Rumors of the Death of Emotional Intelligence in

Organizational Behavior are Vastly Exaggerated

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Abstract

In the first of two articles presenting the case for emotional intelligence in a point-counterpoint exchange, we present a brief summary of research in the field, and rebut arguments against the construct presented in this issue. We identify three streams of research: (1) a four-branch abilities test based on the model of emotional intelligence defined in Mayer and Salovey (1997); (2) self-report instruments based on the Mayer-Salovey model; and (3) commercially available tests that go beyond the Mayer-Salovey definition. In response to the criticisms of the construct, we argue that the protagonists have not distinguished adequately between the streams, and have inappropriately characterized emotional intelligence as a variant of social intelligence. More significantly, two of the critical authors assert incorrectly that emotional intelligence research is driven by a utopian political agenda, rather than scientific interest. We argue, on the contrary, that emotional intelligence research is grounded in recent scientific advances in the study of emotion; specifically regarding the role emotion plays in organizational behavior. We conclude that emotional intelligence is attracting deserved continuing research interest as an individual difference variable in organizational behavior related to the way members perceive, understand, and manage their emotions.

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The case for emotional intelligence is presented in two articles. In this, the first of these, we deal specifically with the points raised by Landy, Locke, and Conte in the preceding three articles critical of the conceptualization and measurement of emotional intelligence. Indeed, if one were to read the three critical articles, especially Landy's and Locke's, one could be excused for concluding that emotional intelligence is not viable as a scientific construct, and that organizational researchers ought to stop wasting their time in researching the construct. In other words, emotional intelligence is dead. We argue in this article that, far from being moribund, emotional intelligence is an exciting and developing area of research in organizational behavior, and a key component of the current burgeoning interest in emotions in organizational settings (Ashkanasy & Daus, 2002, Ashkanasy, Härtel, & Daus, 2002). At the same time, we also warn that emotional intelligence researchers need to be careful that they fully understand the construct, and also show appropriate levels of circumspection in their research endeavors. In the second of the two articles in defense of emotional intelligence (Daus & Ashkanasy, this issue), we take our arguments a step further, and provide an up-to-date and cogent summary of current research in work settings based on the four-branch model of emotional intelligence (as defined in Salovey & Mayer, 1997). Our hope is that this article will serve to guide future research in a blossoming new field in organizational behavior and industrial/organizational psychology.

The present article is arranged into three sections. In the first, we provide a brief history of the emotional intelligence construct, and introduce the three main streams of

research in the field. We then summarize the points raised by the protagonists, and discuss the authors' criticisms within the context of the more recent literature on the psychology of emotions, emotional intelligence, and emotions in organizations. In the third and final section, we discuss in general the role of emotional intelligence research in organizational behavior research, and in particular within the context of the study of emotion in organizations.

Models of emotional intelligence

    Although the term 'emotional intelligence' was used occasionally in the general literature beginning in the 1960's (Payne, 1986), the first definitive application of the term appeared in a doctoral dissertation by Wayne Payne (1986). Payne, however, did not publish his theory, so the article published in 1990 by Salovey and Mayer (1990) is generally regarded as the wellspring of thought on this topic. (A second 1990 article, by Mayer, DiPaolo, and Salovey, also introduced the construct but in a more restricted sense, and is less well known.) In particular, Salovey and Mayer (1990) provided the initial definition of the construct in terms of an individual's ability to perceive emotion in self and others, to understand emotion, and then to manage emotion in self and others. Shortly thereafter, *New York Times* social science journalist Daniel Goleman, who was researching for a book on 'emotional literacy' in education, came across Mayer and Salovey's work, and decided to rename his book *Emotional intelligence: Why it can matter more than IQ*, which was published in 1995. It is now a matter of history that this became a bestseller, including a *Time Magazine* cover feature (Gibbs, 1995), and brought emotional intelligence to the forefront of public attention. Goleman's book, in turn, came to the attention of a young doctoral graduate, Reuven Bar-On, who had recently

completed his dissertation on psychological well-being. Bar-On had not heard of

emotional intelligence until he read Goleman's book, but quickly recognized the potential

of the measure he had developed for his dissertation work, and rebadged his scales as the

EQ-i, a multidimensional questionnaire measure of emotional intelligence, now marketed

and distributed by Multi-Health Systems (Bar-On, 1997). This was followed by a

veritable flurry of assorted measures, including Goleman's Emotional Competency Index

(ECI; Sala, 2002), a proprietary instrument, developed within the Hay Group, and

scholarly measures based on Mayer and Salovey (1997) by Jordan, Ashkanasy, Härtel, &

Hooper (2002), Schutte et al. (1998), and Wong and Law (2002). In the meantime,

Mayer and Salovey (1997) continued to develop their model of emotional intelligence,

which they recast in terms of four 'branches': (1) perception of emotion (in self and

others); (2) assimilation of emotion to facilitate thought; (3) understanding of emotion;

and (4) managing and regulating emotion in self and others. This model was

operationalized as an "abilities measure" of emotional intelligence in the tradition of

measures of intellectual intelligence (i.e., answers can be right or wrong) called the

MSCEIT (Mayer-Salovey-Caruso Emotional Intelligence Test; Mayer, Salovey, &

Caruso, 2002; see our second article, Daus & Ashkanasy, this issue, for a more

comprehensive overview of the psychometric development of the ability measure of EI).

In essence, three streams comprise the set of emotional intelligence research, and

associated measures. Stream 1 is based on the four-branch abilities model, proposed by

Mayer and Salovey (1997), and measured using the MSCEIT. Stream 2 encompasses

various self- and peer-report measures based on the Mayer-Salovey representation (e.g.,

Jordan, et al., 2002; Schutte et al., 1998; Wong & Law, 2002). Stream 3 comprises

expanded models of emotional intelligence that encompass components not included in Salovey and Mayer's definition, and are represented by the *EQ-i* and the ECI (see Conte, this issue, for more details and criticism of Stream 1 and 3 measures).

Mayer, Salovey, and Caruso (2000a,b), in reviewing the various measures of emotional intelligence characterized the Stream 3 models as "mixed", insofar as they comprise a mixture of personality-type items and behavioral preferences. In this instance, McRae (2000) illustrated how the Stream 3 models of emotional intelligence overlap with more traditional measures of personality. The Bar-On and ECI measures, in particular, include aspects of personality and social competence that go well beyond the bounds of the original definitions given by Salovey and Mayer (1990) and Mayer and Salovey (1997). Moreover, questionnaire measures did not in general fare well in early empirical evaluations that focused on their reliability and psychometric properties (e.g., Davies, Stankov, & Roberts, 1998), although more recent studies have suggested that the scales are much improved (e.g., Conte in this issue).

More particularly, the various emotional intelligence measures, especially Stream 3 measures like the Bar-On scale and the ECI, have proved extremely popular in management consultation applications. Like measures themselves, claims for the management consulting applications of emotional intelligence have often extended far beyond what Salovey and Mayer (1990) envisaged. Goleman (2000; see also Goleman, McKee, & Boyatzis, 2002), for example, subsequently developed his initial broad conceptualization of emotional intelligence into a comprehensive model of organization management and leadership (see Locke, this issue, for discussion and criticism of the Goleman leadership model). The net result is that public and commercial perceptions of

the emotional intelligence construct are often at variance with the definitions of the construct given by its originators, Mayer and Salovey.

The corollary of the foregoing discussion is that we actually agree with many of the points raised by Landy and Locke in their criticisms of emotional intelligence. Indeed, we made it "crystal clear" in our summary (Daus & Ashkanasy, 2003) of the SIOP "Great Debate" on this topic (DeNisi, 2003) that "we do not endorse a Goleman (1995) or Bar-On (1997) type of approach to studying emotional intelligence in the workplace" (p. 69). At the same time, we do not wish to disparage practitioners who wish to use these and other measures either as a consulting tool, nor do we wish to discourage researchers who wish to study the measures in applied settings. Nonetheless, as we stated in our 2003 paper, "These models may indeed be useful for organizational development and interventions, but they are much too broad in scope, and do not appear to markedly differ from traditional personality models or competency models" (Daus & Ashkanasy, p. 69). Our point here is to say to practitioners and researchers who wish to use and to further develop these measures and concepts, "Go ahead, by all means, but please do not confuse them with emotional intelligence." In this respect, we acknowledge that Goleman prefers to use the term "emotional competency" in his consulting applications (i.e., as in the ECI).

Against this background, we now turn our attention to the case against emotional intelligence, as proffered by Landy, Locke, and Conte. In particular, and consistent with Daus and Ashkanasy (2003), we repeat that we are in agreement with many of their points of criticism. Our main issue, however, is that by not sufficiently differentiating

among the different models, they run the risk of 'throwing the baby out with the bath water.'

Refuting the case against emotion intelligence

The authors of the critical articles in this issue approach the topic from three quite different points of view. Landy argues that most of the research in support of the construct of emotional intelligence lies "outside the scientific tent", and that the construct thus represents a continuation of a long line of discredited research into "social intelligences". Locke attacks emotional intelligence at its theoretical core, arguing that the construct is inadequately defined and even contradictory, and posits further that its application, especially in respect to leadership, is hopelessly flawed. Finally, Conte discusses issues in measurement of emotional intelligence, concluding that future users of emotional intelligence need to proceed with caution until more data are forthcoming to establish the bona fides of the various measures that have been proffered to date. In the following sections, we discuss each of the three critical articles in turn.

Landy: historical and scientific issues

Landy's criticism addresses three broad areas: (1) There is a lack of scientific scrutiny of measures of emotional intelligence; (2) the construct is rooted in the (discredited) concept of "social intelligence;" and (3) research in emotional intelligence is characterized by weak designs that have yet to demonstrate incremental validity over traditional models of personality and social/organizational behavior, and it is therefore premature to apply the results.

On the first point, Landy argues that emotional intelligence has been for the most part developed by purveyors of commercial tests, so that critical scientific data, normally associated with establishing the validity and psychometric properties of constructs and their measures, are simply not available. Instead, he notes that proponents such as Goleman support their ideas with selective anecdotal evidence, not subject to scientific scrutiny. To a large extent, we agree with this criticism. Goleman (1995, 2000) and Goleman et al. (2002) have all too often resorted to anecdotal evidence; and MHS has been somewhat secretive in releasing data to researchers, although it must be noted in defense of MHS that this is a commercial operation, entitled to protect commercial-in-confidence archival data. Nonetheless, we acknowledge that many of these data do indeed lie outside the "scientific tent". So this is a restriction, but hardly a fatal flaw. It simply means that researchers and commercial test distributors like MHS need to work cooperatively so that a sufficient body of research data does come into the ambit of scientific research. In fact, this is occurring, as *bona fide* researchers (including the authors of this article) receive discounted use of the MCEIT for scientific research.

The result of all this is now being reflected in a steady stream of scientific research activity centered on emotional intelligence, and the Mayer-Salovey model in particular (see the following article, as well as Mayer, Salovey, & Caruso, 2004a,b, for the most recent summaries of research based on the four-branch model). Landy provides a bibliographic analysis of studies of emotional intelligence. We do not want to engage in a blow-for-blow argument on these figures, but suffice to note that, at the time of writing the present article, a (rather restrictive) keyword search for "emotional intelligence" in PsychINFO (1985-present) resulted in 545 hits, including 2 in the

Journal of Applied Psychology, 3 in *Personnel Psychology*, and 1 in *Human Performance*.[i] This compares to the total of 4 for these journals counted by Landy about six months earlier – not bad considering the highly selective editorial policies of these prestigious journals. (The count for the *Journal of Organizational Behavior* is 3.) In effect, and as one would expect in a burgeoning field of research, the volume of quality research publications is steadily mounting; and this is despite the negative attitudes to emotional intelligence research held by many senior scholars in our field – who often review for these journals (as represented by the contributors to this issue).

Landy devoted a considerable number of pages to the proposition that the notion of "social intelligence", rooted in an article penned by Thorndike in 1920, and published in the popular *Harper's Magazine* (Thorndike, 1920), is invalid. In fact, this issue seems to be lie at the core of Landy's objection to emotional intelligence as a valid construct. The question is, however, whether emotional intelligence research is appropriately characterized as a form of social intelligence. Goleman (1995, 2000) would probably agree with this proposition. But what about Mayer and Salovey? In fact, Mayer at al. (2000) have gone to some lengths to distinguish emotional intelligence from concepts of social intelligence. They argue that emotional intelligence is essentially about *emotion*. In this respect, emotional intelligence is founded in modern understanding of the role of emotional circuits in the brain (see Mayer, 2000). Thus, while emotional intelligence is an example of what Mayer and Salovey (in press) refer to as "hot intelligence", it is nevertheless distinct as a construct from the other members of this category, including social and spiritual intelligences. From our own perspective (Ashkanasy & Daus, 2002; Ashkanasy, et al., 2002), we see ourselves primarily as scholars of emotion, focusing on

the antecedents and consequences of emotion in organizational settings. Based on our training as researchers in psychology, however, it is not surprising to learn therefore that we were interested in the role of individual differences in the way that people at work deal with their own and others' emotions. In this sense, the four-branch model of emotional intelligence seems to best address our research and applied needs.

At the same time, we acknowledge that emotions play a critical role in developing and maintaining social relationships (Ashkanasy, 2003) As it turns out, however, although different from the notion of social intelligence, emotional intelligence seems to be a key ingredient in the process of developing social relationships and working with other people in groups (Lopes & Salovey, 2001). In the context of workgroups, in particular, recent research by Jordan and Troth (2004) and Offermann, Bailey, Vasilopoulos, Seal, and Sass (2004) has demonstrated that, while intellectual intelligence is the pre-eminent predictor of individual work performance, group performance is more a function of emotional than intellectual intelligence.

In summary, there is little to be gained in making comparisons between the mainstream of research into emotional intelligence and past attempts to find a workable model of social intelligence. In terms of what Thorndike (1920) meant by his references to social intelligence, there seems also to be little point in speculating on his intentions. The fact remains that he did write about social intelligence and, as such, caused scholars to think about intelligence as a wider phenomenon than previously. Today's emotional intelligence researchers are continuing that tradition. It is clearly far too early to conclude they will fail just because of fruitless earlier attempts in this field.

Landy's final point concerns the nature of current research in emotional intelligence. He argues that cross-sectional studies of emotional intelligence have added little or nothing to the prediction of workplace outcomes above that predicted by personality and GMA (and is supported by Conte on this point). He further notes that ambiguity about definitions of emotional intelligence has obfuscated the area even more, and that emotional intelligence researchers should focus their efforts on understanding leadership and development of positive organizational attitudes and behaviors. In fact, current research in these respects is proceeding vigorously (e.g., se Salovey, Mayer, & Caruso, in press). In the second article in defense of emotional intelligence, we present a comprehensive and up-to-date summary and review of research based on the four-branch model of emotional intelligence.

In summary of Landy's three points of criticism, we feel that they provide an unconvincing case for abandoning research into emotional intelligence. It's true that there has been a measure of debate as the new construct has been developing; but this is a healthy process in scientific research (Jordan, Ashkanasy, Härtel, 2003). It's true that the concept of social intelligence has had a checkered history, but emotional intelligence is a distinct concept, grounded in theories of emotion, so there is no reason to imagine that emotional intelligence research will flounder similarly. And it's also true that there is much to be done before we understand fully the nature and effects of emotional intelligence. In this respect, Landy's suggestions for future research in the field are appreciated.

Locke: Theoretical underpinning of emotional intelligence

Locke's case against emotional intelligence goes much deeper than Landy's. While Landy's objections deal with the pragmatics of definitions, research, and application, Locke seems to have issue with the whole idea; for him, "emotional intelligence" seems like an oxymoron. We respond to his case in two parts. First, we proffer that Locke fails to acknowledge more recent trends and research in emotions. Second, he does not distinguish between the mixed models of emotional intelligence and the four-branch model, which, for the time being at least, remains the only scientifically defensible model of emotional intelligence.

In many ways, Locke's arguments reflect the reluctance of organizational scholars prior to the 1990's to engage with emotions, as noted by Pekrun and Frese (1992), Ashforth and Humphrey (1994), and Weiss and Brief (2002). From this point of view, emotion and affect represent illegitimate areas for research because the world is inherently perceived through a cognitive lens, characterized by Ashforth and Humphrey as "the norms of rationality" (p. 101). As Weiss and Brief point out, however, recent advances in our understanding of emotion and its effects have now totally discredited this view (see also Lane, 2000, for a recent review of the neural bases of emotional experience). This research demonstrates, for instance, that Locke's point that individuals cannot reason *with* emotion is, in fact, not correct. While it's true that the idea of separation of mind and body has been a key tenet of scientific philosophy since Descartes, this idea has now been superseded. This is illustrated in a passage from Damasio's (1994) book *Descartes Error*, where the author describes a poignant example of a patient who had suffered damage to a part of his brain associated with emotional

experience. This patient demonstrated high IQ in all the standard tests, and was capable

of driving his car on icy roads while all around was mayhem. But, when it came to

making a simple decision about a date for his next appointment, Damasio's patient

proved totally incompetent. Indeed, despite his high IQ, he was completely unable to live

an independent life. Damasio explains this in terms of "somatic states" (or bodily

feelings) that play an essential role as shapers of cognitive thought processes, especially

thoughts that involve evaluative comparisons and judgments. It seems that people do

indeed reason *with* emotion. More recent research on emotions (e.g., see Lane, 2000) has

further elaborated on this idea.

Locke also makes the rather surprising allegation that advocates of emotional

intelligence seem to be motivated by some form of political agenda. In this sense,

proponents of emotional intelligence become left-wing advocates of the idea that

everyone can become intelligent in some way. Landy also alludes to this notion. Again,

this seems to reflect the rigid cognitive-behavioral view that for so many years inhibited

research into emotion in organizations (Ashforth & Humphrey, 1994). On the contrary,

and as we noted above, our own work has been motivated by a desire to understand the

role of emotion in organizations and work life. In this respect, from the perspective of

organizational scholarship, we believe that we (together with emotion researchers around

the world) are adding a new dimension of understanding to our field. Nothing in our

background suggests a Utopian perspective regarding people and their intelligence levels.

In fact, as academics, we are in favor of rigorous cognitive standards for admittance to

colleges and graduate programs…hardly the perspective that Landy and Locke accuse us

of. We simply see emotional intelligence as an way to explain incremental variance in

important organizational outcomes. We thus reject completely any idea that we are running a political agenda.

Locke devotes a considerable portion of his criticism to the work of Goleman, and especially Goleman's ideas on emotional intelligence and leadership. This is a point he forcefully made at the 2004 meeting of SIOP (Van Rooy, 2004). Indeed, he has failed to distinguish Goleman's position from ours (Daus & Ashkanasy, 2003; Jordan et al., 2003) and that of Mayer and his associates (e. g., Mayer et al., 2004a), both of whom have disavowed Goleman's position.

The picture that emerges of Locke's position is that he appears to be ideologically wedded to the idea – widespread in the period from the 1950's to the 1980's (see Weiss & Brief, 2002) – that emotion is an inappropriate subject for serious research by scholars of organizational behavior and I/O psychology. In the end, Locke suggests that a more fruitful area for research is "introspection", which he characterizes as "an important human skill". This is true, but introspection is not based in emotional processes, as is emotional intelligence (see Ashkanasy, Ashton-James, & Jordan, 2004), and is therefore quite differentiated from the concept of emotional intelligence. Jordan and Ashkanasy (in press), for example, have shown that introspection (which they refer to as emotional self-awareness) is a predictor of group process performance over and above the effect of emotional intelligence.

In summary of Locke's position, we reject outright his (and Landy's) assertion that emotional intelligence research is politically motivated. Moreover, we believe that his views are representative of an outmoded model of organizational behavior, where there is no place for the study of emotion, nor of the processes that underlie emotion. We

are of the view that modern theories of organizational behavior have moved on from this
view, as represented in Brief and Weiss's review published in the 2002 edition of the
Annual Review of Psychology.

Conte: Measures of emotional intelligence

Of the three articles in this issues that are critical of emotional intelligence,
Conte's is the most balanced and pragmatic. Our major criticism of his argument is that
he fails to give sufficient recognition to the primacy of the Mayer and Salovey (1997)
definition of emotional intelligence. As such, he continues to press the view that the
alternative models proposed by Goleman (Sala, 2002) and Bar-On (1997) are legitimate
alternatives to the model proposed in Mayer and Salovey (1997). In this respect, he
commits the same error as Van Rooy and Viswesvaran (2004) in that he 'compares
apples and oranges.'

We thus do not dispute the criticisms Conte makes about the ECI and the *EQ-i*.
And there is little point is disputing points in respect of the MEIS (an early Stream 3
measure), which has been superseded by the MSCEIT (currently available in Version
2.0). Moreover, we feel that he presents an honest evaluation of the MSCEIT, which is
not without it problems, as acknowledged by its developers (Mayer et al., 2000) and also
by its critics (e.g., Matthews, Zeidner, & Roberts, 2002). The main issue he identifies is
that there are no 'objective truths' on which to base the 'right answers' to the MSCEIT
items. The MSCEIT is scored in terms either of consensus norms, or is based on average
scores obtained from a panel of experts. Clearly, there are problems with both
approaches, as Conte identifies. Alternatively, it does not seem to be all that
unreasonable to presume that a person who answers "angry" in response to an image that

is evaluated as a happy face by either method would be scored 'wrong'. Thus, while the consensus and expert ratings are not strictly and objectively 'correct', they can still be regarded as reliable indicators, and have proven so in subsequent research (see Mayer et al., 2004a)

The question, therefore, is not so much whether the mechanics of scoring the current version of the MSCEIT are correct or not, but whether research can demonstrate that the MSCEIT has reliability and validity in field tests. The latest reviews by the scale's authors suggest that it does (e.g., Mayer et al., 2004a). Furthermore, as we establish in the second of the articles in this issue, the preponderance of data emerging in the I/O field appear to be strongly supportive as well.

Perhaps the most telling point of criticism that Conte makes is that the results of the studies he reviewed suggest that measures of emotional intelligence, and in particular the Stream 1 measures (especially the MEIS) fail to deliver sufficient incremental predictive power over traditional measures of personally, attitude, and behavior. He thus echoes Landy in comparing emotional intelligence to the social intelligence movement of the 1920's and 30's. Our research in this respect (see our next article in this issue) would seem to offer a more hopeful prognosis for emotional intelligence, especially in organizational research.

In summary of Conte's position, we can only say that we are largely in agreement with his analysis, although we do believe that he may be prematurely pessimistic about the incremental explanatory power of emotional intelligence. On a more positive note, Conte, like Landy, offers some helpful advice for future research into emotional intelligence. Issues concerning the dimensionality of emotional intelligence, faking and

adverse impact, and cross-cultural differences are all topics that will keep emotional intelligence research healthy and vigorous in the foreseeable future.

Is emotional intelligence in organizational research dead?

The three critical articles in this issue are in many ways representative of the views of a good number of scholars in I/O psychology and organizational behavior. Our summation, however, is that they fail to consider fully the recent scientific work on emotions in organizations. In this respect, some of the most effective criticisms of emotional intelligence have come from among the ranks of emotions researchers themselves (e.g., Matthews et al., 2002). Ashton-James (2003), for example, while agreeing with the overall theoretical basis of emotional intelligence (as in Ashkanasy et al., 2004), has criticized the abilities measures of emotional intelligence on the basis that they can do no more than tap respondents' semantic knowledge about emotion. For Ashton-James, a true measure of emotional intelligence must place respondents into a context where they can actually experience the emotions that they are asked to respond to.

More particularly, critics such as Ashton-James (2003) and Matthews et al. (2002) recognize, and focus on emotional intelligence as defined by its originators. This is in contrast with the three protagonists in this issue who, in common with many of the lay views of emotional intelligence, fail to distinguish between 'the real thing' (as defined in Mayer and Salovey, 1997) and versions of the construct that have been promulgated by (among others) Goleman and Bar-On. A further overarching flaw in their arguments is that they see emotional intelligence as a modern manifestation of social intelligence. It matters little whether or not Thorndike (1920) first coined the term 'social intelligence.'

More importantly Thorndike, as well as contemporary scholars of intelligence (e.g., Gardner, 1983; Sternberg, et al., 2000), recognized that a model of intelligence based only on intellectual capacity is insufficient to explain human capabilities and behavior in real life. Damasio's (1994) brain-damaged patient, for example, invariably scored highly on IQ tests yet, because of his inability to experience emotion, was found to be incapable of executing simple decision tasks others perform routinely.

For us, the most surprising point of criticism was Locke's (and to a lesser extent, Landy's) assertion that the emotional intelligence movement is somehow politically motivated by egalitarian ideals so that "everyone will, in some form, be equal in intelligence to everyone else". We feel that this is reflective of a deeper commitment on the behalf of Locke to the idea that, somehow, emotion in not an appropriate topic for scholarly study and discourse in organization science. As such, and as we noted earlier, Locke seems to be unable to see past the outmoded model of thinking identified by Ashforth and Humphrey (1995) and Pekrun and Frese (1992), where explanatory theories of behavior in organizations must be expressed solely in terms of cognition and behavior.

The burgeoning interest in the study of emotions in organizations would seem to belie this idea. Since the publication of Pekrun and Frese (1992) and Ashforth and Humphrey (1995), there has been an outpouring of literature dealing with emotions in organizational settings, reflected in recent special issues of journals (e.g., Ashkanasy, 2004; Fisher & Ashkanasy, 2000; Fox & Spector, 2002; Humphrey, 2002; Weiss, 2001, 2002) and edited books (e.g., Ashkanasy, Härtel, & Zerbe; 2000, Ashkanasy, Zerbe, & Härtel; 2002; Fineman, 1993, 2000; Härtel, Zerbe, & Ashkanasy, 2004; Lord, Klimoski, & Kanfer, 2002; Payne & Cooper, 2001). The level of interest is so high that Barsade,

Brief, & Spataro (2003) have characterized it as an "affective revolution" in the study of organizational behavior. And leading textbooks (e.g., Robbins, 2005) now include substantial coverage of the latest research on emotions in organizations, including emotional intelligence.

It is in this context that the authors of the present article have developed their interest in emotional intelligence. As a consequence, we see emotional intelligence neither as some new form of social intelligence, nor as a substitute for intellectual intelligence. From our perspective, emotional intelligence is another tool that I/O psychologists and scholars of organizational behavior can use in their efforts to understand and to predict behavior. In earlier articles on this topic (Ashkanasy & Daus, 2002, Ashkanasy et al., 2002), we listed the following "safe" four-point summary of views on emotional intelligence:

1. Emotional intelligence is distinct from, but positively related to other intelligences.

2. Emotional intelligence is an individual difference, where some people are more endowed, and others are less so.

3. Emotional intelligence develops over a person's life span and can be enhanced through training.

4. Emotional intelligence involves, at least in part, a person's abilities effectively to identify and to perceive emotion (in self and others), as well as possession of the skills to understand and to manage those emotions successfully.

(Ashkanasy & Daus, p. 83)

We see little reason to retreat from this position, although recent research has extended the applicability of the construct to more and more aspects of organizational behavior, as will be outlined in the following review article.

The important message we have to researchers entering this field is to take care. They need to understand that emotional intelligence is defined in Mayer and Salovey (1997). While research is sure to elaborate and to challenge this theory (e.g., see Ashkanasy et al., 2004; Ashton-James, 2003), this is the working model of emotional intelligence that must form the basis of serious research in emotional intelligence for the immediate future. Moreover, researchers need to take time to read the emotions literature, so that they have a full understanding of the theoretical underpinning of emotional intelligence and of the role that emotions play in organizational settings in particular.

In conclusion, we posit that emotional intelligence research will continue to be a central plank of organizational behavior research for the foreseeable future. The study of emotions in organizations was for too long neglected, and the idea of an individual difference variable that focuses on emotional abilities, as reflected in the Mayer and Salovey (1997) model of emotional intelligence, is an entirely appropriate focus for researchers working in this field. At the same time, we welcome the sort of vigorous debate that is manifested in the SIOP debates (De Nisi, 2003; Van Rooy, 2004) and in the present issue. We see this activity as sign that the field of organizational behavior in general, and the study of emotions in organizations in particular, is flourishing. House, Shane, and Herold argued in 1996 that, "Rumors of the death of dispositional research are

vastly exaggerated" (p. 203). We suggest that the same holds true in respect of emotional intelligence in organizational behavior research.

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Footnotes

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[i] Note that this number includes all journals, while Landy searched only empirical

articles.