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Understanding, Measuring, and Applying Emotional Intelligence: What Have We Learned? What Have We Missed?

Richard D. Roberts
Educational Testing Service, USA
University of Sydney, Australia

Ralf Schulze
Educational Testing Service, USA
Westfälische Wilhelms-Universität Münster, Germany

Moshe Zeidner
University of Haifa, Israel

Gerald Matthews
University of Cincinnati, USA

Summary

This chapter provides a synthesis of the theory, research, and applications surrounding emotional intelligence (EI) that are presented throughout the current volume. We note, for example, the breadth of the theoretical models that have been offered in discussing the concept of EI. Providing definitional issues are resolved and efforts towards demarcation of the subject domain are made, this may be a more healthy state-of-affairs than previously suggested. Measurement issues provide one of the more intractable problems currently facing the field, particularly dis-

junctions between performance-based and self-report approaches to assessment. Some considerable space is given to describing a possible rapprochement between these measurement approaches, along with some paradigms that we have recently developed. Applications of EI in the fields of education, organizational, and clinical psychology hold much promise, particularly if theory and measurement issues are satisfactorily resolved. This commentary ends with discussion of two additional areas that EI might find ready application—gerontology and affective computing—wherein we provide ideas for future research that might be profitably explored.

15.1 INTRODUCTION

In this concluding commentary, we set about the task of reconciling the various chapters. This is by no means an easy task as the authors often represent conflicting views and perspectives on the nature of emotional intelligence (EI). Nevertheless, we point out how each chapter contributes to the current state-of-the art in theory, assessment, and applications. We also highlight some areas that seemingly need to be considered in order to enhance current knowledge and understanding of EI.

One point of consensus emerging throughout this volume is that populist accounts of EI should find a firmer scientific foundation. The extent to which popular accounts have embraced the concept (or its various derivatives) is perhaps not surprising. Peddling the virtues of new, emerging intelligences—moral, sexual, promotional, naturalistic, entrepreneurial, political, cultural, spiritual; the list seems boundless—appears part of effective, twenty-first century marketing strategies by business-people, journalists, and media-savvy scientists alike.[1] In turn, these groups feed into the laypersons interest in self-help issues, often without the care required of emergent, scientific constructs. Equally, interest in EI owes much to sober attempts to develop and validate tests of EI (and other measures of affective processes), which, potentially, may be as important for psychological assessment as measures of academic, cognitive performance. Indeed, as commentators throughout the book attest, the concept of EI appears among the more promising of the new constructs emerging from psychological science that are directed towards improving the human condition. Moreover, the construct resonates with a popular zeitgeist that emphasizes personal growth, the minimization of psychological harm (to both self and to others), and an appreciation of elevated levels of self-esteem (Matthews, Zeidner, & Roberts, 2005, in press-a, in press-b; Salovey, Mayer, & Caruso, 2002).

[1]The phenomenon may not be as recent as we perhaps think. In a recent historical review, Landy (in press) notes that for various reasons, eminent psychologist like Thorndike (1920) might have done something similar in order to promote the virtues of early psychological research and differentiate it from its less scientific ancestors like phrenology.

Reviewed throughout the current volume, in almost every chapter, are a large number of tests that appear to meet conventional standards for reliability. Several of these tests of EI also possess at least some properties supporting their validity. At the same time, difficulties remain apparent with current approaches to understanding the concept of EI (see also Matthews, Zeidner, & Roberts, 2002). Conceivably, in being just over a decade old, the field is too new for definitive judgments, and in several instances the contributors to this volume go to great extremes to try and remedy the status quo. One feature emerging from these various commentaries is that the term *emotional intelligence* refers to multiple constructs, a sample of which may not represent forms of intelligence at all. Equally, since some of these constructs may already be encapsulated by existing theories of personality it appears problematic to develop new models around them. At the same time, individual differences in affective processes had received short shrift until recently; EI has focused scientific research on this doubtless important topic (MacCann, Matthews, Zeidner, & Roberts, 2004; Zeidner, Matthews, & Roberts, in press).

In the remainder of this concluding exposition, we recapitulate the promises offered throughout the present volume for the study of EI, while also suggesting domains of this emerging subdiscipline where there might be a need for more balanced discourse. In this commentary, we also alert the reader to certain pitfalls that may impede proper scientific progress if due caution is not exercised. In addition, we offer some suggestions for a more unified, scientific framework, discussing both a measurement and developmental model we have developed with this goal in mind. It is pivotal that EI also find meaningful applications and real life consequences; we review further evidence supporting the calls made by commentators in that section. Finally, we suggest some additional domains of applied psychology where we believe the construct of EI might be profitably explored.

15.2 THEORETICAL ISSUES

15.2.1 What We Have Learned

The range of theories of EI actually covered in this volume may be construed as daunting. First, there exists a great divide between so-called ability and mixed model approaches, with the latter, if the review by Pérez, Petrides, and Furnham (Chapter 9) is any guide, yielding over a dozen idiosyncratic theories tied to specific self-report measures. Second, even the well-known performance-based Mayer-Salovey-Caruso model, as noted by Neubauer and Freudenthaler (Chapter 2), has noteworthy differences between early and later versions, such that the reader should be circumspect in assuming that they have the same conceptual underpinnings. Further still, in this volume, an account is made outside of these approaches (doubtless not for the last time), with Ciarrochi and Godsell's (Chapter 4) attempt to mesh a theory from clinical psychology with EI concepts. Finally, we note from various commentators that there exists a possible rapprochement between social, emotional, and practical intelligence

that might not only add in definitional clarity across these various domains, but also the assessment of EI itself (see Austin & Saklofske, Chapter 6; Kang, Day, & Meara, Chapter 5; Weis & Süß, Chapter 10).

The question that springs to mind is whether this is a healthy state-of-affairs. The answer to this question is by no means straightforward. In intelligence research, which appears further advanced there appears a great deal of tolerance for alternative perspectives. Thus, some researchers favor a view of a single, important construct—psychometric *g*—while others talk of multiple cognitive abilities (see Roberts, Markham, Zeidner, & Matthews, 2005, for a recent review). Within these approaches there are also noticeable disjunctions. For example, the theory proposed by Gardner (e.g., 1993) assumes seven to ten multiple intelligences, determined largely on the basis of neurological, computational, evolutionary, and developmental criteria, and often a weak empirical base. By contrast, the theory of fluid and crystallized intelligence is steeped in psychometric evidence, test construction, and gerontological research, yet posits different constructs, albeit a similar number to Gardner (see, e.g., Carroll, 1993; Horn & Noll, 1997; Roberts & Stankov, 1999). Sternberg (e.g., 1985) provides a still different perspective. His triarchic theory encapsulates analytic intelligence, creativity, and practical intelligence.

Given this precedent, it is perhaps appropriate that there are so many different models of EI. However, consider several important facts. First, principles for measuring intelligence constructs are largely undisputed. Individual differences in cognitive ability can be determined on the basis of the responses to tasks scored correct/incorrect or determined as response per unit of time (see, e.g., Carroll, 1993; Guttman & Levy, 1991). Self-reports of intellectual ability have been utilized, but in general these are thought to provide different information from the actual test scores; notably, the term *intelligence* is generally not reserved for such measures (see Wilhelm, Chapter 7). Furthermore, taxonomic models have been posited, by which it is possible to locate both the universe of ability constructs and measures. Moreover, models underlying test performance have been linked to developmental, neurophysiological, cognitive, biological, and evolutionary concomitants. These ubiquitous and important features of ability models stand in stark contrast to the current state of play in EI research, which raises the possibility of researchers talking at cross-purposes (Matthews, Roberts, & Zeidner, 2004).

Fortunately, the contributors to the present volume, where possible, attempt to bridge these gaps. For example, Schultz, Izard, and Abe (Chapter 3) provide a much needed call to consider developmental models. Notably their arguments combine neurophysiological concepts, developmental evidence, emerging principles from educational practice, and measurement models. Wilhelm's (Chapter 7) appeal to consider the modeling of EI concepts and to suggest that some of these might be arranged in similar fashion to cognitive constructs is consistent with our contention that taxonomic models are needed to guide EI research. Legree, Psotka, Tremble, and Bourne's (Chapter 8) discussion of models of consensual scoring suggest too that it is possible to develop promising psychometric analyses for non-veridical responses (i.e., those not having a

clearly defined right or wrong answer). Chapters of this kind provide a much needed impetus towards a sounder theoretical basis for the conceptualization and measurement of EI, offering the potential to take it to a similar plateau to academic intelligence measures in rapid time. They also provide a number of noteworthy suggestions for principled, systematic research.

Perhaps more important, however, when it comes to evaluating the impetus of research in the domain of academic abilities, is the societal value given by the intelligence test. Many proponents of these instruments, rightly or wrongly, see this as the most practical contribution made to humanity by all of psychology (e.g., Anastasi & Urbina, 1997). Several lines of converging evidence support the pragmatic usefulness of intelligence tests. First, standardized tests of intelligence, multiple aptitudes, and academic achievement are widespread across the Western world, influencing individual life decisions en masse (Campbell & Knapp, 2001). Second, various meta-analyses indicate that measures of intelligence predict job and academic performance particularly well, in the process saving national economies billions of dollars (Roberts et al., 2005). Indeed, these instruments appear better suited for this purpose than any other measure of psychological, sociological, or demographic significance (see, e.g., Schmidt & Hunter, 1998). Finally, scores on intelligence tests have been implicated with physical and psychological well-being and quality of life constructs (Neisser et al., 1996), with studies also demonstrating that it is an important predictor of mortality (e.g., Deary & Derr, 2005).

The question that perhaps will be become most pertinent in any long-term evaluation of the importance of EI research might also be those psychological, sociological, and demographic factors that it consistently predicts. This possibility is certainly acknowledged in, and arguably may even be at the crux of, many of the chapters comprising the present volume. Thus, besides each of the chapters focusing on educational, organizational, and clinical applications (i.e., Goetz, Frenzel, Pekrun, & Hall, Chapter 11; Abraham, Chapter, 12; Parker, Chapter 13), where prediction is clearly vital, we are presented with data that are suggestive of the predictive validity of EI measures in a variety of domains (sometimes for the first time). For example, Schultz et al. (Chapter 3) review several studies in their own laboratory where emotion expression, emotional, and situational knowledge predict teacher ratings of social skills, behavioral problems, and objective measures of academic competence in first- and third-grade children. The studies reviewed by Engelberg and Sjöberg (Chapter 14) suggest relations between measures of emotion perception and various indices of social adjustment in adult samples. Indeed, proposed relations between EI and factors like social skills, social support, and other indices of social adaptation, as they define it, are further buttressed by the studies examined by Austin and Saklofske (Chapter 6), as well as studies conducted by Lopes, Salovey, and Strauss (e.g., 2003).

The onus will be on researchers to replicate and extend these findings and, providing a corpus of knowledge is reached, undertake the kind of principled

meta-analyses advocated by Schulze (2004).[2] Careful demarcation of the criterion space will also need to be undertaken; simply correlating self-report measures against other self-report measures runs the risk of criterion contamination. Moreover, those criteria that are most important, are more likely to be variables that are outside the traditional criterion space (i.e., number of widgets, worker output derived from supervisor ratings, etc.), though certainly those it might predict are clearly vital (e.g., life satisfaction, lowered absenteeism, citizenship behaviors). Furthermore, EI may be useful for the prediction of certain job clusters (e.g., those in the health and service industries) and not others. As evidenced in the discussions by Austin and Saklofske (Chapter 6), Kang et al. (Chapter 5), and Weis and Süß (Chapter 10), it will be necessary to not only show the extent that EI provides incremental validity over personality and academic intelligence but also exhibit how EI measures differ from the related constructs of social and practical intelligence. Nevertheless, the fact that data on the predictive validity of EI constructs is accruing stands as testament to the potential of the field.

15.2.2 What We Have Missed

Each of the commentators dealing with theoretical issues highlight the need for greater conceptual coherence, positing models that offer a rapprochement between developmental (Schultz et al., Chapter 3) and evolutionary (Ciarrochi & Godsell, Chapter 4) antecedents of EI, or else offering a compelling case as to why one approach to the conceptualization of EI—often in terms of performance components—is superior to others (e.g., Neubauer & Freudenthaler, Chapter 2). The missing ingredient, arguably, is how each of these disparate aspects might coalesce, in a similar fashion to what has happened with cognitive abilities, to move the field forward. Integrating each of these features is no trivial undertaking, yet the onus to do so rests with the current authors. In the passages that follow, we attempt to provide an integrative summary of the preceding chapters, which also encapsulates features that may have been overlooked by the contributors.

Implicit in virtually every chapter is the prospect that EI refers to multiple constructs that are weakly, though meaningfully, related to one another. For example, measures of self-reported EI, like the SEIS correlate around .30 with performance-based measures like the MSCEIT (e.g., Wilhelm, Chapter 7). Similarly, as Austin and Saklofske (Chapter 6) demonstrate, a cognitive measure of emotional processing based on the inspection time methodology correlates around $-.30$ with self-reported EI. The correlations between self-report EI also tend to vary considerably, seemingly because some such as the BarOn EQ-i are largely proxies for personality measures (see Neubauer & Freudenthaler, Chapter 2), while tests like the TEIque tend to be based more on a concep-

[2]We note that at least one meta-analysis has already been conducted with EI measures (Van Rooy & Viswesvaran, 2004), though as we suggest later in this chapter, there are several problematic features associated with it.

tual match to the Mayer-Salovey-Caruso model, and hence may act more like self-reports of intelligence than proxies for personality (Pérez et al., Chapter 9). There is also evidence from performance-based measures that emotion perception and assimilating emotions form a separate higher-order construct (i.e., Experiential EI) that is moderately correlated with a second-order factor comprising the understanding and managing branches (i.e., Strategic EI) (Wilhelm, Chapter 7; also Mayer, Salovey, Caruso, & Sitarenios, 2003). Of note, this latter set of distinctions parallels that offered by Kang et al. (Chapter 5) when they suggest it might be judicious to consider separate forms of fluid and crystallized social-emotional intelligence.

Toward a unified measurement framework: The four-source model. It appears feasible that there are discrete sets of constructs discussed throughout these chapters that may be differentiated psychometrically, in terms of processing concomitants, and in terms of adaptive significance. In Table 15.1 we list four of these constructs, while drawing parallels with similar constructs from the literature on intelligence, as well as some comments on developmental influences that likely operate in each instance.

It is worth noting that several commentators, among them Schultz et al. (Chapter 3), Kang et al. (Chapter 5), and Wilhelm (Chapter 7) explicitly make reference to the need for multiple constructs (which they variously refer to as *emotion systems*, *declarative emotion knowledge*, *fluid emotional intelligence*, and the like). In the passages that follow, we discuss our proposed integrative, working model in more depth, and highlight some potentially important research issues that need to be resolved in terms of it (see also MacCann, Matthews, et al., 2004). We suggest this framework as a comprehensive way to categorize the domain of EI (including its constructs, assessments, and underlying processes). To date, research has only focused on the measurement of EI and its relations to other constructs, but has not tried to create an overarching framework for the field. This framework, which draws ready parallels to the approach that Weis and Süß (Chapter 10) advocate for social intelligence (SI), was developed to circumvent this limitation.

Temperament. The dimensions of childhood temperament (Schultz et al., Chapter 3) map onto adult personality dimensions such as neuroticism, extraversion, and conscientiousness that, in turn, are highly correlated with many EI questionnaires. Complexes of various biological and cognitive processes support such dimensions, the adaptive consequences of which are not easily traced since intricate and multifaceted (Matthews, Emo, Funke, Zeidner, & Roberts, 2003; Matthews et al., 2005; Matthews, Zeidner, & Roberts, in press-a). For example, although distress-prone children may have difficulties in interacting with the caregiver, vulnerability to distress may also attract the caregiver's attention, and promote risk-avoidance. Much is known of temperamental qualities (e.g., Rothbart & Bates, 1998), though as we note in subsequent discussion, this appears an important aspect of EI certainly in any attempt to develop a comprehensive developmental model.

Table 15.1 Multiple Types of Construct That May Contribute to Emotional Intelligence

Construct	Possible measure in chapters	Equivalent in IQ research	Key processes	Adaptive significance	Developmental influences
Emotionality-Temperament	Scales for Big Five personality EQ-i ECI	None that are direct, though links to Openness	Neural and cognitive processes controlling arousal, attention, and reinforcement sensitivity	Mixed: most temperamental factors confer a mixture of costs and benefits	Genetics and early learning
Emotional Self-Confidence	SEIS sub-components TEIque subscales	Self-assessed intelligence	Self-concept and self-regulation	Predominantly, but not exclusively positive: presumed similar to self- esteem	Learning and social-ization: for example, mastery experiences, modeling, etc.
Emotional Information Processing	Lightfoot Facial Expressions, Emotional Inspection Time, Emotional Stroop	Choice Reaction Time, Inspection Time, working memory	Specific processing modules	Uncertain: Is speed of processing necessarily adaptive?	Genetics and early learning
Emotional knowledge and skills	MSCEIT, LEAS, SJTs	Crystallized intelligence (and related aptitudes)	Multiple acquired procedural and declarative skills	Adaptive within context for learning: may be irrelevant or counter-productive in certain other contexts	Learning, social-ization and training of specific skills and knowledge

Note. EQ-i = Emotional Quotient inventory, ECI = Emotional Competence Inventory, SEIS = Schutte Emotional Intelligence Scales, TEIque = Trait Emotional Intelligence Questionnaire, MSCEIT = Mayer-Salovey-Caruso Emotional Intelligence Test, LEAS = Levels of Emotional Awareness Scale, SJT = Situational Judgment Test.

Emotional self-confidence. A novel aspect of EI research is that it provides assessments of an individual's belief of the extent that they can manage emotions and interpersonal encounters (e.g., self-reported confidence in understanding emotional states). Emotional self-confidence may be at the core of questionnaires such as the TEIque (see Pérez et al., Chapter 9). This construct is akin to self-rated intelligence and at the process level might compare favorably with self-knowledge. That is, self-confidence may depend on the content of the self-schema shaped by social learning (Bandura, 1999). As such, it is likely to be more dependent on learning within specific contexts than is temperament. Like self-esteem, high emotional self-confidence may be predominantly adaptive, but with a dark side, taking the form of narcissism, denial of problems, and excessive self-enhancement (Baumeister, Smart, & Boden, 1996). We note, in passing, that alternative means, other than self-assessment, exist for measuring self-confidence (e.g., having participants make confidence judgments after responding to cognitive test items; see e.g., Pallier et al., 2002).

Emotional information processing. Individual differences in processing stimuli of positive or negative valence are best known from personality research. For example, extraversion and neuroticism may relate to small biases towards prioritizing positive and negative stimuli, respectively (Rusting & Larsen, 1998). It is unclear whether factors for processing emotive stimuli exist (e.g., whether some individuals are quick to recognize negative stimuli). However, factors that define aptitudes for processing emotional stimuli appear an aspect of EI. Austin and Saklofske's (Chapter 6) findings with the Emotional Inspection Time paradigm are important here, as is our own recent work with the Emotional Stroop paradigm (O'Brien, MacCann, Reid, Schulze, & Roberts, 2005). There also appears to be a factor for accurate emotion perception, discussed for example by Engelberg and Sjöberg (Chapter 14), which may share relations with the Experiential EI component of the MSCEIT (Matthews, Zeidner, & Roberts, in press-a). If factors of this kind exist, they would appear to constitute abilities. A general factor for such abilities might correspond to fluid intelligence (Gf) in the abilities domain, especially given similar measures of cognitive processing from the intelligence domain correlate highest with Gf (Roberts & Stankov, 1999). However, the adaptive value of such factors remains to be explored; it is unclear that rapid processing of positive stimuli and slow processing of negative stimuli (or various permutations thereof) is necessarily beneficial. Plausibly such a factor might improve over the course of schooling and decline in the later years of life.

Emotional knowledge. EI also appears related to acquired, contextualized skills for handling specific encounters, such as calming an upset friend. Conceivably, such skills have similar properties to cognitive skills. Thus, although emotional self-confidence may facilitate acquisition and execution of skills, skills are numerous, and specialized for specific problems. Similarly, depending on levels of practice and the stimulus-response mapping (consistent or varied), skills will likely vary on an explicit-implicit continuum. Implicit skills

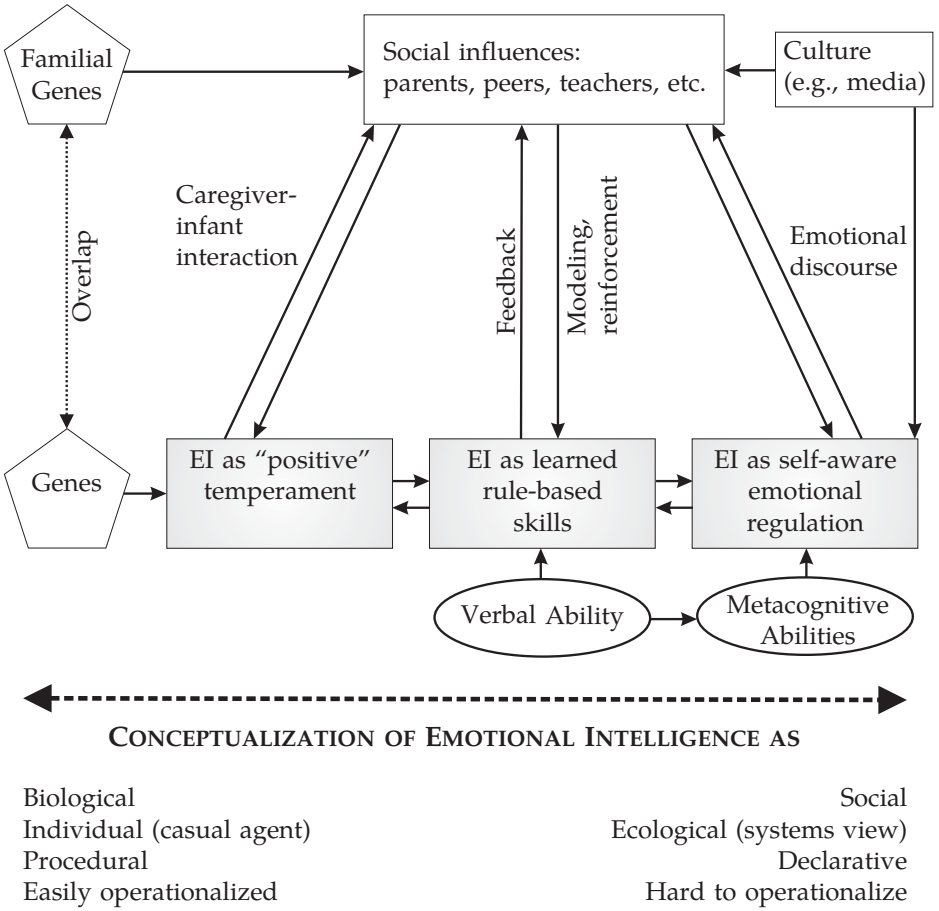

Figure 15.1 An investment model of emotional intelligence (see also Zeidner et al., 2003).

perhaps resemble crystallized intelligence, while explicit skills might correspond to declarative knowledge of emotions (Ackerman, 1996). It is likely that the understanding and management branches of the MSCEIT involve explicit skills, with implicit skills important to perception and assimilation (Neubauer & Freudenthaler, Chapter 2). By and large, increased knowledge is adaptive, but it may transfer poorly across different situations. The fact that these various types of emotional components are related to knowledge, suggests that they are likely to improve over the school years, perhaps late into life, and be susceptible to various forms of intervention.

A developmental framework: The investment model. Consistent with ideas contained in Schultz et al. (Chapter 3), Zeidner, Matthews, Roberts, and MacCann (2003) have suggested that the multiple constructs discussed above may be linked developmentally rather than structurally. Their investment model, akin to that proposed for cognitive abilities, is shown in Figure 15.1.

The model describes how developmental processes may generate associations between different components of EI, such that lower levels (e.g., posi-

tive temperament) are invested through experience into the development of higher levels of EI (e.g., emotion management). In particular, a child's positive temperament, when invested in interactions conducive to the acquisition of emotional knowledge, produces returns of declarative, rule-based knowledge about emotions. This knowledge, when invested in interactions in emotional situations, produces returns of self-aware emotion regulation, social skills, and the like. The model describes how basic temperamental qualities influence the acquisition of emotional skills and self-understanding. For instance, temperament appears to interact with situational factors to influence rule-based skills. Much of this learning is verbal in nature, such that verbal ability also facilitates skill acquisition. However, aspects of temperament (e.g., being excessively distress-prone) may disrupt the child's conversations with the caregiver, delaying emotional skill learning. The older child acquires skills that are more dependent on insight into self and others, allowing a more flexible response to interpersonal situations. Both temperament and rule-based competence may moderate insight-based learning. In addition, metacognitive awareness and regulation of personal thoughts and feelings become increasingly important. The adult thus possesses a varied repertoire of emotional responses, ranging from low-level emotional modulation (temperament), through simple rule-based skills, to more complex competencies based on insight (emotional knowledge).

The investment model is suggestive both of long-term macro developmental processes (continuing into adulthood), and the short-term micro processes that govern responses within a specific interpersonal encounter or emotive event (Zeidner et al., 2003). Personality research identifies various developmental patterns of person-situation interaction. One developmental pattern is that distress-prone temperament (linked to adult trait anxiety and neuroticism) leads to hypervigilance for threat, avoidance of feared social situations, and diversion of attentional resources to process internal worries rather than the environment. This configuration of response reduces exposure to emotional stimuli, meaning fewer opportunities to develop emotion recognition skills (e.g., Wells & Matthews, 1994). The resultant skills deficits lead to further avoidance, and maladaptive self-beliefs and metacognitions that typically lead to further withdrawal. Similarly, temperamental activity and impulsivity (corresponding to adult extraversion) lead to engagement with challenging situations, and hence greater opportunities for learning skills for handling exciting (but potentially risky) encounters. Thus, temperamental traits may influence emotional development both directly, via individual differences in emotion and attention, and indirectly, through exposure to emotional situations and opportunities for practicing and learning skills for specific emotional challenges.

It is also possible to look at a micro-process for the interaction between the individual's traits, skills, and the environment to determine how the different EI constructs relate to the cognitive processes that mediate adaptation to situational challenges. The leading theory of the adaptational process is Lazarus's (1999) transactional model, which includes several sub-processes.

Appraisal refers to the evaluation of the personal significance of an event, and the likelihood of successfully coping. Appraisal depends on multiple information processing components, including fast, unconscious evaluations and consciously accessible processing that is more flexible and context-sensitive (Scherer, 2001). Coping includes efforts to manage demands appraised as threatening, overtaxing, or challenging (e.g., Shimazu & Kosugi, 2003). Coping strategies are varied; they differ in the extent to which the skills required are well-learned and routine, or require controlled processing in order to formulate a new strategy for dealing with exigent or unfamiliar demands (Matthews & Wells, 1999). Thus, both appraisal and coping may recruit different levels of processing depending on the nature of external demands, and the person's repertoire of skills for understanding and managing the situation concerned.

The idea of relating EI to individual differences in these cognitive stress processes is appealing. There is some conceptual overlap between the literatures on stress and on EI, and indeed each of the four different conceptualizations discussed previously may play some role in adaptation. Emotionally intelligent individuals might, for example, have more accurate appraisals or be better able to focus attention on the stimuli critical for resolving a difficult social encounter. Information processing may also influence some of the more automatic, less consciously accessible aspects of appraisal, influencing speed of making emotional judgments. EI might also relate to the subset of coping strategies that are directed towards adaptive emotion-management and processing, such as emotion repair and emotional disclosure (Salovey, Bedell, Detweiler, & Mayer, 1999). In a recent review, however, we suggested that existing research literature does not support the notion of a continuum of adaptive competence, though we also welcomed future efforts directed towards resolving this issue (Zeidner et al., in press). To fulfill this objective, we also proposed that research linking EI, coping with stress, and adaptation would be need to be guided by the following principles: (1) clearer conceptual and psychometric discrimination of the multiple constructs related to EI; (2) a stronger focus on mediating mechanisms; (3) a stronger focus on situational moderators of EI constructs; and (4) a greater emphasis on building causal models using data from experimental and longitudinal studies.

15.3 MEASUREMENT ISSUES

15.3.1 What We Have Learned

Almost all of the chapters comprising the current edited volume, whether they be theoretically focused or slated towards discussing applications, have also at their core some additional concern with measurement issues. The number of assessment instruments discussed in this volume is large and, in light of our preceding commentary, touch on disparate constructs that collectively fall under the broad umbrella of *emotional intelligence*. Also included, is fairly detailed discussion of social and practical intelligence measures. The lessons learned, from each of these chapters, are many and varied. In this section, we

offer a distillation of core perspectives, some potential promises, and a series of conceivable pitfalls that should be avoided.

Pérez et al.'s (Chapter 9) account is worth noting, most especially in light of the large number of self-report measures that propagate the field. A similar array of instruments populates the field of personality assessment, and it is worth drawing to the reader's attention that these have been the subject of detailed comparisons and cross-tabulation. For example, Goldberg (in press) has formed the International Personality Item Pool (IPIP) website[3] that essentially references all known personality instruments with respect to the Five Factor Model of personality. Conceivably, it may be in the best interests of EI researchers to at least show the extent that self-report measures map onto each other. Currently, it appears that there is a notable disjunction between these measures, rendering them talking at cross-purposes and creating a virtual Tower of Babel (MacCann, Matthews, et al., 2004). If our previous account is correct, the divide between these measures may indeed be extreme—some relate to temperament, others to self-rated EI, while still others might represent an amalgam of these two domains. While Pérez et al. (Chapter 9) have admirably isolated a number of self-report measures they fall short in drawing commonalities and divergences between them. This is no fault of these authors; the vast majority of researchers working with self-report measures seem content to suggest that their new instrument is superior to all others, without requisite attention to empirical instantiation.[4] Landy (in press) has recently attributed a similar state-of-affairs at early attempts to measure SI, which has arguably left that field short of reaching its full potential.

Wilhelm's (Chapter 7) account of measurement issues is thought provoking. While we feel the criticisms of self-report measures per se are certainly pertinent, we suspect too that there may be something more to them. Indeed, the notions of typical intellectual engagement and need for cognition have been at the core of Wilhelm's own research interests, with some remarkable evidence for incremental validity (Wilhelm, Schulze, Schmiedek, & Süß, 2003; see also Ackerman, 1996; Cacioppo, Petty, Feinstein, & Jarvis, 1996). We suspect that there may be a dimension of self-motivated cognition to emotionally relevant stimuli that, to our knowledge, no EI researcher has attempted to fully develop (see, however, Epstein, 1998). Notwithstanding, the type of structural models that Wilhelm proposes appears an urgent research issue, worthy of empirical investigation. Moreover, his suggestion to consider paradigms like the Levels of Emotional Awareness Scale (LEAS; Lane, Quinlan, Schwartz, Walker, & Zeitlin, 1990) is one that finds resonance among the current commentators as well as several contributors to this volume. Clearly, the more objective such indices, the more resemblance they will share with traditional intelligence measures, though commensurate with this, such tests run the risk of becoming

[3]http://ipip.ori.org/

[4]We note that such criticisms cannot be leveled at the actual co-authors of this chapter; Petrides and Furnham have been particularly active in studies that included various self-report EI measures in large multivariate designs (see e.g., Petrides & Furnham, 2001).

proxies for academic intelligence (more so given Spearman's [1927] concept of the indifference of the indicator). Even so, in later passages, we discuss potential objective measures of EI not considered elsewhere in this volume.

Legree et al.'s (Chapter 8) account of situational judgment tests (SJTs) and the methods that might be used to scores these are also in the spirit of finding new and innovative ways of assessing dimensions of social-emotional behavior. It is non-incidental that the consensual scoring paradigm advocated in the MSCEIT and MEIS was derived from Legree's (1995) original research examining measures of SI for military personnel. Elsewhere, we have drawn certain problems to consensual scoring procedures (Zeidner, Matthews, & Roberts, 2001), as well as some psychometric techniques that might increase both their reliability and validity (MacCann, Roberts, Matthews, & Zeidner, 2004). Leaving this issue aside, Legree et al.'s (Chapter 8) account is the first to lay out the full-blown rationale supporting this scoring technique, which we believe, with sufficient development, might lead to important advances in measuring EI and related constructs. Interestingly, as early as Thorndike (1920), so-called *in situ* tasks were advocated for assessing SI; why there has not been more concerted test development using SJTs is puzzling (Landy, in press).

The chapter by Weis and Süß (Chapter 10), albeit focusing largely on SI, is, in many ways, highly similar to our earlier exposition highlighting the need for multiple assessment techniques, and a means of classifying these appropriately, in the domain of EI. Multitrait multimethod (MTMM) designs have so far received relatively short shrift in the literature, and their call to consider this methodology (along with that made by Kang et al., Chapter 5) is well taken (more so given the success that they appear to have had in using such techniques). Indeed, Carroll (1993), among others, has advocated the usefulness of MTMM designs incorporating objective, self-, and peer-reports, with respect to SI research. We contend (like many of the issues raised in this section on measurement) that this approach is requisite in developing a more fully developed science of EI.

15.3.2 What We May Have Missed

The preceding discussion, as well as our exposition of the four-source measurement model, offers some interesting suggestions for forms of assessment that were not necessarily covered by any of the contributors. Some of these have been used elsewhere, particularly in emotions research, while others have a track record in personality research and industrial-organizational psychology. Still others have been developed in our own laboratories, largely as a result of our reviews and critiques in the area. Indeed, we are in the midst of developing a series of EI instruments, comparing these with tests like the MSCEIT and other emotion measures and collecting predictive validity in various countries (USA, Germany, Norway, and Australia) with different populations (young, elderly, community college, and university students). In the passages below, we briefly describe a selection of the measures we are examining in these studies, along with findings where available, or speculations as

to what they might actually assess. Our focus in these passages (as in this program of research) is on measures of self-confidence, information processing, and emotional knowledge, since we contend that measures of temperament have saturated the field.

Assessing emotional self-confidence. We have recently developed the Personal Introspection of Emotional State (PIES), a self-report measure that requires participants to rate the extent to which they agree or disagree with statements about their emotions in specific contexts. In the underlying model, it is hypothesized that emotional self-confidence involves different skills when directed towards the self versus when directed at others, and when the emotions involved are positive versus negative in terms of emotional-salience. In addition, the questionnaire is designed to measure all four facets of EI proposed by Mayer et al. (2003). Thus, through the complete cross of all of these dimensions, some 16 sub-scales have been designed, with 7–8 items per scale. To give the reader some impression of these scales, we present sample items in Table 15.2. Data are currently being collected on the instrument, which we intend to examine using confirmatory factor analytic techniques.

Assessing emotional processing. Of note, tests measuring the ability to recognize emotion in various stimuli (i.e., tests of emotion recognition ability; ERA) have not generally been conceptualized as measures of EI. Indeed, the disjuncture between experimental and individual differences psychology is apparent here (see Cronbach, 1957), though the former is tied more directly to theory and could with psychometric development more fully meet the demands imposed by the latter subdiscipline. Indeed, conceptual correspondence with branches of the Mayer-Salovey-Caruso model (especially, emotion perception) makes many of these tasks feasible candidates for assessing EI (see Wilhelm, Chapter 7). In Table 15.3 we list a selection of these, along with source references, and a brief description.

Consideration of these experimental paradigms might plausibly lead to a model of EI more closely resembling taxonomies common to the intelligence domain (see MacCann, Matthews, et al., 2004). With this fact in mind, we recently had 138 first-year university students complete the MSCEIT, two measures of ERA (JACBART and RAFL), and measures of fluid (Gf) and crystallized (Gc) intelligence (O'Brien et al., 2005). Exploratory and confirmatory factor analysis recovered the two MSCEIT higher-order factors (Experiential and Strategic EI) and Gc; however, there was also evidence for a third factor that combined Gf and the ERA measures. The study suggests that the relation between performance-based EI and ERA is not substantial and that ERA is more strongly related to Gf than the MSCEIT, a finding we preempted in our previous discussion of emotional processing.

Aside from measures of ERA, future experimental paradigms assessing emotional processing components might include: the emotional Stroop and derivatives thereof (e.g., taboo Stroop); variants of the Wisconsin card-sorting task utilizing emotional stimuli; and variations on search tasks utilizing emo-

Table 15.2 A Faceted Framework for Self-Assessment of Emotional Skills

	Self		Others	
	Positive	Negative	Positive	Negative
Emotion Perception	I can tell when I am happy	I feel flushed when embarrassed	I can tell if people are happy or not	I can tell when others are in a bad mood
Emotion Facilitation	I play happy music when doing boring tasks	When I am angry, solving problems is easy for me	I generate a positive mood in others when coming up with new ideas in a group	When I'm in a position of authority, I find that generating fear in others will make them more productive
Emotion Understanding	When I'm in a good mood I can usually tell people why	It is unusual for me to be unhappy without knowing why	When people are in good mood it's easy for me to see why	When people are glum it's easy for me to understand what caused it
Emotion Management	When I am happy, I feel like I have more control over my emotions	When I am in a bad mood, I feel like I have more control over my emotions	I can easily put another person in a good mood	If I wanted to, I could easily make someone feel angry

Table 15.3 Test, Sampling Domain, and Scoring Methodology for Tests of Emotion Recognition Ability

Test	Sampling Domain	Test composition	Scoring
JACBART	Emotion recognition for faces varying	56 items, one score	According to veridical criteria from FACS
ERT	Emotion recognition (in verbal labels, faces and simple contexts)	8×4-part item	Multiple choice with 1 correct alternative from among 4 choices
DANVA2-AF	Emotion recognition of facial expressions	24 items	Multiple choice with 1 correct alternative from among 4 choices
DANVA2-AP	Emotion recognition in tone and voice	24 items	Multiple choice with 1 correct alternative from among 4 choices
RAFL	Emotion recognition (for tone and voice)	30 items, one score	According to veridical criteria from acoustic research

Note. JACBART = Japanese and Caucasian Brief Affect Recognition Test (Matsumoto et al., 2000), FACS = Facial Affect Coding System, ERT = Emotion Recognition Test (Shimokawa et al., 2000), DANVA2-AF = Diagnostic Assessment of Non Verbal Affect—Adult Facial Expressions (Nowicki & Carton, 1993), DANVA2-AP = Diagnostic Assessment of Non Verbal Affect—Adult Paralanguage (Baum & Nowicki, 1998), RAFL = Recognition of Affect in a Foreign Language (Scherer, Banse, & Wallbott, 2001).

tional stimuli (e.g., finding and circling sad faces among an array of sad, scared, and angry faces) (MacCann, Matthews, et al., 2004). Indeed, there are several possibilities for constructing such tasks from cognitive and differential psychology; a principled selection of such measures included in a large-scale multivariate design might result in an empirically founded taxonomy of emotional ability. Indeed, resolving the dimensionality of emotional processing arguably appears a necessary step for theoretically enriching EI models.

Assessing explicit emotional knowledge. Of note, virtually all measures of emotional knowledge constructed thus far make use of consensual scoring techniques. Notwithstanding the advantages of this approach highlighted by Legree et al. in this volume, various commentators have expressed concern about the ease with which such rubrics might be coached, their legal and ethical soundness, and/or various other features that would make them unlikely to be used in high-stakes assessment (Kyllonen & Lee, 2005). For this reason, we have embarked on developing measures of emotional knowledge that have a response that may be scored objectively. One of these is the Affective Quote Completion Test (AQCT), which assesses the ability to label emotions

and recognize the relationships between them, the ability to understand complex feelings, and the transitions that can take place between them. A 20-item test, AQCT essentially measures emotional understanding using a veridical scoring procedure; that is, selected quotes on human emotions, uttered by famous philosophers, scientists, or literary figures, which have responses that may be scored as right or wrong. The measure uses a cloze procedure, in which key emotion-relevant words have been removed from these quotations. One's ability level is reflected in one's understanding of the words related to emotions and the relations among the words and the emotions themselves. Data are currently being collected on this new measure; one issue we are targeting is the extent to which this measure overlaps with crystallized intelligence.

Assessing implicit emotional knowledge. Most individuals can be thought of as having at least functional EI. For example, they can be considered to be respectful toward others, to abide by the rules of society, to accept legitimate authority, and to attempt to lead productive lives. In contrast, a small proportion of people may be thought of as emotionally illiterate (Goleman, 1995). These individuals are undependable and untrustworthy, frequently aggressive, and defiant of social norms. One form of emotional illiteracy appears to be aggression. James and colleagues (e.g., James, 1998; James et al., 2005; James, McIntyre, Glisson, Bowler, & Mitchell, in press) have spent the last ten years developing a new technique to break through aggressive individuals' attempts to mask their true dispositions.

Justification mechanisms are reasoning biases that operate below the level of consciousness of the reasoner. Unconscious or implicit biases in reasoning can be measured using indirect and objective procedures. On the surface, these problems appear to be basic reasoning tasks, which in fact they are. However, the real purpose of these problems, which is not apparent, is to draw out aggressive respondents' reliance on reasoning biases—that is, justification mechanisms—to determine what they believe is logical. What consciously appears to be rational versus implausible reasoning is determined not by reasoning skills, but by whether reasoning is (or is not) guided by unconscious justification mechanisms. These mechanism bear close correspondence to the management components of the Mayer-Salovey-Caruso model; hence, we consider this may serve as a measure of this component (notably distinct from expert- or consensus-scoring per se).

Based on the preceding rationale, James (1998) has developed the Conditional Reasoning Test for Aggression (CRT-A). This instrument has been shown to have acceptable psychometric properties and an average, uncorrected validity of .44 against behavioral indicators of aggression (James et al., 2005, in press). These results are consistent with prior conditional reasoning studies on achievement motivation, and indicate that it is possible to make reasonably accurate assessments of aggressive tendencies that people often attempt to deny or conceal. The results suggest further that it is possible to increase, perhaps substantially, the ability to predict whether people will behave aggressively in the future. We are currently collecting data on this instrument, along

with measures of self-report and performance-based EI, and a range of criteria (biographical data, social support, mental health, and emotional well-being).

15.4 APPLICATIONS

15.4.1 What We Have Learned

As we noted in discussion of theoretical issues, at least part of the importance of traditional intelligence research derives from demonstration of its ecological validity and practical utility. Claims for the relevance of EI to school, work, and family life appear part of its initial appeal, not only in popular instantiations (e.g., Goleman, 1995, 1998), but also in the titles (and content) of at least two previous edited volumes with a clearer academic focus (Bar-On & Parker, 2000; Ciarrochi, Forgas, & Mayer, 2001). Indeed, much of our own previous research has discussed the extent that EI may contribute to handling challenging events successfully in a wide array of domains, including the workplace (see Zeidner, Matthews, & Roberts, 2004), clinical intervention (Matthews et al., 2002), education (Zeidner, Roberts, & Matthews, 2002), and in social interaction, more generally (Aicher et al., 2005). The chapters by Abraham, Parker, Goetz et al., Engelberg and Sjöberg are thus in domains we have special interest in and appear recurring themes for showing the value-added of studying EI.

Organizational applications of EI. Abraham (Chapter 12) rightfully pays homage to the fact that many workplaces now acknowledge the importance of overall emotional climate and both team and individual levels of EI. Thus, corporations are currently selecting incumbents on the basis of EI assessment or else using these for succession planning, while seminars promising to increase EI have become the standard fare of staff-in-service. However, contrary to some of the data Abraham cites, her admirable attempts to develop a model of EI that is relevant in organizational settings, and a focus on showing how EI relates to organizational commitment, we suggest that the scientific evidence supporting workplace applications is often equivocal.

Thus, in reviewing research on the validity of EI in occupational settings, Zeidner et al. (2004) concluded that the various scales for EI are, at best, weak predictors of job performance. For example, in a study of customer service teams, Feyerherm and Rice (2002) found that, at the team level, the MEIS predicted some subjective performance criteria, including customer service but not productivity. Moreover, contrary to expectation, several significant negative correlations were found between the EI of the team leader and team performance. A more recent study conducted by Donaldson-Feilder and Bond (2004) showed that with psychological acceptance and job-control statistically controlled, EI did not significantly predict any major workplace outcomes, including job satisfaction.

Abraham (Chapter 12) cites the meta-analysis reported by Van Rooy and Viswesvaran (2004) as supporting associations between EI and occupational criteria. Importantly, many of these studies, even now, have not appeared in the peer-reviewed literature. Another concern that we have with this meta-analysis is that occupational studies typically use supervisor ratings; given that EI scales typically correlate with social involvement and desirable personality characteristics, such ratings may be confounded by a halo effect. Another serious problem is that studies have typically ignored the personality and ability confounds of EI tests, which might be responsible for the typically modest validity coefficients that have been reported. On the flip side, as we suggested earlier, the criterion that EI might need to predict in work settings remains poorly operationalized and in urgent need of attention. Abraham's (Chapter 12) insights into studies of organizational commitment are in the spirit of this call, as is her contention that more sophisticated methodologies are required other than quasi-experimental correlational studies.

We note, in closing this section, that the benefits of training EI in the workplace have also yet to be demonstrated satisfactorily. For example, Slaski and Cartwright (2002) found that a training program improved self-reported EI scores (as measured by the EQ-i), but had no effect on ratings of managerial performance. Training studies using objective measures to assess interventions would arguably be more compelling, with the interventions perhaps tailored to each of the dimensions provided by the four-branch model or perhaps even specific information processes. There is clearly a need to conduct these types of studies, as well as suitably designed longitudinal investigations, which would ultimately give measures of EI (if suggestive) greater impact in the occupational context.

Educational applications of EI. Goetz et al.'s (Chapter 11) insightful chapter on educational applications of EI contains a number of suggestions for instructional techniques (both for student use and teacher implementation) that promote social emotional learning (SEL; see Zins, Weissberg, Wang, & Walberg, 2004) and other closely related constructs. Consistent with this body of work, they emphasize that person-centered approaches are insufficient; the learning environment (including teachers, administrators, the family, and community) must also support SEL.

Programs instantiating these principles have a good record of success, with beneficial outcomes reported for mental health, antisocial behaviors, and academic performance (e.g., Greenberg et al., 2003). One issue of some concern though, not necessarily addressed by Goetz et al., is that of scalability. Thus, it is unclear whether these programs can be applied across a whole nation or in locales with weaker infrastructure than those where these programs are presently implemented. It is also unclear what recent conceptions of EI add to the research described by Goetz et al. Although educational programs capitalize on enthusiasm for EI, interventions are actually tailored towards specific skills (e.g., conflict-resolution) rather than a general factor (Zeidner, Matthews, & Roberts, 2002). It is also unclear whether training general EI would be more

cost-effective than focusing on the specific skills discussed by Goetz et al. (a point they certainly acknowledge).

Supposing a general factor of EI is found, the practical techniques of choice in many educational interventions will also depend on the conception adopted. Conceptions of EI can be divided into those primarily dependent on gene-environment interaction in early childhood (e.g., temperament) and those that are most directly influenced by learning and socialization (e.g., specific knowledge). In principle, temperament and information processing competencies might be altered in infancy and early childhood, within the range of reaction set by the child's genotype. However, without an adaptive analysis, there is little basis for choosing to do so (Matthews, Emo, Zeidner, & Roberts, in press).

An alternate strategy rests in exploring aptitude-by-treatment interactions, leading to recommendations that would allow the person to make best use of their emotional dispositions (Matthews, Emo, et al., in press). By contrast, providing the learner actively cooperates, emotional self-confidence, declarative knowledge, and procedural skills may be trained at any stage of life. Emotional self-confidence might be trained by assisting the person through learning experiences that build a sense of mastery. Generally, although a worthy goal, there is also a danger of building narcissism and indifference to personal limitations, commensurate with the growing awareness in psychology of the limited benefits of high self-esteem (Baumeister, Campbell, Krueger, & Vohs, 2003). Training declarative emotional knowledge appears less contentious, although, as with any skill, the person requires insight into its applicability.

These caveats notwithstanding, the Collaborative of Academic, Social, and Emotional Learning (CASEL) states that one of the main questions educators ask is how they can measure student social skills and how they can evaluate the quality and effects of SEL practices (Greenberg et al., 2003; Zins et al., 2004). To address this concern, CASEL plans to compile and create tools that (a) educators can use to assess SEL-related student outcomes and (b) schools and districts can use to assess implementation of SEL programs. Given their attempt to develop a theoretical model around educational interventions, we contend that many of the recommendations made by Goetz et al. (Chapter 11) might be of benefit to this initiative. It is hoped too that our four-source model might guide considerations related to the development of a scientifically sound assessment system for measuring the effects of SEL programs.

Clinical applications of EI. Direct applications of EI in clinical psychology have been more cursory than in the two previous applied areas discussed, though there are a series of studies that have emerged since Parker (Chapter 13) completed the writing of his chapter and this book going to press. These include studies showing EQ-i scores to be lower in an offender population (Hemmati, Mills, & Kroner, 2004), the TMMS to be related to borderline personality disorders (Leible & Snell, 2004), and the TEIque predicting deviant behavior at school (Petrides, Frederickson, & Furnham, 2004). Various reasons might be offered for the relatively slow transition of EI to clinical applications. Related concepts like psychological mindedness have been in clinical psychol-

ogy almost since its inception and may explain why the field has not so readily embraced EI; why invest resources studying an emergent construct when another with a considerable background literature, which it closely resembles, already exists? Moreover, as we noted earlier, up until the present point in time, applications of EI to understanding human behavior seem to have been directed more strongly towards positive psychology and that part of clinical psychology dealing with normal individuals: life skills coaching.

Parker (Chapter 13) does, however, provide an excellent exposition of alexithymia, a concept thought to be on the opposite end of a continuum anchored by high EI. The means for measuring alexithymia extend beyond self-report to include structured behavioral interviews and peer-ratings, with an impressive body of evidence supporting its biological, developmental, and other psychological concomitants. The fact that this concept shares moderate negative relations between self-report and performance-based measures of EI is suggestive; perhaps as with academic intelligence there is less differentiation at the lower end of the EI continuum. Even so, as with other applications, different implications for applied clinical psychology may derive from the particular form of EI assessed; how to intervene at the information processing level is less clear than how one might develop targeted clinical approaches for changing self-confidence and explicit knowledge. Parker's (Chapter 13) remarks concerning tailoring different forms of therapy (e.g., group versus individual) for individuals with alexithymia also appears worthy of consideration by those engaged in professional practice.

Social adaptation and EI. Closing this edited volume with a chapter on the relations between EI and social adaptation is non-accidental; the sorts of criteria discussed by Engelberg and Sjöberg (Chapter 14) are precisely those that may prove essential to establishing the study of EI as a legitimate scientific discipline. As with many of the other applied areas discussed, the significance of EI to a variety of socially relevant phenomena may, however, depend on the manner that EI is assessed. Relations between performance-based measures of EI and self-reported social support do constitute one of the more impressive findings in the field to date, more so given that shared method variance (and/or criterion contamination) can not explain the observed results (see Aicher et al., 2005; Lopes et al., 2003). We are currently exploring whether these findings are replicable with some of the new measures of EI that we have developed (e.g., the AQCT) or borrowed from other disciplines (e.g., information processing measures like the Emotional Stroop).

15.4.2 What We May Have Missed

There appear a number of applied fields where we might have spent more detailed time considering the status of EI by inviting specialists in a core domain. For example, as Ciarrochi and Godsell (Chapter 4) allude, the issue of physical health and well-being may be at the core of successfully managing emotional states (see also, e.g., Pennebaker, 1997). An entire chapter devoted

to that topic would not have seemed unreasonable. While Abraham (Chapter 12) covers organizational applications, there has also been a recent spate of articles devoted to specific applications of EI in medical (e.g., Bellack, Morjikian, & Barger, 2001), legal (e.g., Silver, 1999), and engineering (e.g., Marshall, 2001) professional practice. Further, as we have outlined elsewhere, there are pertinent human factors issues that research on EI might address (Matthews et al., 2003; Matthews, Emo, et al., in press). In the interests of space and time, we will outline two domains where EI is, or could readily be, applied beyond those domains of interest that we have already mentioned in this brief account. The first is gerontology, and the second, affective computing.

Aging and EI. Having found various practical applications, as well as being considered as part of the modeling of early human development, the time would appear ripe to consider EI across the adult lifespan. Thus far, information on this relationship is scant, though there are various studies that have linked both emotional regulation and memory for emotionally salient events to chronological aging (see, e.g., Carstensen, Pasupathi, Mayr, & Nesselroade, 2000; Charles, Mather, & Carstensen, 2003; Isaacowitz, Charles, & Carstensen, 2000). Four questions related to the conceptualization, measurement, and lifespan trajectory of EI appear pertinent to a detailed investigation. These are:

1. *Are there age-related differences in EI?* No studies have determined empirically how EI might rise or fall as a function of chronological age when assessed by various methodologies or their ensuing constructs (i.e., temperament, self-confidence, processing, or knowledge measures). Conceivably, some perceptual and lower level processing components of EI change in similar function to those linked to more pure, sensory and/or cognitive processes (i.e., visual perception, Gf), while higher-order measures, requiring the investment of language, emotional understanding, and metacognitive components change in similar fashion to knowledge components (i.e., crystallized intelligence).
2. *How do age-related differences in EI compare to age-related differences in ability and personality?* Processing measures of EI are poorly understood. Because they may simply represent common processes tied to different media (i.e., emotional stimuli versus stimuli containing verbal, spatial, or numeric material), it is important to include traditional measures of fluid intelligence and processing speed, in particular, when considering the developmental trajectory of emotional processes.
3. *What does EI predict across the lifespan?* Almost all previous predictive studies have focused on adolescent or workforce samples; the outcomes predicted by EI remain poorly specified with respect to to older adults. We contend that there are several that appear theoretically justified and practically important, including quality of life, loneliness, ability to cope with stress, and physical and mental health and well-being.
4. *Are there race and gender differences in EI, and if so, do they change across the lifespan?* One of the most appealing features of EI, especially in its var-

ious popular instantiations, has been the suggestion that it counters the pessimism contained in various academic treatises that cognitive ability is destiny (e.g., Herrnstein & Murray, 1994). Being on the one hand noncognitive (because of its emotional constituents) and on the other, cognitive (because information, reasoning, and metacognitive processes are simultaneously implicated), another appeal of the EI construct appears its promise in redressing issues associated with adverse impact (see, e.g., Sackett, Schmitt, Ellingson, & Kabin, 2001). In short, EI offers hope for a more utopian, classless society, less constrained by biological heritage and conditions where assessment of it does not presuppose "destiny" (Goleman, 1995). Despite these claims, the data on group differences in EI are scant. Norms for at least one published instrument, the EQ-i (Bar-On, 1997) show substantial cross-national differences in mean EI, but no corroborative evidence has been offered to show that such differences are in any way meaningful.

We have planned a large-scale, cross-sectional multivariate study investigating these four issues. Participants, from various ethnical backgrounds and age cohorts, will be administered many of the measures previously explicated in discussion of EI measures. Using structural equation modeling, path, and regression analysis with a range of criteria measures, assessed via self-report, biographical data, and peer-report, we hope to obtain a clearer understanding of the aging of the factors circumscribing the domain of EI.

Affective computing. Improved understanding of academic intelligence has generally been enriched by developments in both cognitive psychology and artificial intelligence (see, e.g., Carroll, 1993; Roberts et al., 2005). Significantly, leaders in the cognitive revolution, among them Simon, Norman, and Neisser, always envisaged better representation of affect in their models, a point that for many years seems to have passed relatively unnoticed (Picard et al., 2004). Over the past decade, however, there appears to have been a significant shift towards redressing this imbalance, giving rise to the field of affective computing. Picard (1997), a pioneer of this field, defines affective computing as "computing that relates to, arises from, or deliberately influences emotions . . . (and includes) giving a computer the ability to recognize and express emotions, developing its ability to recognize and express emotions, and enabling it to regulate and utilize its emotions" (p. 3).

Despite being a relatively new field, affective computing boasts an impressive array of applications in the research and/or development phase. These include technologies for mirroring affect, devices for assisting those with autism and those without effective speech communication; principles for improving correspondence over the internet; technologies for improving consumer feedback; and a range of interventions aimed at improving student learning, including those based on intelligent tutors that impact (e.g., through the use of avatars) or otherwise adapt (e.g., through monitoring interest level) to the learner and her/his environment (see, e.g., Picard, 1997; Picard et al., 2004; Trappl, Petta, & Payr, 2002). Research at the MIT Media Lab also appears

directed towards a variety of new measures of affective state. For example, Picard et al. (2004) report promising correlates of teacher's ratings of student affect from measures of chair pressure patterns (assessed with a device that records how postures shift during learning), upper facial features (captured using a sophisticated video camera and analyzed with a proprietary algorithm), and a skin-conductivity sensing glove that communicates wirelessly with the computer.

15.5 CONCLUSIONS

Our concluding commentary has suggested that there are many research issues requiring attention in order to advance a coherent scientific approach to the study of EI. We contend that the following are especially relevant:

1. Currently, there appear at least four different theoretical meanings attached to the concept of EI (as well as frequent conceptual confusions with other classes of construct). These different meanings carry with them various suggestions for further concept development, assessment, and real world applications. We have provided a working model that links several of these concepts, though clearly there is an urgent need to build on this model and explore its various implications both for research and practice. Related to this issue, there appears a need to develop fully formed developmental accounts, evolutionary hypotheses, genetic, biological, and cognitive models, and a scientifically grounded taxonomy. Advances in each of these domains will depend on valid measurement and carefully designed experimental, multivariate, and longitudinal studies that make use of advanced statistical procedures (e.g., polytomous IRT, Bayesian nets).
2. Across all disciplines of human endeavor, measurement is often considered that which separates science from pseudoscience. In the interests of advancing the field, we contend that a moratorium is needed on the development of still further measures assessing the more temperamental aspects of EI. Considerable resources are nonetheless required to develop and research information processing and emotional knowledge measures, in particular. Techniques should not be limited simply to existing approaches that borrow on self-assessment, consensual scoring techniques, and/or paper-and-pencil methodologies. Rather, attempts should be made to develop tests that are based in multimedia, with alternative scoring rubrics and a range of parameters, perhaps buttressed by the methods and technologies suggested from emerging advances in affective computing.
3. Applications of EI are already being touted in business, health and clinical psychology, human factors, education, and even educational policy. Conservatively, one might imagine that the science underlying the domain needs to be further advanced before its effectiveness is fully realized. At the same time, history suggests that a symbiosis between science

and practice is commonplace; the lessons learned from attempts to institute available research on EI into practice will feed into scientific models. Disciplines covered in the section on applications, as well as those we have added (i.e., gerontology and computer science), have previously impacted on the development of psychology from a fledgling discipline towards a more mature science. Prediction, implementation, intervention, and relevance to policy will ultimately stand as tests of the veracity of the field.

We contend that each of the aforementioned issues will require a number of scientists and practitioners to invest considerable resources of time, effort, and intellectual capital; perhaps the current reader pursuing an academic or professional career will be among those who direct their energy towards addressing some of these core concerns. Tantamount to such efforts will be demonstration that EI predicts important outcome variables (over and above personality and intelligence) and buy-in from various professional bodies, testing corporations, businesses, and/or governmental agencies. Given the history of intelligence testing per se and some of the ill-conceived research programs it has led to, it is hoped this will be done with due diligence.

Author Note

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