as the occurrence of events," learned by repeated exposure to a series of interlinked events (Abelson, 1981, p. 717). Theoretically, the ability to learn scripts is a general ability and follows from definitions of "g" (generally defined as the ability to learn, to process information, and to abstract, see Gottfredson, 1997, 2002; Schmidt, 2002; Schmidt & Hunter, 1998).

Learning, retrieving, and acting on scripts can be applied to a variety of condition-action procedures or causal sequences, including the meanings associated with emotions (Abelson, 1981, p. 727; see also Zeidner et al., 2001). There is no logical reason why this general cognitive process would require one to be "high EI" or empathetic (often associated with EI). That is, one does not need to be "high EI" to learn scripts associated with reading the emotional states of others. Using my reasoning implies that individuals can have the ability to read and act on the emotional states of others but that they are not necessarily "bogged down" (i.e., inordinately influenced) by the emotional states of others by being high "EI."

The same cognitive processes are probably involved in forming scripts associated with emotional or non-emotional processes, whether these scripts have been learned vicariously or experientially. I have not seen any convincing arguments or data to support the contrary. After repeated exposure to patterns of events—whether emotional or non-emotional—individuals abstract common elements and represent these symbolically in a schematic form that denotes descriptive and prescriptive information about that event (cf. Fiske & Taylor, 1991). As mentioned by Becker (2003), "why can't emotional intelligence simply be seen as general intelligence directed at emotional phenomena?" (p. 193).

## GENERAL REMARKS

I was most pleased to see that Prati et al. (2003b) conceded that at times their "wording was poorly phrased" (p. 367) with respect to the fact and that they misattributed certain positions to scholars (e.g., suggesting that Wasielewski, 1985, stated that EI mattered for charisma). Prati et al. (2003a) also stated that other scholars, for example, Williams and Sternberg (1988), provided empirical evidence to show that EI matters for effective leadership or team functioning. They stated for instance that "An individual's lack of emotional intelligence might be detrimental to effective team interaction as Williams and Sternberg (1988) found" (Prati et al., 2003a, p. 36). However, Williams and Sternberg never used any recognized measure of EI. Prati et al. (2003b) justified their approach by

stating that they were attempting to compile theoretical evidence in support of their propositions. I would have no problem with such an approach. Prati et al. (and other researchers) should always be clear in defining what types of measures were used in the works reviewed and when indirect inferences are being drawn. Anything short of this approach is misleading readers and misrepresenting the evidence.

On another note, Prati et al. (2003b) agree with my reasoning that emotional gauging skills might simply be a function of national culture. My argumentation in this regard explicitly suggested that the skills considered effective in one culture might not be effective in another culture because individuals vary in the ways they display and act on emotions as a function of national culture. This position is inconsistent with the position that EI is an individual-difference variable because membership in a group and not the individuals' traits determines the individuals' skills (apart from the ability to learn scripts, as I discussed above). Prati et al., have overlooked this apparent contradiction in position by implying that that EI should be studied as an individual-difference variable.

Prati et al. (2003b), like many EI researchers, have not considered the implications of conducting multi-level research (see Dansereau, Alutto, & Yammarino, 1984; Klein, Dansereau, & Hall, 1994). Prati et al. (2003a) suggested that the effects of the leader's EI would be manifested at the team level of analysis (and I explicitly stated "at the team level of analysis," p. 358). For an effect to be evident at a higher level of analysis than that at which it is measured (i.e., from the individual to the group), the researcher has to demonstrate that the variation in follower perceptions is homogenous (using the appropriate statistical procedure, e.g., WABA, see Antonakis et al., 2004). In other words, because of their group membership, followers will see their leader in more or less the same way. This position would be inconsistent with the effects that a high EI leader would apparently engender. Treating each follower in a customized, heterogeneous and individually tailored, empathetic way would suggest a certain degree of intimacy with followers (or reduced social distance, see Antonakis & Atwater, 2002). Consequently, follower individual differences might account for the ways in which the followers view the leader (i.e., each follower would see a comparatively "different" leader), suggesting that the effects of the leaders would be evident at the individual level of analysis.

Finally, Prati et al. (2003a) strongly professed the position that leaders should demonstrate and spread positive emotions. They expressly noted "emotions such as anger or sadness . . . expressed by the team leader" would prove to be detrimental to team effectiveness (p. 26). They noted further "anger displayed by the team leader might be perceived by the team members as a weakness or lack of control in the leader [and that] Goleman (1998) and Lewis (2000) found that a leader's lack of emotional control was related to leader ineffectiveness" (p. 27). I challenged this position stating that charisma is based on emotional appeals that might include anger, disgust, and so forth (Antonakis, 2003; see also Tiedens, 2001, who found that displays of anger were associated with leadership status). Prati et al. (2003b) asserted "lack of emotional control in displaying distressed emotions [is] the reason for negative effects on team member perceptions of the leader" (Prati et al., 2003b, p. 367). This statement contradicts what they said previously. It appears Prati et al. are essentially saying that EI can be about seeming to lose emotional control (i.e., acting *really* angry) as long as one is really in control and knows what one is doing (it would be interesting to see how this ability could be measured). More importantly, this position would suggest to me that such leaders are not being authentic because they are not as angry as they make out to be.

## CONCLUSION

> A common phenomenon and problem in leadership practice concerns undue reliance on popular ideas and fads without sufficient consideration given to the validity of these ideas. Recent examples include . . . emotional intelligence [EI]. . . .[As regards EI] two core beliefs that are prevalent in today's managers, but still without sufficient empirical validation—that [EI] influences organizational and individual effectiveness and that [EI] can be changed and, implicitly, that such change can happen over a relatively short-term. (Zaccaro & Horn, 2003, pp. 779-781)

Zaccaro and Horn (2003) are rightly skeptical. The onus of responsibility to show the baselessness of this skepticism rests on the shoulders of EI's proponents. EI's proponents need to move science forward, objectively. It is normal to have faith in intuitively appealing arguments. However, scientists have a responsibility to inform the public of what works and what does not work, independent of the scientist's beliefs and intuitions. Scientists need to pay attention to the evidence and from time-to-time they must "bite the bullet" when the tide has turned against them.

Endless theoretical debating and propositions are not going to help science and practice. We have had enough propositions and armchair speculation regarding the utility of EI. Now we want to see data. EI's proponents should pit their boat against strong competitors (i.e., "g," "big five," etc). The EI boat has hardly left its theoretical moorings. When it does it will suffer a calamity of titanic proportions. Then, perhaps, a new and improved EI boat will be designed that will better serve the interests of science and business.

**Acknowledgment:** I would like to thank Saskia Faulk and Marika Angerfelt for their comments on an earlier draft of this manuscript.

## NOTES

1. The IQ measure used should include all aspects of IQ and not specific aspects only (e.g., verbal intelligence).
2. A recent meta-analysis established that the meta-analytic $\rho$ (i.e., estimated population correlation) between "g" and objective measures of leader performance was .33 (Judge, Colbert, & Ilies, 2004), which can be qualified as a medium effect size (Cohen, 1988).
3. As mentioned previously, agreeableness is not predictive of leadership (Antonakis, 2003).
4. I have difficultly in imagining high EI individuals (or in conceptually similar terms, empathetic, affiliative, agreeable individuals) with finely adjusted emotional appraising skills who can simply ignore the emotional states of followers when needed, act contrary to the desires of their followers, and take tough positions on contentious issues (Antonakis, 2003; Antonakis & House, 2002). I can imagine though that Prati et al. would probably claim that gauging and selectively acting on the emotional states of others *is* emotional intelligence. However, to what extent is this "ability" measured in the MSCEIT or other such measures? Also, ability questionnaires measure intentions or knowledge of appropriateness of responses and not what respondents would actually do in practice. Furthermore, what constitutes a correct response on the MSCEIT? Responses are generally graded using a consensual or expert approach and there are no clear-cut and objectively-correct responses (see Matthews et al., 2002; Zeidner et al., 2001).

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