



Emotion Researcher

The Official Newsletter of the International Society for Research on Emotion

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EMOTIONAL INTELLIGENCE: FACT OR HYPE?



In this issue of Emotion Researcher, we tackle one of the most contentious topics in contemporary emotion theory: emotional intelligence. Introduced in the scientific literature in 1990, the concept has morphed into a ubiquitous meme, found on the covers of Time Magazine and countless self-help books. But are the scientific credentials of emotional intelligence solid, or is emotional intelligence all bark and no bite?

An Interview With Klaus Scherer



Read an interview with Klaus Scherer, one of the world's leading affective scientists. Klaus reminisces about his upbringing in Germany, reflects on the power of the many coincidences that have affected his life and discusses his latest views on emotions as synchronizing mechanisms, on appraisal, on musical emotions, and on emotional intelligence (in the picture, Scherer at the University of Giessen in the early eighties – with collaborators Ladd and Silverman)

Editor's Column

Click on the post's title to get a preview of what's inside, and enjoy!

ISRE Matters



Check out Arvid Kappas' latest column. ISRE's President discusses what emotion theorists can do to contribute in the fight against terrorism

Young Researcher Spotlight



The recipient of this issue's Young Researcher Spotlight is Dan Kelly, the author of Yuck!: The Nature and Moral Significance of Disgust.



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Editor's Column

Andrea Scarantino, Department of Philosophy and Neuroscience Institute, Georgia State University

We have all met people who appear singularly unable to understand and manage their own emotions and the emotions of others, despite being smart in other areas of their lives. Consider Uncle Joe, a successful cardiologist who recently began his Thanksgiving dinner by cheerfully telling the host that she had gained weight, loudly remarked about the turkey's lack of tenderness in front of the cook, and proceeded by sharing details about his sexual escapades during a recent trip to Thailand in front of children, clearly oblivious to the horrified looks on everyone else's face.

Or consider Job Candidate Sally, whose job talk on quantum gravity was widely considered to be a success, but who sank her chances to get an offer during dinner, when she ordered the most expensive wine on the list, yelled at a confused waiter in a fit of sudden rage, and confided to the stunned hiring faculty that she wrote several fake positive reviews on her own Rate My Professor page using pseudonyms. And yet, Sally thought the dinner had gone great.

There clearly is something wrong with Uncle Joe and Job Candidate Sally, although they are far from being globally dumb people. A first pass at what is wrong with them is to say that they both lack *emotional intelligence* or that they both engaged in *emotionally dumb behaviors*. This is because they seemed unable to predict how others would feel as a result of their actions, unable to contribute to a collectively cheerful mood and capitalize on it, unable to control their own emotional impulses and unable to read the emotional reactions of others on their faces.

The objective of this issue of *Emotion Researcher* is to delve into the concept of *emotional intelligence*, tracing its (brief) intellectual history, theoretical developments, recent challenges and practical applications. The first point to clarify is that Emotional Intelligence (EI) is both a *scientific construct* and a *cultural meme*. The scientific construct of EI was born in 1990, introduced by John (Jack) D. Mayer and Peter Salovey in a seminal paper (cited 7,362 times so far!). The cultural meme entered popular culture in 1995, when *New York Times* journalist Daniel Goleman took the publishing world by storm with his bestseller *Emotional intelligence: Why it can matter more than IQ*.

Ever since, emotional intelligence has led a double life: widely acclaimed on the shiny covers of Time Magazine and self-help books during the day, so to say, and extensively challenged on the drabber pages of scientific journals at night. Quite paradoxically, supporters of EI in the popular culture have promoted the concept for its alleged scientific credentials, while critics of EI have challenged the concept because the scientific facts do not seem to back up the popular hype.

In this issue of *Emotion Researcher*, we will try to understand what is the current status of EI as a *scientific construct*, leaving aside the *cultural meme* of EI and the hype (and money trail) that comes with it. We are guided in this endeavor by a cast of leading EI researchers.

Our exploration begins with [Neal M. Ashkanasy and Marie T. Dasborough](#)'s overview of the history of Emotional Intelligence research, with special focus on the development of the three main scientific measures of EI: task-based measures of EI as an ability, self-reported measures of EI as an ability and self-reported measures of EI as a personality trait. Ashkanasy and Dasborough are critical of personality measures of EI, but are generally optimistic about the scientific prospects of ability measures of EI, especially if task-based. They are highly supportive of the Mayer-Salovey-Caruso Emotional Intelligence Test (MISCEIT), which they characterize as the "gold standard" of EI testing.

[John Antonakis](#) has an unequivocally critical outlook on MISCEIT and on EI testing more generally. In his piece, he argues that EI as currently conceived is not a valid scientific construct. Antonakis makes the case that poor testing standards have been used to measure EI and to figure out whether it adds anything to the predictive value of personality traits and IQ. He also brings up a moral problem related to the way in which EI testing packages are mar-

keted to unsuspecting human resource managers. On his view, it is unethical to claim, as publishers of EI tests routinely do, that there is solid scientific evidence backing up that high EI scores lead to success on the workplace: this amounts to false advertising.

[Dana Joseph and Daniel Newman](#) are also critical of current EI research, although a bit more optimistic about its possible future developments. Besides questioning the incremental validity of EI measures for predicting other work-related outcomes, once personality and IQ are taken into account, Joseph and Newman also raise the issue that the scientific study of EI is impaired by the high costs involved in administering EI tests, which are copyrighted intellectual property. Their suggestion is that such tests should be made available to academic researchers for a nominal fee or for free. They also explore gender differences in emotional intelligence, and whether having high EI could have a 'dark side'.

[Moira Mikolajczak and Ainize Peña-Sarrionandia](#) distinguish between measures of emotional intelligence that focus on knowledge, abilities, and traits. They argue that Trait EI, measured through questionnaires, has a significant impact on well-being, health, relationships and work performance. Their central question is whether Trait EI can be improved through training. They provide a qualified "yes", and share some of the techniques used for improving Trait EI. You can also check a New York Times article on whether emotional intelligence can be taught [here](#).

These first four articles are followed by a "Topical Q&A" with [John \(Jack\) D. Mayer](#), the father of the science of Emotional Intelligence (jointly with Peter Salovey). Unlike regular interviews with prominent emotion researchers also featured in ER, "Topical Q & A"s focus exclusively on points of theoretical analysis. I presented Jack Mayer with some of the key challenges EI has received over the years, using the critical pieces in ER as a starting point, and he generously answered them, conceding some points to his intellectual adversaries, but also defending the value of EI as a scientific construct.

All in all, I think these five pieces on EI can provide us with a snappy, up-to-date and truly helpful overview of the main live issues in the science of emotional intelligence, highlighting both challenges and opportunities as we move forward. I am very grateful to the authors for working with me over an extended period of time to make their contributions comprehensible to an interdisciplinary audience.

The featured interviewee of this issue is [Klaus Scherer](#), one of the world's leading affective scientists. In a wide-ranging interview full of entertaining moments and scholarly digressions, Scherer traces the history of his intellectual life, offering a systematic overview of his influential research on a number of central topics in emotion science, with special attention to his Component Process Model of emotions. I am hopeful you will enjoy this interview as much as I did (and check out Klaus' advice on where to eat at ISRE 2015 in Geneva!).

ISRE's President [Arvid Kappas](#) uses his ISRE Matters column to discuss how emotion theory could help in the fight against terrorism, which is occupying an ever-increasing portion of our public discourse due to the fact that it has become hard to escape terrorist imagery in mass media. Some possibilities Arvid explores include studying countermeasures to emotional manipulation through imagery, helping law enforcement develop facial expression recognition systems, and helping to figure out how to block the emotional appeal terrorism exerts on some young people.

Last but not least, [Daniel Kelly](#), recipient of the latest *Young Researcher Spotlight*, gives us a thorough self-presentation on his cutting-edge research in empirically-based philosophy of mind, which covers three main topics: the origin and nature of disgust, the moral-conventional distinction and racial cognition and implicit bias.

As usual, be in touch with comments, suggestions, ideas for future issues, feedback on the website, and anything else that strikes your fancy. And enjoy this issue, of which I am especially happy.

Previous Editor's Columns

[Editor's Column – Musical Emotions Issue](#)

[Editor's Column – Emotions and Social Engagement Issue](#)

ISRE Matters – Emotional Intelligence Issue

In the Face of Terror: The Need for More Concerted Applied Emotion Research

March 2015 – Let me just state it at the beginning: I am sick and tired of being bombarded (pun intended) with images and stories of terrorist threats. The effects on politics, and on our local, national, and transnational conversations are as undeniable as they are pervasive. As a result, everyday life is tainted in the after-9/11 world by surges in fear, anxiety, anger, indignation, hatred, and other unpalatable emotions. This is not accidental, but by design. Terror *is* about emotions, as the name already implies. Surely, terrorism is not a new invention, but in the age of global mass-media in the second half of the last century, it has become more visual, more media-savvy and practically inescapable.



In this millennium, the boundaries between mass media and interpersonal communication have been blurred, as a result of the rise of the Internet and the use of powerful portable communication technology. Production (and reception) possibilities are now ubiquitous with more filming and editing power in a current smart phone than a small movie-making studio might have had a few decades ago. Suddenly, it feels like we have become pawns in a terrorist chess game. How so? We let them kidnap our senses, we let them invade our memories with grotesque displays of violence instead of, if you pardon my Reagan, *Just Saying No*.

Who has *never* indulged in clicking on a gruesome terrorist-produced video linked by a reputable news source only to regret it seconds later? Doing so is not only unhealthy for its lingering effects on memory: it also amounts to playing the terrorist game. There are many reasons to believe that ever more extreme violence is fostered by the knowledge that the outrageous images are being perceived by the desired targets. This explains the movie-like quality of the latest ISIS productions, with their sick attention for the gruesome detail, with their leaving nothing to the imagination. This amounts to violence porn. As Marion G. Müller puts it – [the best weapon against terrorism is oblivion](#).

This is all well and true in general, but as emotion researchers we have an additional responsibility, namely using our skills to unveil the role of emotions in the creation and propagation of terrorism, so as to provide our small contribution toward helping to address it. I do remember vividly having Nico Frijda as a guest-speaker during my decade at Laval University in Quebec City. Nico was truly upset about the atrocities happening in the Balkans at the time. He cried out “How is it possible that emotion researchers have nothing to say about this?” How indeed? In my current musings on what we could actually say, I find myself confronted with a lack of serious application of emotion science to the study of terrorism-related emotions, from those of the recruits, to those of the victims, to those of the impacted society at large. One possibility is that there is nothing truly helpful we could say about such topics. I refuse to believe it, and if it were true we ought to find ourselves another job. What I think is the case is that we do have knowledge that could be applied to shed light on the causes of terror, on the role of media in the communication of terrorist deeds, and ultimately on what we could do as a society to respond to this scourge.

Dealing with such large scale projects would demand that emotion science gets its hands dirty and collaborates with journalists, law enforcement officers, social workers and more generally all parties with a role in shaping the social forces that give raise to terror and give it a world-wide echo chamber. But who else should do this research but us? Who has more experience in emotion research than ISRE? So what should we do?

One of the problems with expanding the scope of our research and making it practically helpful in the way required is that our research budgets are typically ridiculously low compared to the task at hand. We might be excited to have a grant of 50k\$ but that does not buy much. It is enough if the job to be done is to figure out what facial expressions a few surprised undergraduates produce. It is a pittance if the job to be done is to figure out what leads an educated English undergraduate to become a ferocious killer like Jihadi John or to figure out what leads French high school young girls to be fascinated by terrorists in a chat room to the point of joining them in the Islamic State.

To be clear – such efforts would require complex interdisciplinary research. Emotions play some part here, but this is embedded in complex social and historical contexts, obviously. The task might be easier regarding issues relating to mass media processes and effects than dealing with the specific root causes of terrorism. There are many aspects of this story where emotions play an important role!

My own research has been well funded over the last few years, but as I was visiting a chemistry laboratory a couple of days ago, I could not help but reflect on the fact that the value of some equipment standing around in a corner would equal the total cost of my research of the last 5 years including all salaries and equipment. Serious emotion research of the sort required to provide even a small contribution towards helping defeat terrorism would cost serious money. Not one PhD. Not two. Not three. Not one lab. Not two. Not three. Not one country. Not two. Not three.

Consider this: globally, many millions of dollars are invested in behavioral software and training to identify potential perpetrators at airports – and emotions play an important part here. How solid is the evidence behind these programs? In many cases, software engineers and their consultants rely on hunches rather than established scientific data, or cherry-pick the work of individual emotion scientists. I am not saying their suggestions are necessarily wrong – even though [serious doubts have been raised recently](#). I am saying that the investigation of the underlying phenomena is lacking scientific systematicity. As large-scale collaborative efforts to verify the [reproducibility of psychological science are underway](#), the same should be done for the body of evidence specific to emotion science.

There is good reason to believe that neither of the two main competing theories (neuro-cultural vs. behavioral ecology) trying to explain the relationship between emotion and expression that software engineers or computer scientists often appear to take for granted [captures what is going on](#). However, this is currently a matter of us sitting on sofas and having a friendly conversation – should we not define a serious research program that would once and for all clarify whether we can trust facial activity enough to use it for the diagnosis of impending crime and terror?

I am not talking about individual researchers here. I am talking about a scientific panel formed by world-class experts that defines a series of practice-oriented studies, and tries to avoid idealization as much as possible, including all relevant variables, such as cultural differences, individual differences, contextual factors and so on, and aiming for solid replications across labs. You know, serious stuff. Not research politics. Not the usual turf battles between basic emotion theorists and their opponents. And if the answer is not definitive, at least we can be explicit about what the current scientific state of the art is.

This does not mean that such a project would solve all problems and automatically lead to the development of excellent software applications, e.g., regarding the interpretation of facial behavior. For example, in the debate on the validity of lie-detectors/polygraphs, serious panels have been installed. They have clearly concluded that [scientific evidence does not support the use of lie detectors](#) as conclusive proof in courts of law. That has [not stopped companies or agencies from using them](#). But at least the situation is clear – the scientific evidence is not solid enough to trust “lie detectors” conclusively.

Facial behavior is not the only area of research related to terrorism to which emotion researchers could contribute. Other areas ripe for scientific examination comprise the emotional and behavioral impact of consuming images of terror, the influence of mass-media portrayals of terror on political decision making, the emotions of perpetrators and victims of terror, and many other topics.

I am suggesting that our community of researchers reflects on this simple question: How can my research help? We should involve all of the disciplines that are part of the ISRE family and that can provide a helpful contribution – from sociology to biology, from psychology to anthropology, from philosophy to history, from political science to neuroscience. I am addressing here the issue of terror as a special and immediate challenge, but these reflections are related to the more general issue of applied emotion research.

Let me be clear, I am making these suggestions without prejudice as regards the results of such efforts. While not all outcomes are equally likely, most are possible when it comes to understanding extremely complicated societal phenomena involving emotions. But if we can say that something has been studied seriously and transparently,

then the arguments and recommendations we can make towards the media, towards governments, and towards other stake-holders can be a lot more serious. And serious we should be in the face of terror.

This is a new challenge, and we need the best emotion research to tackle it. We need joint scientific efforts, rather than individual musings under the shower or non-expert chit chat in talk shows – I hear too many of these whenever I turn on my TV. We have over a century of good ideas and data. It is time to put them to work towards solving a major social problem that is in danger of threatening our social lives for many years to come. So let's get serious about this now.

EMOTIONAL INTELLIGENCE: FACT OR HYPE?

Check out the articles in this issue of Emotion Researcher, which is devoted to the concept of Emotional Intelligence. Leading researchers in the science of EI cover the history of the concept, recent theoretical developments, persistent challenges and practical applications. The father of the science of EI, John (Jack) D. Mayer, then engages in a Q & A on whether emotional intelligence is a valid scientific construct.



**“Yes, I think I have good people skills.
What kind of idiot question is that?”**

Neal M. Ashkanasy, UQ Business School, The University of Queensland & Marie T. Dasborough, School of Business, University of Miami

[Reintroducing Emotional Intelligence: What it Is and Where We Stand Now](#)

John Antonakis, Department of Organizational Behavior, University of Lausanne

[Emotional Intelligence: The Hype, the Hope, the Evidence](#)

Dana L. Joseph, Department of Psychology, University of Central Florida & Daniel A. Newman, Department of Psychology, University of Illinois at Urbana-Champaign

[Emotional Intelligence: Some Research Findings and Remaining Questions](#)

Moira Mikolajczak, Research Institute for Psychological Sciences, Université Catholique de Louvain & Ainize Peña-Sarrionandia, Psychology Department, University of the Basque Country

[On the Efficiency of Emotional Intelligence Training in Adulthood](#)

John (Jack) D. Mayer, Department of Psychology, University of New Hampshire

[Q & A On Emotional Intelligence With John \(Jack\) D. Mayer](#)

Reintroducing Emotional Intelligence: What it Is and Where We Stand Now

Neal M. Ashkanasy, UQ Business School, The University of Queensland & Marie T. Dasborough, School of Business, University of Miami

The History of Emotional Intelligence

March 15 – While the term “emotional intelligence” was first introduced to English language readers in a dissertation by Payne (1986), the initial formal definition was published four years later by Salovey and Mayer (1990), who defined the construct as an *ability*, specifically an ability to perceive emotions in self and others, to understand emotions, and ultimately to manage emotions. The concept was subsequently popularized by *New York Times* journalist Daniel Goleman (1995), whose book, *Emotional intelligence: Why it can matter more than IQ*, became a bestseller and was even featured on the cover of *Time Magazine*. Mayer and Salovey (1997) later refined their definition into the “four branch” model, which involves four abilities:

- Ability to perceive emotion in self and others (e.g., correctly identifying a perceived emotional expression as fear)
- Ability to use emotion to facilitate cognitive activities like thinking and problem solving (e.g., knowing how to capitalize on a happy mood swing to engage in a creative task)
- Ability to understand emotional information (e.g. understanding how two emotions can blend into a third emotion)
- Ability to manage emotion in self and others (e.g. detaching from fear states that interfere with one’s functioning).



Based in this four-branch model, Mayer, Salovey, and Caruso (2002) devised an “abilities measure” of emotional intelligence, which they called the MSCEIT (Mayer-Salovey-Caruso Emotional Intelligence Test). The MSCEIT resembles an IQ test, in the sense that emotional skills are measured on the basis of performance on actual tasks (two tasks for each ability).

Examples of such tasks include identifying the discrete emotion displayed on a face (ability to perceive emotions), identifying the type of mood that would facilitate a certain cognitive activity (ability to use emotions), identifying whether or not contempt is a blend of anger and disgust (ability to understand emotions), and identifying an optimal regulation strategy in the emotional situation described in a vignette (ability to manage emotions) (see <http://www.emotionaliq.com/msceitexamples.html> for example of MSCEIT items).

At around the same time, however, and as a direct consequence of the popularization of the construct, a number of alternative measures of emotional intelligence began to appear. Ashkanasy and Daus (2005) categorized such measures into three “streams.” Stream 1 measures are based on the Mayer and Salovey (1997) definition of emotional intelligence as an ability, and they include most prominently the MSCEIT. Ashkanasy and Daus referred to the MSCEIT as the “gold standard” of emotional intelligence measurement.

Stream 2 measures also rely on the Mayer-Salovey definition, but they are based on self- or peer-report questionnaire instruments, rather than a task-based test. For example, instead of asking respondents to identify a particular emotional expression (as depicted in a photograph), respondents might be asked to indicate the extent to which they feel they are able to recognize emotion in facial expressions. Examples of Stream 2 measures include the WEIP (Workgroup Emotional Intelligence Test: see Jordan, Ashkanasy, Härtel, & Hooper, 2002) and the WLEIS (Wong and Law Emotional Intelligence Scale, Wong & Law, 2002). Since these Stream 2 measures are self-

reports about emotional abilities, we suggest that they might be better viewed as targeting perceived “emotional self-efficacy” (i.e., how emotionally intelligent we think we are).

Stream 3 measures take a further step away from MSCEIT. Besides usually relying on self-reports, Stream 3 measures also presuppose definitions of emotional intelligence that are different from the one proposed by Mayer and Salovey (1997). For example, the Bar-On EQi measure defines emotional intelligence in terms of intra- and interpersonal self-awareness, stress management, adaptability, and general mood (Bar-On, 2006), using a scale developed in the author’s doctoral research into the clinical condition of alexithymia (Ashkanasy & Daus, 2005). Another Stream 3 model is the so-called “Trait” model developed by Petrides and Furnham (2001), which emerged as a derivative of the Bar-On measure from factor-analysis.

Ashkanasy and Daus (2005) recommended strongly against use of Stream 3 measures, because they rely on self-reports (they are measures of perceived self-efficacy rather than emotional skills *per se*) and because they are largely confounded with personality measures many items overlap with items measuring personality traits such as extraversion). Despite these serious shortcomings, Stream 3 measures continue to be used extensively, both by practitioners and in the academic literature (see O’Boyle, Humphrey, Pollak, Hawver, & Story, 2011).

In Figure 1 below, we illustrate the relationship between the three streams in a space defined in terms of three broad psychological categories: intellectual intelligence, emotional intelligence, and personality.

As the figure indicates, measures of EI on the three Streams are significantly different in terms of their validity as measures of emotional intelligence. Stream 1 measures, which test emotional abilities on actual tasks, strike us as the most accurate measures of emotional intelligence, although they may reflect some contamination from IQ measures and personality measures (Antonakis in Antonakis, Ashkanasy & Dasborough, 2009). We place Stream 1 measures well within the “zone of validity for measurement of emotional intelligence.”

Stream 3 are on our view outside that zone, and appear much closer to personality measures; as we noted earlier, one of these measures (Petrides & Furnham, 2001) is explicitly referred to as a “trait” measure, so we place it squarely in the “zone of validity for measurement of social skills and personality”. The same holds for another Stream 3 self-report scale (see Palmer, Stough, Harmer, & Gignac, 2009), which was even developed from a factor analysis of personality scale items.

Stream 2 measures straddle the boundary between validity for measurement of emotional intelligence and social skills/personality. Also represented in the figure are IQ, self-report measures of social self-efficacy (e.g., Goleman, 1998; Lowe & Cautela, 1978), and self-report personality measures (e.g., “Big Five”, McCrae & Costa, 1987), which lie, respectively, in the “zone of validity for measurement of intellectual intelligence” and in the “zone of validity for measures of social skills/personality”.

In Defense of Emotional Intelligence

Soon after its popularization, Davies, Stankov, and Roberts (1998) published the first substantial academic criticism of the construct we are aware of, attacking its ontological and epistemological integrity, and noting in particular that self-report measures of the construct overlapped with measures of personality. The most vehement attacks,

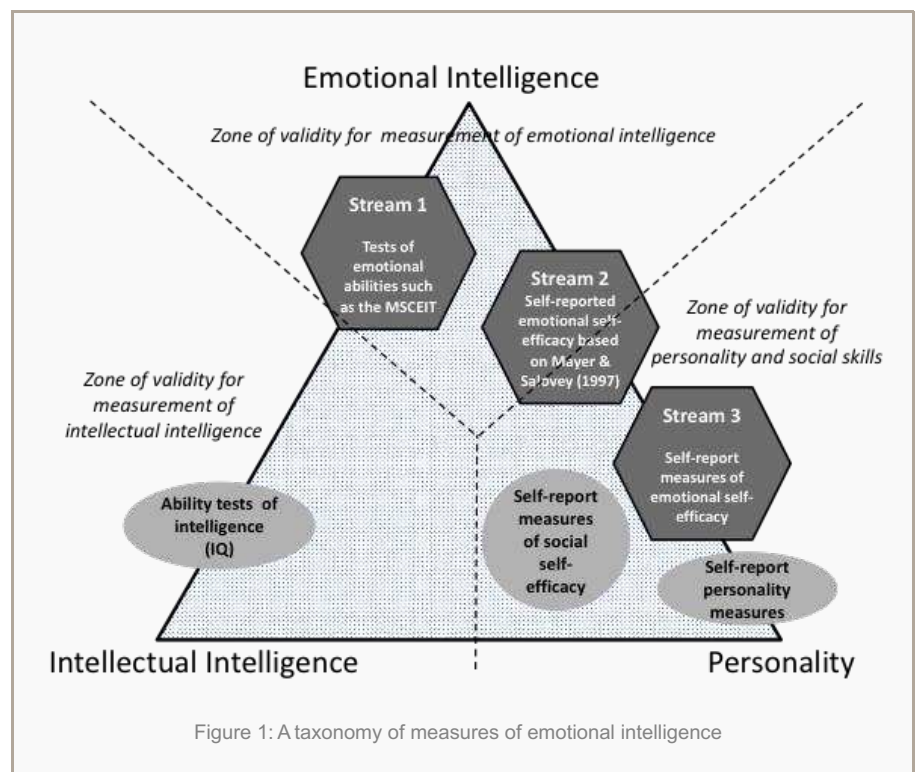


Figure 1: A taxonomy of measures of emotional intelligence

however, came from the field of industrial and organizational psychology, where noted authors (Landy, 2005; Locke, 2005) further emphasized that self-report measures of emotional intelligence tend to be confounded with personality measures, and took issue with some of the admittedly exaggerated claims made by Goleman (1995) and other popularizers of EI.

Other critics, for example Antonakis (in Antonakis, et al., 2009), have criticized the emotional intelligence construct at a deeper level, claiming that its purported predictive validity with regard to leadership effectiveness is confounded with personality (self-report measures) or intelligence (ability measures). Antonakis argues with particular reference to leadership studies that all the claims that emotional intelligence is a predictor of leadership acumen can be explained in terms of more established theories of personality and intelligence.

We acknowledge that there is much truth in many of the concerns regarding emotional intelligence (e.g., see Jordan, Ashton-James, and Ashkanasy, 2006) and in particular the overlap of Stream 3 scales with personality measures. Nonetheless, as we point out in Antonakis et al. (2009), this is not sufficient justification to dismiss the construct out of hand. The problems for EI measures, we think, lie primarily with Stream 3, and not with Streams 1 or 2. Ashkanasy and Daus (2005) argue in particular that emotional intelligence is, after all, just another individual difference variable, where some individuals score high and others score low on a battery of tests. In this regard, given that psychologists traditionally study individual differences, the question becomes one of establishing the construct validity of the measure, not a philosophical argument about the meaning of “intelligence” or whether or not emotional intelligence is really a form of “social intelligence,” as Landy (2005) and Lock (2005) argue. Moreover, given recent evidence regarding the neurological basis of the construct (e.g., Krueger et al., 2009, found a direct relationship between MSCEIT branches and prefrontal cortex substrates), the grounds for claiming that emotional intelligence is a totally invalid construct (cf. Locke, 2005) are fast evaporating.

A Maturing of the Field

Despite the early controversy surrounding the definition of emotional intelligence and its measurement, scholars recently came to some agreement. In 2010, Cary Cherniss initiated a dialogue between scholars representing the various streams in order to clarify the construct. The conclusion reached within this dialogue was that scholars using the Stream 3 approach should either refer to their approach as measurement of personality (e.g., Petrides & Furnham, 2001) or use the label “ESC = Emotional and Social Competencies” rather than the label of emotional *intelligence* (e.g., see Goleman, 1998). Stream 3 scholars acknowledged that they were examining more than just emotional abilities, and that they were looking more broadly at social skills and emotional skills. It was recommended that only Stream 1 and 2 research (i.e., research based on the ability-based model), should use the label of emotional intelligence (Jordan, Dasborough, Daus, & Ashkanasy, 2010).

Since 2010, we have seen evidence of the maturing of the field through the publication of meta-analyses on emotional intelligence (e.g. Joseph & Newman, 2010; Kong, 2014; O’Boyle et al., 2011). Although these were not all highly supportive, they do demonstrate that the field has now matured to the point where there are now sufficient empirical studies available to settle the debate on the nature and value of emotional intelligence on empirical grounds. Indeed, empirical research on emotional intelligence has also begun to enter the top mainstream journals within the fields of management, organizational psychology, and organizational behavior. Prior to this, it was rare to see any mention of emotional intelligence in the top journals in these fields, and it was usually theoretical articles only (e.g. Huy, 1999; Jordan, Ashkanasy, & Härtel, 2002). Just in the past four years, there have been five studies published in the *Journal of Applied Psychology*, and one in the *Academy of Management Journal*. We note that all of these articles are based on Stream 1 measures, with the majority using the MSCEIT as their chosen test of emotional intelligence. (Studies using Stream 3 self-report measures simply do not seem to be published in these top tier journals).

So: What is emerging from this more sophisticated scholarship on EI? We think that recent work on emotional intelligence has firmly established that the construct is a type of cognitive ability applied to the emotion domain (McCann, Joseph, Newman, & Roberts, 2014). IQ is also a cognitive ability, but EI differs from it in that it is specifically the ability to deal with emotion. This should quell the earlier critics somewhat. For example, in another recent article, Ybarra, Kross, and Sanchez-Burks (2014), in contrast to critics such as Antonakis (in Antonakis et al., 2009), expressed enthusiasm about the future of emotional intelligence, provided well-established principles of psychological processing can be incorporated into measures of EI so as to enhance their predictive validity. Specific-

ly, Ybarra and his colleagues call for new types of EI research that include automatic and deliberate processing of emotional information, motivational principles, and person-situation principles to examine context (similar to Jordan et al., 2010). They conclude with very positive views about the future of research on emotional intelligence. In line with these scholars, we too feel that the future of the field is looking more stable and brighter than ever, and that further progress may be achieved in the future by exploring new approaches (e.g. developing context-dependent measures of emotional intelligence).

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Emotional Intelligence: The Hype, the Hope, the Evidence

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March 2015 – In 2001, I arrived at the psychology department of Yale University to undertake a postdoc; I thought exciting things were going on at the time in terms of broadening our understanding of human abilities. I went there as a pilgrim—to work on leadership (and “practical intelligence”) with Robert Sternberg. Peter Salovey’s work on emotional intelligence (EI) was beginning to get a lot of traction at that time. At the outset, I thought that the ideas and research programs around these alternative notions of intelligence, and particularly in EI, were laudable and done with good intent. Yet, when I left Yale, I left as a skeptic. Why?



To be clear, let me first say that Salovey and Mayer have reshaped conventional thinking; one of their key contributions to science has been the construct of emotional intelligence (EI), defined as the “ability to monitor one’s own and others’ feelings and emotions, to discriminate among them and to use this information to guide one’s thinking and actions (Salovey & Mayer, 1989-1990, p. 189). Although more than 20 years have passed since the publication of this seminal piece, the scientific credentials of EI are still very much in question. There are three major issues concerning the EI construct:

1. There is great disagreement in the scientific community with respect the conceptualization of EI, whether EI should be measured as an ability, akin to an intelligence test (which relies on scoring subjects’ performance using “objective” scoring keys), or if EI should be measured as a trait analogous to how personality is measured (using self-reports). This debate has no end in sight; the proponents of ability measures, with whom I sympathize, say that it is folly to ask subjects to self-rate an ability (e.g., like asking a subject to self-rate their general intelligence). The self-rating aficionados argue that ability measures are bedeviled with measurement issues.
2. Very poor testing standards have been used to assess whether EI can be measured, whether it is distinct from personality and IQ, and whether it matters for outcomes like job or leadership performance or other outcomes; when rigorous testing standards are used, the effects of EI vanish (e.g., Antonakis, 2009; Antonakis & Dietz, 2011a, 2011b; Cavazotte, Moreno, & Hickmann, 2012; Fiori & Antonakis, 2011, 2012). Because there is evidence that EI is strongly correlated with personality and general intelligence, and disentangling the effects of EI from the effects of the variable with which EI correlates is often ignored, whatever results are reported in the literature about the apparent predictive power of EI are highly biased and not trustworthy (Antonakis, Bendahan, Jacquart, & Lalive, 2010).
3. Exaggerated claims have been made, by popular writers but also publishers of what are apparently reputable EI tests, about the accuracy with which EI measures can predict success in a number of performance domains. These claims are highly problematic because many well-meaning human resources directors or other professionals use EI tests in selection or for clinical purposes; doing so is neither ethical nor economical.

I will address each of the three points in more detail below. But let me first lay my conclusion on the table: I am very doubtful that emotional intelligence as currently theorized and measured is a valid scientific construct (Antonakis, 2003, 2004, 2009); there are theoretical and empirical reasons for my positions. I return to the theoretical issues at the end of this article and focus here on the empirical ones. In order to be valid, a psychometric measure ought to have at least construct validity (it should elicit responses that reveal differences in the latent ability we are trying to measure) and incremental validity (it should predict practically useful outcomes beyond what competing constructs like personality and intelligence can already predict). But, EI as currently measured fails on both accounts. All measures of EI currently in use elicit responses meant to capture abstract knowledge about emotions, without gauging how individuals use such knowledge in real world situations. That is, the latent ability researchers are trying to measure, the ability to monitor one’s own and others’ feelings and emotions, to discriminate among

them and to use this information to guide one's thinking and actions, is not fully captured: no test exists whatsoever to predict people's *actions* in using this EI ability in real life situations, whereas this is the primary evidential ground we use to distinguish emotionally intelligent from non-emotionally intelligent people in ordinary life.

Furthermore, there are a priori reasons to think that EI as currently measured cannot be distinguished from other competing constructs, and add to their predictive power. For example, it is pretty clear that personality traits (mostly emotional stability, agreeableness, and openness) correlated strongly with measures of EI and play a role in monitoring one's own feelings and others' feelings. Moreover, the ability to use emotion knowledge to guide one's thinking and action depends in part on one's ability to learn condition-action scripts (i.e., if a subordinate is distraught by previous failure, act empathetically towards him or her). Scripts require repeated exposure to condition-action links (Abelson, 1981) and being able to abstract from these depends on general intelligence (Gottfredson, 1997, 2002; Schmidt, 2002; Schmidt & Hunter, 1998); the very definition of intelligence is about the ability to learn. Whether the scripts are about emotions or other concepts is, I believe, not relevant and neural underpinnings about there being more than one general intelligence are lacking (Antonakis, Ashkanasy, & Dasborough, 2009).

As concerns my position on the three major issues I listed above, first, I have been rather clear that if there is anything to EI—then ability tests, like the MSCEIT (Mayer-Salovey-Caruso Emotional Intelligence test) are the way to go (Antonakis, et al., 2009). It is harder to provide socially-desirable response on an ability test, because the “correct” answer is not so obvious. In self-rated tests, however, it is all too easy to select “I agree” when presented with statements like “I am sensitive to the feelings and emotions of others” (this is an item taken from the WLEIS test of Wong & Law, 2002); additionally, these self-rated tests overlap too much with known constructs (e.g., personality, intelligence) and do not predict performance measures once when controlling for the known constructs (Joseph, Newman, & O'Boyle, in press).

Yet, evidence is starting to pile up that even the venerable MSCEIT, the Mayer-Salovey-Caruso Emotional Intelligence test, and the theory underlying it may be flawed (Amelang & Steinmayr, 2006; Antonakis & Dietz, 2010; Fiori & Antonakis, 2011, 2012; Fiori et al., 2014; Føllesdal & Hagtvat, 2009; Maul, 2012; Zeidner, Matthew, & Roberts, 2001). The problems identified by these researchers are manifold. Briefly, and focusing on the MSCEIT—the flagship measure of the ability movement—its four factors or branches (perceiving emotions, understanding emotions, managing emotions, and using emotions in thought) do not appear to reflect a general (i.e., higher-order) construct of EI, as conceived by the original authors (Mayer, Caruso, & Salovey, 1999). Such results raise doubts about its construct validity, and call into question the idea that one's global EI is causally responsible for the scores obtained on the four branches (see Fiori & Antonakis, 2011 for further discussion).

An additional worry relates to the scoring system used in MSCEIT (Fiori & Antonakis, 2011; Legree et al., 2014; Matthews, Zeidner, & Roberts, 2002; Maul, 2012; Roberts, Zeidner, & Matthews, 2001). Unlike most IQ tests, which have clearly established objective answers, EI tests are calibrated based on experts' judgments (“expert” scoring) or majority respondent ratings (“consensus” scoring). Unfortunately for the developers of the test, correlations between scores of experts on the test correlate close to unity (i.e., r is approximately equal to 1.00) with scores of lay individuals; how can commoners score so highly with experts? Who are these experts anyway? It was reported that they are 21 emotion theorists gathered at ISRE 2000, but who are they and what are they experts on exactly? Are the items on the MSCEIT test too easy? Can a consensus-based scoring system truly detect expertise in a tested domain, which presumably requires being above average?

Finally, there is growing empirical evidence that the MSCEIT factors overlap way too much with personality and intelligence (e.g., Fiori & Antonakis, 2011; Legree, et al., 2014; Schulte, Ree, & Carretta, 2004)—far more than its architects are willing to acknowledge. The problem is not that correlations are present; but, that they are so strong suggests EI is in serious danger of redundancy.

A lot of EI “eureka” moments—that is apparent discoveries showing that EI really matters for success—have been based on very lax and even sloppy application of basic psychometric testing principles. The litmus test of validity is incremental validity, whereby the key construct is examined alongside competing constructs. For instance, if one claims to have a fast horse (EI), this claim should be examined by testing the horse's speed relative to other horses known to be champions (personality, intelligence). Yet, most studies testing EI fail to control for competing constructs like IQ and the big five personality factors, and this using robust designs (e.g., correcting for measure-

ment error, using a correct model specification, etc.).

This has not prevented developers of EI, who have commercial interests in the construct, from making extravagant claims about the predictive power of emotional intelligence. For example, Goleman, Boyatzis, and McKee (2002, p. 251)—whose trait model of EI blends almost every imaginable trait that is not general intelligence into an unwieldy mix (Sternberg, 1999)—stated the following: “To get an idea of the practical business implications of these [EI] competencies, consider an analysis of the partners’ contributions to the profits of a large accounting firm ... those with strength in the self-regulation competencies added a whopping 390 percent incremental profit—in this case, \$1,465,000 more per year. By contrast, significant strengths in analytic reasoning abilities added just 50% more profit. Thus, purely cognitive abilities help—but the EI competencies help far more.” What all this means from a statistical and validity point of view is quite unclear.

To really know, however, if a variable predicts performance one must refer to well-designed studies and ideally to meta-analyses, a statistical technique that pools together the results of many independent studies. These analyses have clearly established that the single most important predictor of work performance is general intelligence, and that the correlation between general intelligence and performance increases as job complexity increases (Salgado, Anderson, Moscoso, Bertua, & De Fruyt, 2003; Schmidt & Hunter, 1998). These results, when corrected for statistical artefacts (measurement error and restriction of range) show that the correlation of general intelligence to job performance is about .70, even when controlling for conscientiousness and emotional stability (Schmidt, Shaffer, & Oh, 2008). To get an idea of the predictive strength of a correlation of .70, we can convert this statistic to a practical measure of effect (Rosenthal & Rubin, 1982); doing so shows that 85% of individuals who are above the median on general intelligence will score above the median in job performance; however, only 15% of individuals below the median on general intelligence will score above the median in job performance. In other words, a smart individual is more than five times more likely to have above median performance than will a less-smart individual. These validity coefficients are as good as it gets in psychology; I would be very surprised to see anything beat this in my lifetime!

EI does not even come close to matching the predictive power of general intelligence with respect to job performance. As summarized by Van Rooy and Viswesvaran (2004, p. 87) “EI did not evidence incremental validity over GMA [general mental ability, i.e., general intelligence]. However, GMA did significantly predict performance beyond that explained by EI. Thus, the claims that EI can be a more important predictor than cognitive ability (e.g., Goleman, 1995) are apparently more rhetoric than fact.” When testing for incremental validity via meta-analysis, researchers usually assume certain validity coefficients for competing constructs. Researchers must use the most accurate coefficients for those competing constructs so as to obtain unbiased estimates. If they do not, they will severely tilt the research record.

For instance, some researchers have “plugged in” values of .47 for the relation between intelligence and job performance in the statistical model tested (e.g., Joseph & Newman, 2010) instead of more realistic values (which are substantially higher, as I reported above); ironically, though, even in this this meta-analysis, where the cards were stacked in EI’s favor by using a validity coefficient for general intelligence that was understated by a hefty margin, these researchers reported that “measures of ability models of EI show only a modicum of incremental validity over cognitive ability and personality traits” (p. 69); another recent meta-analysis showed too that ability EI tests are not incrementally valid over personality and intelligence (see “Stream 1” results in Table 6: O’Boyle, Humphrey, Pollack, Hawver, & Story, 2011). Interestingly too is that even for a performance domain like leadership that is heavily emotions-based, there is no meta-analytic evidence that EI matters for leadership when controlling for personality or the big five (Harms & Credé, 2010).

Why then is research still undertaken with such zeal and why do practitioners still use tests of EI while repeating the mantra that “EI matters much for performance”? It is difficult to answer this question. One certainty is that there are commercial interests at stake (i.e., selling EI tests is a big business), which makes it even more important to pay attention to all the evidence, and ensure that EI tests are viable and do what they are intended to do prior to marketing them. Unfortunately, there is no body like the FDA to regulate psychometric tests—it is up to the academic market and the good consciousness of publishers to decide.

This leads me to the third issue I want to raise about EI, namely that it is borderline unethical to sell EI tests under

the false pretense that they are scientifically proven, when it is pretty clear that there is an ongoing scientific controversy on whether or not EI tests capture what they intend to capture and add anything to the predictive power of other existing constructs. Although lives are not directly at stake by using invalid EI tests, I find it unconscionable that money is being made from claims that are not backed up by very solid data.

At first blush, there is a clear distinction to be drawn in this respect between scientific researchers of EI and popularizers of EI. For example, Mayer, Salovey, and Caruso have repeatedly distanced themselves from Goleman's wildly unsubstantiated claims about the predictive power of EI, noting: "Our own work never made such claims, and we actively critiqued them" (2008). I praise them for taking this stance. The line is blurred, however, when we read what the publisher of MSCEIT has to say about the test: "A large and growing body of independent scientific research has identified it [EI] as the single most important determinant of superior functioning; emotionally intelligent people succeed because they are better able to read and deal with social complexity. As confirmed by independent academic research, one-quarter to nearly one-half of all job success can be attributed to Emotional Intelligence." All I can say to that is: *Gasp!* How can researchers of this caliber allow their publisher to make such claims? And how are such claims any different from the claims Goleman and company continue to peddle to the unsuspecting public?

A related worry is that the commercialization of MSCEIT stands in the way of its scientific testing. Suppose I wanted to run a test on the incremental validity of MSCEIT with 200 participants, the sort of study I have argued is sorely needed to establish whether or not MSCEIT predicts job performance beyond intelligence and personality. I would need to spend \$52.50 for a manual purchased with a researcher's discount, and \$6 per participant to take the on-line test, making for a total of \$1,252.50. This is an amount that many researchers might not be able to afford. So the very price of MSCEIT makes it hard for the scientific community to test its scientific credentials while the test is being sold as being backed by solid scientific evidence. Is there not something wrong with this picture?

Where do we go from here? Although I have so far focused on what's wrong with EI, I acknowledge that Mayer, Salovey and other pioneers of the research program are right that emotion-management ability is differentially distributed in the population and that it matters for various performance domains; the question is whether we need to call this ability EI and whether smarts and personality traits alone can explain why some people are better than others at understanding and managing emotions and at reaping the benefits of this ability in consequential settings. To fully answer this question, we need robust testing, using the most rigorous psychometric standards, and to pay attention to the evidence—all of it and particularly that from robust studies. We also need to move away from tests that merely capture people's knowledge of emotions, and develop tests that test the real-world side of "emotional intelligence." It is one thing to measure knowledge of emotional processes or hypothetical intents with respect to these emotional processes as is done with the MSCEIT—it is another thing entirely to enact the correct decision "on the fly." In other words, how individuals respond to items on the MSCEIT test (e.g., using emotions intelligently in decision making) does not necessarily map on how they would act in real-world settings (Fiori & Antonakis, 2012). Nothing in the current EI measures gauges this ability in high fidelity situations.

A final worry is that there is no "a priori" reason to expect that having a highly attuned "emotional radar" can help individuals take appropriate decisions, especially in emotionally-charged situations. I have called this problem the "curse of emotion" (Antonakis, et al., 2009). Briefly, it can become more difficult to make effective decisions as one becomes increasingly sensitive to one's own or others' emotional states. This is because in many circumstances the morally or economically advisable decision may require hurting the feelings of some stakeholders (i.e., "cut the branch to save the tree" type decisions), and increased sensitivity to such feelings can lead to either failure to act properly or massive costs in "emotional labor" for the decider. This suggests that future research on EI should take the decision context into account too, and not simply assume that being "attuned" to the emotions of others is advantageous in all contexts. Also, I have yet to see a test that measures, on one hand the ability to be highly attuned to emotions while, on the other hand "setting emotions aside" when needed in decision making (i.e., not being bogged down by them) or using them in appropriate "doses" in decision making (see Antonakis, et al., 2009).

To conclude, debate in science is healthy and is needed. And, there comes a time when we have to rethink theories or measurement strategies and then "move on." This time is nigh for those doing research in emotional intelligence. My message to them is: "Drop the hype, keep the hope, but pay attention to all the evidence. Not only is it the moral thing to do; it is also the economical thing to do."

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Emotional Intelligence: Some Research Findings and Remaining Questions

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March 2015 – The past quarter century has seen impressive growth of emotional intelligence (EI) as a topic of interest in the fields of psychology and management (Grandey, 2000; Law, Wong, & Song, 2004; Matthews, Zeidner, & Roberts, 2002; Mayer, Roberts, & Barsade, 2008), likely fueled in part by claims that EI predicts job performance better than general intelligence does (Goleman, 1995). Claims regarding the strong relationship between EI and work performance have also stimulated interest among consultants and practitioners, who have made EI a widely used tool for personnel hiring and training (Fineman, 2004).



Despite the impressive commercial success of emotional intelligence (EI), scholars have levied the following criticisms against the construct: (a) EI has definitional ambiguities, (b) there is considerable overlap between EI and related constructs of personality and general mental ability/intelligence (Landy, 2005; Locke, 2005; Murphy, 2006; Zeidner, Matthews, & Roberts, 2004), and (c) many EI measures are proprietary and expensive to administer, which makes it unnecessarily difficult for researchers to build a strong scientific foundation for the EI construct. Below we discuss each of these issues in the hope of clarifying recent progress in EI research, while pointing out a few remaining difficulties associated with the concept.



Definitional Ambiguity: Ability EI vs. Mixed EI

The definitional ambiguity surrounding EI stems from a jingle fallacy (i.e., an assumption that two concepts are the same because they share the same name; Kelley, 1927), wherein the label “emotional intelligence” has been used to refer to two separate constructs: *ability EI* and *mixed EI*. *Ability EI* models propose that EI is a form of intelligence and should therefore overlap with measures of general mental ability/intelligence, whereas *mixed EI* models do not classify EI as a form of intelligence but rather as a composite or *mix* of personality traits and abilities (Petrides & Furnham, 2001).

Ability EI is generally considered to be the “gold standard” model of the construct (Daus & Ashkanasy, 2005; Murphy, 2006) because: (a) it has a relatively clear theoretical definition and (b) it is typically measured with performance-based (i.e., multiple-choice, correct/incorrect) assessments, which are arguably less susceptible to social desirability biases than are self-reports of EI. Recent evidence supports the construct validity (i.e., the extent to which a measure captures its intended construct) of ability EI measures, indicating that ability EI is indeed a facet of intelligence, as the construct label emotional *intelligence* implies (MacCann, Joseph, Newman, & Roberts, 2014; also see Mayer, Caruso, & Salovey, 1999). Specifically, modern conceptualizations of intelligence such as the Cattell-Horn-Carroll theory of mental abilities (McGrew, 2009) suggest that intelligence consists of various specific abilities including verbal ability, quantitative reasoning, visual processing, and retrieval ability.

MacCann et al. (2014) showed that the Mayer-Salovey-Caruso Emotional Intelligence Test (MSCEIT; Mayer, Salovey, Caruso, & Sitarenios, 2003), a popular measure of ability EI, can be represented in the Cattell-Horn-Carroll theory of intelligence as an additional specific ability, which formalizes ability EI as an aspect of intelligence characterized by the ability to process information in the emotion domain. It should be noted, however, that the MSCEIT has been criticized for its potential underrepresentation of the entire domain of EI and for construct-

irrelevant variance. For example, although the MSCEIT was designed to assess all dimensions of Mayer and Salovey's (1997) model of EI, some have noticed that the MSCEIT fails to assess various dimensions of this model such as the ability to express emotion, the ability to perceive emotions in oneself, and the ability regulate emotion in oneself (Maul 2012).

The MSCEIT may also unintentionally assess abilities other than EI (i.e., the MSCEIT exhibits construct-irrelevant variance) such as the ability to match emotions to colors and tastes (i.e., the Sensations measure of the MSCEIT; Maul, 2012). Therefore, although evidence suggests the MSCEIT can be represented in the Cattell-Horn-Carroll theory of intelligence (MacCann et al., 2014), this type of construct validity evidence should be replicated on additional EI measures beyond the MSCEIT.

Ability EI: Does it Predict Job Performance and Other Work Behaviors?

Meta-analytic evidence indicates there is little-to-no incremental validity of ability EI in predicting job performance, above and beyond Big Five personality and general mental ability/intelligence ($\Delta R^2 = .002 = 0.2\%$; Joseph & Newman, 2010; $\Delta R^2 = .004 = 0.4\%$; O'Boyle, Humphrey, Pollack, Hawver, & Story, 2011). In other words, ability EI does almost nothing to predict overall job performance, after one has accounted for personality and general intelligence.

Although the incremental validity of ability EI is slightly larger in jobs that require emotional labor ($\Delta R^2 = .015 = 1.5\%$; Joseph & Newman, 2010; that is, emotional intelligence can predict emotional job performance), and meta-analytic evidence has identified emotion regulation ability as the key driver of the relationship between ability EI and emotional job performance (i.e., Cascading Model of EI; Joseph & Newman, 2010), it appears that empirical evidence does indeed support the long-standing criticism of EI as having questionable incremental validity for predicting overall job performance beyond well-established general intelligence and personality constructs.

It follows that the claims about the predictive power of EI with respect to job performance that are common in the popular press are substantially overstated; we currently have no consistent scientific evidence that scoring high on ability EI tests predicts how strong your job performance will be, beyond smarts and personality traits (in particular, Conscientiousness, Emotional Stability, Agreeableness, and Extraversion).

Despite this criticism, it should be noted that the incremental validity of ability EI for predicting other work-related outcomes, besides job performance, is an open question. As suggested by Landy (2005), ability EI might end up predicting job satisfaction, leader emergence, or the size of one's social network much better than it predicts job performance. We await further research on these topics.

Ability EI: How Many Facets (or Branches) are There?

In addition to the limited incremental validity of ability EI, recent theoretical and measurement-related developments involving ability EI have raised new questions about the facet structure of ability EI. Specifically, although the original four-branch Mayer and Salovey (1997) model of ability EI included four dimensions (i.e., the ability to perceive, understand, use, and regulate emotion), recent work has called for the use of three, rather than four, dimensions of ability EI (i.e., by removing the ability to use emotion facet, or by combining it with the ability to perceive emotion facet; Fan, Jackson, Yang, Tang, & Zhang, 2010; Joseph & Newman, 2010).

Moreover, other work has proposed the addition of a new EI dimension (i.e., the ability to use emotional displays to influence others; Côté & Hideg, 2011), and the specification of sub-facets of existing ability EI dimensions (Côté, 2014). In order to establish a consensus definition of ability EI and its facets, we will need a multidimensional measure of ability EI that consistently supports the theorized facet structure. Our own viewpoint follows from Fan et al.'s (2010) meta-analysis of ability EI dimensions and suggests there is consensus on three of the four facets of the Mayer-Salovey model: ability to perceive, ability to understand, and ability to regulate emotion.

Mixed EI: What is it, and Why Does it Predict Job Performance?

In comparison to ability EI, mixed EI is defined as a constellation of personality traits, emotional skills and abilities, and self-evaluations such as general self-efficacy (Goleman, 1995; Petrides & Furnham, 2001). Whereas the ability EI literature is largely based on Mayer and Salovey's (1997) model, mixed EI has not been based on a single,

consensus model, and as such has been referred to as an umbrella construct that is a “grab bag of everything that is not cognitive ability,” (Joseph & Newman, 2010), and consists of a “list of healthy individual differences” (Côté, 2014, p. 462).

Unfortunately, to this point, the constructs that occupy this list of healthy individual differences have been unclear, leaving us to ask, “Which constructs are mixed EI measures really assessing?”. Research has noted that a majority of mixed EI items can be classified as Big Five personality items (de Raad, 2005), and others have suggested that mixed EI likely borrows content from achievement motivation (i.e., Conscientiousness), impulse control (e.g., Conscientiousness), gregariousness/assertiveness (e.g., Extraversion), and self-evaluations—such as general self-efficacy (Mayer, Roberts, & Barsade, 2008) and self-rated performance (Newman, Joseph, & MacCann, 2010).

Given that a majority of items from mixed EI measures seem to borrow from the content domains of other, well-established constructs, it appears that mixed EI measures might have been developed via a process that we call *heterogeneous domain sampling* (Joseph, Jin, Newman, & O’Boyle, 2015). That is, the developers of mixed EI measures have sampled items from a broad set of well-established content domains, and then simply pooled these items under the umbrella construct label that is mixed EI.

If mixed EI measures do indeed borrow content from a laundry list of other well-known constructs (e.g., neuroticism, extraversion, conscientiousness, self-rated performance, self-efficacy), then one important question is, “How much of mixed EI represents *emotional* skills and abilities?” If the construct of mixed EI has only limited content involving emotional skills and abilities, we question the extent to which the label “mixed *emotional* intelligence” is appropriate.

Interestingly, compared to ability EI, we note that mixed EI appears to display more incremental validity in the prediction of job performance ($\Delta R^2 = .142 = 14\%$; Joseph & Newman, 2010; $\Delta R^2 = .068 = 7\%$; O’Boyle et al., 2011). That is, mixed EI strongly and uniquely predicts job performance. This incremental prediction of job performance by mixed EI is perhaps due to mixed EI measures’ having been developed via domain sampling of self-efficacy and self-rated performance constructs, in addition to Big Five personality constructs and general mental ability. Due to its sizable incremental validity, the construct of mixed EI may be a useful practitioner tool that would benefit from future investigations into its construct validity. [We note that although mixed EI measures are typically self-report personality measures, relatively little of their content directly assesses the degree to which individuals perceive themselves to be emotionally competent.]

Do EI Scores Vary Across Sex? Is There a “Dark Side” of EI?

Well-established stereotypes of women as more emotional than men (Timmers, Fischer, & Manstead, 2003) have captured the attention of EI researchers who wish to understand which sex is more adept at expressing, understanding, and/or regulating emotion. Some evidence suggests biological sex differences in EI (e.g., areas of the brain dedicated to the processing of emotion are larger in females; Baron-Cohen, 2002, 2003) as well as situational/socialization factors that can explain sex differences in EI (e.g., girls are taught to label their emotional feelings more often than boys; Cervantes & Callanan, 1998).

Meta-analytic evidence suggests that sex differences in EI are only found for measures of ability EI (e.g., MSCEIT; Joseph & Newman, 2010), upon which females score notably higher than males ($d = .52$). In contrast, self-report measures of EI exhibit near-zero sex differences (self-report ability EI: $d = .01$; self-report mixed EI: $d = .02$), suggesting that although females may have higher ability EI than males, males and females *perceive* themselves to be equally emotionally competent/skilled. Unfortunately, research in this area has yet to examine the extent to which sex differences in EI might vary across discrete emotions; e.g., to discern the extent to which females are more proficient in perceiving, understanding, and regulating happiness, sadness, anger, etc. The issue of gender differences in EI across specific emotions should be addressed in future research.

Another area of recent interest to EI researchers involves the extent to which emotional skills and abilities can be used to manipulate, deceive, or engage in other socially abusive behavior. Despite the EI construct’s typically being described as a positive trait, preliminary evidence suggests that EI has both a positive side and a “dark side” (i.e., emotion understanding facilitates interpersonally deviant behavior; Côté, DeCelles, McCarthy, Van Kleef, &

Hideg, 2011). A thorough examination of both the positive side and the dark side of EI may help us better understand clinical conditions that are characterized by lack of emotional skills (e.g., autism) or the use of emotional skills for manipulation and personal gain (e.g., psychopathy). This also points to the potential advantages of EI research that borrows theory and findings from the domain of clinical psychology.

When Will Scientists be Able to Measure EI Freely, for Research Purposes?

We appreciate that instruments designed to measure ability EI and mixed EI can constitute intellectual property, and thus charging fees for their administration is a way to recoup the costs of measurement research and development. We also understand the need for test security in the arena of high-stakes testing, where personnel decisions are being made on the basis of test scores. However, when the developers of a proprietary EI instrument insist on charging prohibitively high fees to researchers who simply want to advance scientific knowledge on the topic—after the R&D costs for developing the EI measure have already been recouped—then test developers are condoning the appearance that they do not want scientific research to be conducted on their measures and constructs.

This is our one major gripe. Enough is enough. We think the time has come for the proponents of the dominant measures and models of EI to make their instruments (or some versions of their instruments) either freely available, or available at only nominal cost, to researchers seeking to advance EI science. As it currently stands, it is quite difficult for an interested graduate student to conduct a simple study of EI without first paying prohibitively large test administration fees (e.g., an EI study with $N = 200$ participants would cost well over \$1,000 in test administration fees). Most mainstream psychological constructs can be studied for free—EI is an exception.

Summary

In sum, existing measures of *ability EI* have been demonstrated to represent the expression of intelligence in the emotion domain, but to also suffer from a lack of incremental validity for predicting job performance. In contrast, *mixed EI* appears to have strong incremental validity for predicting job performance, but suffers from construct validity problems including unknown content (i.e., the content appears to be recycled from other well-established constructs in psychology), questions involving the appropriateness of the label “emotional” intelligence, and lack of a clear theoretical explanation for why mixed EI predicts job performance so well. These and other questions could be better answered if test publishers would make at least some versions of their EI instruments less cost-prohibitive to researchers.

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On the Efficiency of Emotional Intelligence Training in Adulthood

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A Brief Primer on Measures of Emotional Intelligence

March 2015 – Although we all experience emotions, we markedly differ in the extent to which we identify, express, understand, regulate and use our own and others' emotions. The concept of emotional intelligence (EI) has been proposed to account for this idea (Mayer & Salovey, 1997). Past debates on the status of EI have given birth to a tripartite model of EI (Mikolajczak, Petrides, Coumans, & Luminet, 2009).

Briefly, this model posits three levels of EI: knowledge, abilities and traits. The knowledge level refers to what people know about emotions and emotionally intelligent behaviors (e.g. *Do I know* which emotional expressions are constructive in a given social situation?). The ability level refers to the ability to apply this knowledge in a real-world situation (e.g., *Am I able to* express my emotions constructively in a given social situation?). The focus here is not on what people know but on what they can do: Even though many people know that they should not shout when angry, many are simply unable to contain themselves. The trait level refers to emotion-related dispositions, namely, the propensity to behave in a certain way in emotional situations (*Do I typically* express my emotions in a constructive manner in social situations?). The focus here is not on what people know or on what they are able to do, but on what they typically do over extensive periods of time in social situations. For instance, some individuals might be able to express their emotion constructively if explicitly asked to do so (so they have the ability), but they do not manage to manifest this ability reliably and spontaneously over time. These three levels of EI are loosely connected – declarative knowledge does not always translate into ability, which, in turn, does not always translate into usual behavior – and should therefore be assessed using different instruments.

Knowledge and abilities are essentially assessed using intelligence-like tests such as the MSCEIT (Mayer Salovey Caruso Emotional Intelligence Test; Mayer, Salovey & Caruso, 2002), the STEU (Situational Test of Emotional Understanding ; MacCann & Roberts, 2008) or the GERT (Geneva Emotion Recognition Test, Schlegel, Grandjean & Scherer, 2013), while usual emotional behavior is assessed using personality-like questionnaires such as the TEIQU (Trait Emotional Intelligence Questionnaire; Petrides, 2009), the EQ-I (Emotional Quotient Inventory (Bar-On, 2004) or, more recently, the PEC ([Profile of Emotional Competence](#) ; Brasseur, Grégoire, Bourdu & Mikolajczak, 2013).

The Importance of Trait EI

The literature indicates that the trait level of EI, on which we will focus in this paper, has a significant impact on four of the most important domains of life: well-being, health, relationships and work performance. We remain neutral here on whether measures of EI which are not trait-based have a comparable impact in any of these domains.

For instance, people with greater *trait EI* have enhanced well-being and mental health (for a recent meta-analysis, see Martins, Ramalho & Morin, 2010). They also have better physical health, as evidenced in a recent nationally representative study conducted by our team in collaboration with the largest mutual benefit society in Belgium (Mikolajczak et al., [in press](#)). Socially speaking, they seem to have more satisfying social and marital relationships



(e.g. Schutte et al., 2001; see Malouff, Schutte, & Thorsteinsson, 2014 for a meta-analysis). Finally, a recent meta-analysis (O'Boyle et al., 2011) shed light on the debate on EI and job performance: although not all studies found a significant relationship, the aggregate effect confirms that people with higher *trait EI* do achieve superior job performance.

It is noteworthy that despite early fears that trait EI would not predict additional variance in the above-mentioned outcomes beyond the big five personality factors and intelligence, the vast majority of studies actually refute this fear (see Andrei, Siegling, Baldaro & Petrides, under review, and O'Boyle et al., 2011 for meta-analyses confirming the incremental validity of trait EI regarding health and job performance, respectively).

Improving Trait EI: Data and Recommendations

Because of its importance for people's well-being and success, researchers and practitioners alike have wondered what, if anything, can be done to improve *trait EI* in adults. The question is not trivial as traits are harder to change than knowledge or abilities, especially in adulthood. However, the fact that personality traits can change in response to life experiences shows that traits are somewhat malleable (Roberts, Caspi & Moffitt, 2003; Roberts & Mroczek, 2008). The current note examines the possibility of improving *trait EI* in adults. It provides brief answers to the following four questions: (1) Is it still possible to improve trait EI in adulthood? (2) How? (3) What are the benefits—in terms of well-being, health, social relationships and work success—of such EI improvement and do such benefits last? (4) Will trait EI training work for everyone?

To What Extent Can Trait EI Be Improved In Adulthood?

This question has given rise to a number of studies, mainly in the fields of psychology, management, medicine and education. As these words are being written, 46 studies have been conducted to check whether trait EI scores improve after EI training (for review, see Kotsou, Mikolajczak, Grégoire, Heeren & Leys, in preparation). 90% of them conclude in the affirmative. However, a closer look at the studies reveals that most of them were published in low-impact factor journals (median Impact Factor < 1), which is not surprising as the vast majority of them suffer from crucial limitations, the most important being that 46% of the studies do not include a control group. Among the studies that did, only 36% assigned participants to groups randomly and only 8% of the studies (i.e., 2 out of 46) included an active control group (i.e., a training group with the same format but another content; the only way of excluding the fact that improvements are due merely to the group effect). Moreover, 63% of the studies measured training effects immediately after the training, with no follow-up to assess the sustainability of the changes.

Finally, 75% of the studies did not use a theory- and/or evidence-based training. A theory-based training is a training that is designed according to a theoretical model of emotional intelligence (e.g., if the model comprises 5 dimensions, the training should cover all of them; if the model assumes a hierarchical order in the EI dimensions, the training should be built accordingly). An evidence-based training is a training in which the individual strategies taught to participants to improve EI (e.g., strategies to express their emotions in a constructive manner, strategies to regulate their emotions, etc) have been previously shown to be effective in well-designed scientific research.

That being said, a few studies did not suffer from the above-mentioned limitations (i.e. Karahan & Yalcin, 2009; Kotsou et al., 2011; Nelis et al. 2009, Nelis et al. 2011; Sharif & al., 2013; Slaski & Cartwright, 2003; Vesely, Saklofske & Nordstokke, 2014; Yalcin, Karahan, Ozcelik, & Igde, 2008). And they suggest that it is possible to improve trait EI in adults. The mean improvement of EI in these studies, as measured by the TEIQue or the EQ-I, was 12.4%. This increase was confirmed, although to a much lesser extent (+6.6%), by spouses and friends.

How can trait EI be improved in adults?

A look on the content of effective EI trainings suggests that improving trait EI requires working on at least two of the following EI dimensions: identification of emotions, expression of emotions and regulation of emotions. Among the well-designed studies cited above, the majority focused on five dimensions: identification, understanding, use, expression and regulation of emotions (see Table 1 for an explanation of these dimensions). We reproduce below the content of one of those trainings (Kotsou et al., 2011).

Like in most EI trainings, the pedagogical technique combined theoretical instructions with behavioral and experiential teaching methods (e.g., group discussions, role play, self-observation, etc. The full materials are avail-

able in French on request to the first author). We won't be able to get into the details of how these forms of training work (please contact first author for further details), but to get a flavor of our techniques we will say a few words about the sort of role play we do in the "expression/listening" module.

In this role playing game the trainer invites one participant (A) to play the role of a person who has just been left by his partner. Another participant (B) is invited to take the role of the friend that A has called for support. A is usually very emotional when explaining the situation to B. The group observes how the B deals with the situation. After a few minutes, the trainer interrupts the session and asks A how s/he feels. Usually, s/he still feels very emotional. The trainer then invites another participant to play the friend. The scene is repeated until someone finds a

way to make A feel better. The trainer then asks the group why the first interventions did not work, and why they think that the last did. This then allows the trainer to explain the most common ways people react when someone shares an emotional event with them (asking questions about the facts, offering solutions, minimizing, judging, reappraising...) and discuss why these ways of listening do not work well, even if most of them are well intentioned. He then explains why the last solution (reflecting feelings) works better, and when reappraising and offering solutions can have positive effects (i.e., only after the negative emotion has significantly decreased).

Which Benefits Can Be Expected From Trait EI Improvement?

The first benefit is enhanced psychological well-being: EI training leads to both a drop in psychological symptoms (stress, anxiety, burnout, distress, etc.; Karahan & Yalcin, 2011; Kotsou et al., 2011; Nelis et al., 2011; Sharif et al., 2013; Slaski & Cartwright 2003; Vesely et al., 2014), and an increase in happiness (well-being, life satisfaction, quality of life etc.; Kotsou et al., 2011; Nelis et al., 2011; Vesely et al., 2014; Yalcin et al., 2008). Unsurprisingly, these changes translate into significant changes on personality traits of neuroticism and extraversion after a few months (Nelis et al., 2011).

The second benefit of EI training is an improvement in self-reported physical health (Kotsou et al., 2011; Nelis et al., 2011; Yalcin et al., 2008). It is noteworthy that this improvement in physical health is also evidenced in biological changes such as a 14% drop in diurnal cortisol secretion in Kotsou et al. (2011)'s study and a 9.7% drop in glycated hemoglobin in Karahan & Yalcin's (2009).

The third benefit is improved quality of social and marital relationships (Kotsou et al., 2011; Nelis et al., 2011) with, this time, a remarkable agreement between participants and friend/spouse reports. The fourth benefit concerns work adjustment. EI training increases people's attractiveness for employers (see Nelis et al., 2011, study 2 for the detailed procedure) and work-related quality of life (Slaski & Cartwright, 2003). It is however unclear whether an improvement in EI increases work performance. Although several studies report an effect of EI training on work performance, the only well-conducted study that measured work performance did not observe any effect (Slaski & Cartwright, 2003).

One may wonder whether the foregoing changes generated by EI training last. From the well-conducted EI training studies that included a follow-up, it can be concluded that the changes in trait EI and its correlates are evident after a few weeks and are maintained for at least 1 year (see Kotsou et al., 2011). No study so far has tested whether

<p>Introduction</p> <ul style="list-style-type: none"> •Presentation of the trainers and the training •Presentations of the participants (expectations,...) and warm up •Rules of the group (respect, confidentiality,...) •Introduction to the functions and biological correlates of emotions
<p>Identification</p> <ul style="list-style-type: none"> •Learn to welcome rather than suppress/repress emotions •Learn to identify emotions and distinguish their various components (thoughts, bodily sensations, action tendencies, subjective feeling) •Learn how to identify others' emotions
<p>Understanding</p> <ul style="list-style-type: none"> •Understand the causes of one's emotions (e.g., distinguish between the trigger and the profound cause; identify the unsatisfied needs which are potentially behind the emotion)
<p>Utilisation</p> <ul style="list-style-type: none"> •Learn how to use emotions as indicators of one's needs' level of satisfaction, as prompts to better know oneself and to take better care of oneself
<p>Expression/listening</p> <ul style="list-style-type: none"> •Learn to express one's emotions (or needs) in a socially acceptable and functional manner (when? How?) •Learn how to deal with others' emotions
<p>Regulation</p> <ul style="list-style-type: none"> •Learn when an emotion should be regulated •Learn how to down-regulate a displeasing emotion (or a stress) when necessary •Learn how to up-regulate pleasant emotions (i.e., increase the frequency, intensity and duration of pleasant emotions)

Table 1. Structure of the EI training used in Kotsou et al. 2011

these changes last more than 1 year. Studies with longer follow-up periods are however needed to ascertain that benefits in terms of well-being are resistant to hedonic treadmill effects, namely the tendency of most humans to return to their baseline level of happiness after major positive or negative events or life changes.

Does Trait EI Training Work For Everyone?

From the many studies conducted in our lab (most of them as yet unpublished master's theses), we have observed that EI training seem to be effective for both women and men, both younger and older adults, both normally gifted as well as exceptionally gifted people, for both sub-clinical and non-clinical samples and for both psychosomatic as well as condition-free patients. In all these groups, people with the highest motivation to follow the training derived the largest the benefits of it. By contrast, standard EI training is not effective for severely depressed in-patients or for unqualified workers from very low socio-economic backgrounds. In these two groups, people did not really adhere to the training (depressed people did not believe that it would help them get better; unqualified workers did not understand the usefulness of understanding and/or regulating emotions). Futures studies will have to determine if new versions of EI training could work better.

In conclusion, the current state of the literature suggests that trait EI can be improved in adults and that EI training is effective to that end. Future research is needed to determine the best methods to maximize the size and the duration of the effects, and to determine for whom it works the best/the least.

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Q & A On Emotional Intelligence With John (Jack) D. Mayer

John (Jack) D. Mayer received his Ph.D. and M.A. in psychology at Case Western Reserve University, and his B.A. from the University of Michigan and was a postdoctoral scholar at Stanford University. He is presently [Professor of Psychology at the University of New Hampshire](#). Dr. Mayer has served on the editorial boards of *Psychological Bulletin*, the *Journal of Personality and Social Psychology*, and the *Journal of Personality*, among others, and has been an Individual National Institute of Mental Health Postdoctoral Scholar at Stanford University. He has published over 120 articles, books, and psychological tests related to personality psychology, including on emotional intelligence, integrative models of personality, and more generally on the effects of personality on an individual's life. He is widely acknowledged as the father of the science of emotional intelligence (together with Peter Salovey).



Andrea Scarantino (AS): Let me first thank you for agreeing to participate in the first “Topical Q&A” published in the Emotion Researcher. My job in this Q&A will be to raise some questions about your highly influential work on Emotional Intelligence (EI) that have emerged from the critical pieces published in this issue of ER, giving you an opportunity to respond. I will mainly focus on the two pieces by Antonakis and by Joseph and Newman, and occasionally raise some issues of my own. How does that sound?

John D Mayer (JDM): That sounds fine, Andrea. It's a pleasure to take part in this Emotion Researcher Q & A.

AS: Great, let us get started then. What is your understanding of intelligence, the genus of which emotional intelligence is a species? And how many types of intelligence do you think we have scientific evidence for?

JDM: Well, I personally agree with the many intelligence researchers who regard intelligence as the capacity to carry out abstract reasoning: for example, to understand meanings, to grasp the similarities and differences between two concepts, to formulate powerful generalizations, and to understand when generalizations may not work because of contexts. There are many alternative definitions, including that intelligence is a system of mental abilities—with which I also agree—that it is an index of neurological integrity, and that it reflects the capacity to learn, or is in fact the ability to adapt. (The “abstract reasoning” definition and many of these others appear in the 1921 symposium of experts on intelligence and likely date back earlier; see [Anonymous 1921](#)).

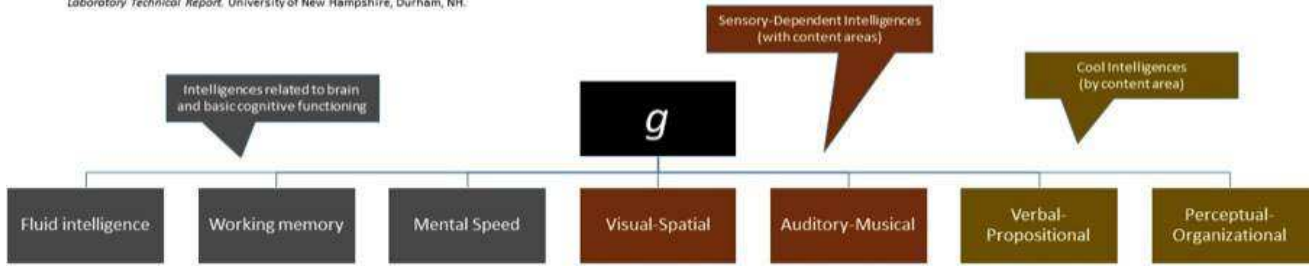
Regarding the types of intelligence, I am content for the time being with what the intelligence researcher Kevin S. McGrew has labeled the Cattell-Horn-Carroll (CHC) model of intelligence (see [McGrew, 2009](#)). The CHC is a three-stratum model in which *general intelligence* or *g*—the capacity to reason abstractly—is at the top. Beneath it are a number of *broad intelligences* of a rather bewildering variety—including fluid reasoning, comprehension-knowledge (similar to verbal intelligence), visual-spatial processing, working memory, long-term storage and retrieval, and speed of retrieval. And at the bottom level are more specific *mental abilities*. For example, comprehension-knowledge includes the specific ability to understand vocabulary.

One reservation I have about this model concerns the exact nature of the broad intelligences and exactly what those intelligences are. The intelligences at this second level are all broad abilities that correlate with one another and with *g*. That said, they are quite diverse and seem to form conceptual subcategories—but what those subcategories are has not been settled. This concern over second level broad intelligences is shared by others (I particularly like the article by Schneider and Newman, 2015, in this regard).

To clarify the situation, I regard these broad intelligences as forming several groups, with *g* at the very top. If I sketched the top two levels of the intelligence hierarchy, they would look something like Figure 1:

Figure 1. One Possible Grouping of Representative Broad Intelligences in the Cattell-Horn-Carroll (Three-Stratum) Model

Adapted from Mayer (2015). What is the relationship between personal and emotional intelligences. Laboratory Technical Report. University of New Hampshire, Durham, NH.



Here, I have chosen to distinguish broad intelligences into three groups as a start. The first group concerns basic neuro-cognitive processing and includes abstract at-the-moment reasoning (fluid intelligence); it includes working memory, which involves the ability to retain and manipulate items in our attention as when we generate and recall a password; and it includes speed of mental processing. A second group is constituted by broad intelligences that are based on a sensory modality. Depicted here are visuo-spatial abilities and auditory-musical abilities.

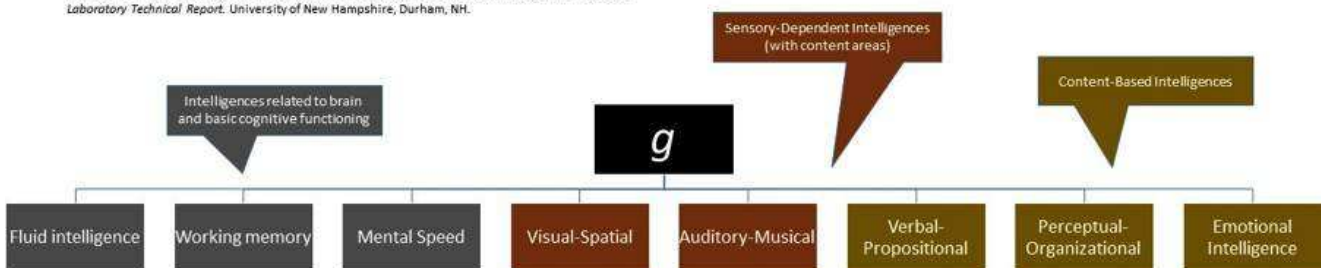
Also quite important are content-focused intelligences including, in this abridged diagram, verbal-propositional and perceptual organizational intelligences. Verbal-propositional intelligence concerns understanding the meanings of words and sentences. Perceptual-organizational intelligence concerns putting together designs and puzzles—sometimes it is called mechanical ability.

AS: And how would emotional intelligence fit into this hierarchy? I am guessing it does somewhere?

JDM: Well, on first reflection it certainly seems to be a broad intelligence, so it could be added next to the verbal-propositional and the perceptual-organizational intelligences. This would yield the diagram in Figure 2.

Figure 2. Another Possible Grouping of Representative Broad Intelligences in the Cattell-Horn-Carroll (Three-Stratum) Model

Adapted from Mayer (2015). What is the relationship between personal and emotional intelligences. Laboratory Technical Report. University of New Hampshire, Durham, NH.



I think, however, that it might make sense to add a new category of broad intelligences devoted to hot intelligences as compared to cool intelligences. Cool intelligences are those that deal with relatively impersonal knowledge such as verbal-propositional intelligence, math abilities, and visuo-spatial intelligence. Hot intelligences, by comparison, concern matters that affect our interests: they may warm our hearts or make our blood boil. Emotional intelligence appears to be of that hotter class, along with social intelligence, and a newly-proposed member of the group that I have called personal intelligence.

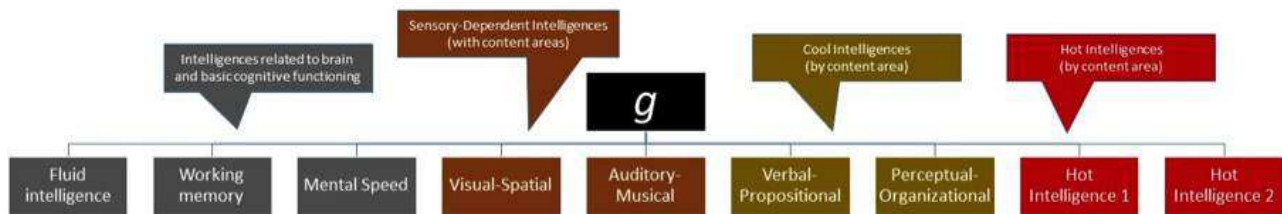
Personal intelligence involves reasoning not just about emotions, but also about a person's motives, plans, traits, self and identity, and goals—about personalities—both one's own and the personalities of other people. I reserve

the term social intelligence for reasoning that is centrally concerned with relationships and larger group dynamics.

In Figure 3 I have revised the CHC model to include hot intelligences.

Figure 3. One Possible Grouping of Representative Broad Intelligences in the Cattell-Horn-Carroll (Three-Stratum) Model—with Hot Intelligences

Adapted from Mayer (2015). What is the relationship between personal and emotional intelligences. Laboratory Technical Report. University of New Hampshire, Durham, NH.



I haven't yet filled in the specific hot intelligences, but my guess is that the most likely division would be between personal and social intelligences with emotional intelligence nested within personal intelligence. Other arrangements also are possible: emotional intelligence may also stand apart from personal intelligence. Sorting out these relations in detail is a job for another day. I note in conclusion that the CHC model, augmented to include hot intelligences, broadens our perspective on emotional intelligence and locates it in a bigger picture of how the mental abilities function as a group within personality.

AS: Before we move on to that bigger picture, I was wondering what led you and Peter Salovey to introduce in 1990 the concept of emotional intelligence, which you defined back then as “the subset of social intelligence that involves the ability to monitor one’s own and others’ feelings and emotions, to discriminate among them and to use this information to guide one’s thinking and actions”?

JDM: At the time, Peter and I both were working in the area of cognition and affect—studying the way that emotion influences cognition and vice versa. Peter and I also had interests in clinical psychology and an interest in how people function healthily.

I trained in the doctoral program in clinical psychology at Case Western Reserve University where I was very interested in emotion and emotional people and how they fared in life. As I became increasingly interested in research, Doug Detterman became a key mentor of mine and I apprenticed in his intelligence laboratory and I later switched full time into research. That gave me several years to think about intelligence and its relation to emotion.

When I moved to Stanford University in the early 1980's, Professor Noel Sharkey invited me to write a chapter for his edited book, *Advances in Cognitive Science* (Chichester: Ellis Horwood). I chose to write on why computers—expert systems, actually—might benefit from experiencing emotions. A few years later these ideas matured in a set of conversations I had with Peter Salovey and the result was our 1990 article “[Emotional Intelligence](#).”

AS: You have characterized emotional intelligence as an *ability*, but there are alternative definitions that identify EI with a *character trait* or with a *mix* of abilities and traits. Since several other contributors mention these alternative models, it would be helpful to know your views on them, especially if you have some major reservations.

JDM: If you want to hypothesize and measure a new part of personality, it helps to develop a clearly focused idea of what you are speaking of, and to measure it properly. I believe emotional intelligence, conceived of as an ability, is a such a clear and focused idea.

Many alternative models of emotional intelligence are well-described as “mixed” because they draw together a diverse group of personality qualities: socio-affective traits such as optimism and persistence, motivational traits such as the need for achievement and traits of self-control such as persistence. The breadth of these alternative

models led me to believe that they would correlate highly with many existing and well-conceptualized measures of personality and therefore fail to add to what psychologists already study. Two recent articles indicate that these alternative EI notions correlate between $r = .69$ and $.72$ with a “general personality factor” (GPF)—that is, the self-report of positive traits on the Big Five, a widely used set of traits that includes extraversion, neuroticism, agreeableness, openness and conscientiousness (Pérez-González & Sanchez-Ruiz, 2014; Van der Linden, Tsaousis & Petrides, 2012). These mixed-model scales of EI, in other words, are primarily duplicative of Big Five content and in my opinion there is little rationale for labeling them emotional intelligence.

This being said, I do believe it makes sense to speak of self-estimated emotional intelligence as some of these alternative models do, so long as that estimate is anchored to the ability model of emotional intelligence to keep it focused. That self-estimated trait is useful for examining discrepancies between actual (ability-based) emotional intelligence and what people believe about themselves. If you compare actual emotional intelligence with self-estimates, you find that people have only a hint of their actual emotional intelligence, with the correlation between ability and self-estimates about $r = .19$ —and that is interesting (see [Brackett et al., 2006](#)).

AS: You are the author, jointly with Peter Salovey and David Caruso, of the most well respected and widely used measure of EI, the MSCEIT (see article by Ashkanasy and Dasborough in this issue for a description of the measure and of its alternatives). I would like to organize my questions around three aspects of the MSCEIT:

- Construct validity: Does the MSCEIT capture the latent ability it is intended to capture?
- Incremental validity: Does the MSCEIT add to the predictive value of general intelligence and personality measures with respect to various phenomena of interest?
- Commercialization and popularization: Are the commercial interests at stake in EI testing in danger of interfering with the scientific assessment of EI? Have popularizers of EI gone too far in their unfettered enthusiasm for EI?

Generally speaking, abilities tests (e.g. IQ tests) aim to present subjects with a battery of tasks designed to elicit responses that will reveal a latent and not directly observable ability. In the case of MSCEIT, the latent ability being measured is that of monitoring and discriminating between emotions, and using the information so obtained “to guide one’s thinking and actions”. One issue raised by Antonakis and several other critics is that the MSCEIT tests for emotional knowledge but fails to capture how such knowledge translates into action. For instance, one may know what mood might be helpful to feel when meeting in-laws for the very first time, to cite one of the 141 questions contained in the MSCEIT, but be unable to feel that very mood when actually meeting the in-laws. Why should we think that theoretical knowledge about emotions translates into emotionally intelligent action in real-world circumstances?

JDM: Intelligence tests measure the ability to reason, but not necessarily whether people use their intelligence effectively in social situations. We probably all know people who achieve very high scores on general intelligence tests and their proxies (e.g., SATs) but who may decide they are not interested in an intellectual life. IQ tests tell us what people *can* do but not what they like to do or decide to do. So I believe Antonakis’ and others’ criticism applies equally to all intelligence tests. These are *mental* tests: They assess inner properties of the individual and not necessarily what the person expresses in a social context. Measures of social expression are, I think, best conceptualized as distinct from personality and its underlying mental abilities, though our personal abilities and personality strongly determine how we act. (For more on distinguishing between personality and its expression see my article “[The Personality Systems Framework: Current Theory and Development](#)” in the *Journal of Research in Personality*).

Our emotional reasoning is just that: reasoning. By comparison our emotional behavior is a product of myriad influences from our past behavioral conditioning, how emotionally reactive we are, and contextual factors including who we are interacting with. We would expect to see our emotional intelligence correlate with good emotional behavior and our well-being over time and situations—which it does. Any of us, however, may exhibit emotional failures in behavior at one or another time.

It’s possible that emotional intelligence is special case in the sense that, whereas understanding vocabulary and expressing it aren’t so different, knowing what’s best to do emotionally is one thing and doing it exactly as we

would like may require considerable extra talent. For instance, even if we know full well that anger and sarcasm would hurt others at a given moment, we may be unable to refrain from expressing them. I regard the core of emotional intelligence as what we know is best to do. Its social expression involves other complementary skills including good self-management, fostered by personal intelligence, as well as understanding social provocation and resisting it, fostered by social intelligence.

People with high emotional intelligence are better liked and have better relationships than others. So I think of emotional intelligence as the mental ability that predicts good emotional behavior much of the time for many people. (For more on what emotional intelligence predicts, see (Mayer, Richards & Barsade, 2008, in the *Annual Review of Psychology*). But it does not predict good emotional behavior all the time for everyone.

AS: This clarification is helpful, but it appears to be somewhat in tension with the original definition of EI, which, as mentioned before, included the ability to “use...information [about emotions] to guide one’s thinking and *actions*”. Your last reply suggests instead that intelligent guidance of actions, although perhaps correlated with emotional intelligence, is not part of it. I noticed also that in later definitions of the concept of EI direct mention of action drops out entirely. For instance, in a 2008 paper we read that “emotional intelligence (EI) involves the ability to carry out accurate reasoning about emotions and the ability to use emotions and emotional knowledge to enhance thought” (Mayer, Richards & Barsade 2008), without any direct reference to enhancing the quality of the resulting actions. So I suppose at this point your view is that if we want to capture what we may commonsensically call *emotionally intelligent behavior* we need a measure different from, and additional to, EI. Is that right? If so, how should we go about coming up with a measure of one’s ability to carry out emotionally intelligent behavior? What practical tests could we rely on (e.g. role playing exercises, behavioral observations in emotionally charged settings, etc.)?

JDM: Yes, my thinking has shifted somewhat in regard to the place of action. Emotional intelligence concerns reasoning well about emotions, their meanings and their expressions. When a person actually guides her behavior, however, she needs to take into account far more than emotions alone and that requires a broader intelligence and other personality factors.

As soon as we begin to think about “what’s best” for us and others all things considered we draw on our personal intelligence. We use personal intelligence to guide ourselves and we do better if we keep in mind not only our emotions, but also our motives, our preferred ways of behaving, our capabilities and our goals—as well as the personalities and goals of others. When we make choices in a complex social world, in other words, there are far more capabilities involved than understanding emotion alone.

For example, let’s say a father of a high school senior watches his son open an acceptance letter to a college. The parent may feel happy and relieved for his son at his admission to college, but the father may also feel sadness at the thought of his son leaving home. There is no way to manage that feeling well without an appreciation of many factors that go beyond emotion alone. The father may elect to put the sadness aside for the moment to allow his son to experience an undiminished sense of achievement, pride and relief for the day. The father has a broad personal goal to protect his son’s moment of excitement; he further understands (back to emotional intelligence) that he can share his sadness with his son over leaving home at a later time in a loving way.

In our MSCEIT test we are very careful to explicitly state the goal of each instance of emotion management we inquire about so as not to tap into too much personal intelligence (which would reduce the validity of the test at assessing *emotional* intelligence specifically). In everyday life, however, we need personal intelligence—a sensitivity and understanding of our own and others’ goals, to balance our own needs with those of other people. (I describe

this balance throughout the book *Personal Intelligence*).

Regarding the assessment of emotional expression, I believe that skills at expressing and controlling emotion are very difficult, but not impossible, to measure. Creating a role-playing exercise that makes people so mad they have trouble controlling themselves creates ethical issues, but sometimes such research is possible. In certain studies, for example, marital researchers ask couples to engage in common arguments they have while being observed in the laboratory. Assessing the behavior requires in-person testing and evaluation—and that is expensive. The costs run higher as journals demand larger samples than they have in the past. Perhaps in the future computer expert systems might automate these observations of people's emotions. For the time being, however, there are many practical challenges facing anyone who is interested in developing measures of emotionally intelligent behavior.

AS: Finally, could knowledge of one's own and other people's emotions actually interfere with good decision-making in practical settings by making one "too sensitive", as Antonakis suggests in his "curse of emotions" hypothesis? In other words, can emotion knowledge actually stand in the way of emotionally intelligent behavior?

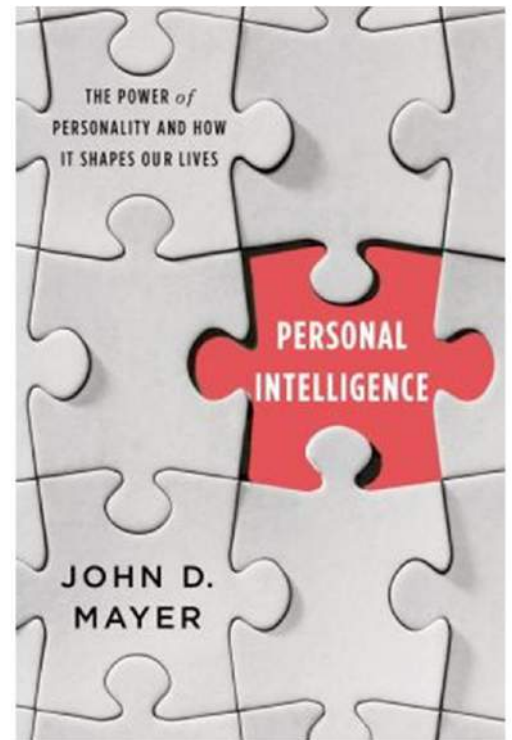
JDM: I agree there is a lot to our functioning beyond emotional intelligence. All things being equal, higher emotional intelligence is an advantage. But if someone is "too sensitive"—high in neuroticism and rejection sensitivity, for example—that individual will carry around a "curse of emotions." People with high levels of empathy may feel other's pain, and in some contexts that can be a drawback—for example, in a teacher who empathizes so much with each pupil that she may find it hard to enforce her classroom's rules.

AS: An important feature of IQ testing is that it is generally clear what the correct answer to a particular cognitive task is (e.g. a mathematical task). Several critics have suggested that this is not the case for the tasks comprised in MSCEIT. Consider for instance the case of Debbie, who just came back from vacation feeling peaceful and content. Another one of the questions in the MSCEIT asks how well various actions preserve Debbie's mood, with choices ranging from making a to-do list, thinking about her next vacation and ignoring her feeling of contentedness. In this case, it is far from clear what the correct answer is. The unspecified context seems to play a key role in determining which action would be most mood-preserving and different respondents are likely to fill in the details of the unspecified context in different ways (What kind of personality do they imagine Debbie to have? When and where are they assuming Debbie's next vacation to be?). Aren't you worried that there isn't enough contextual information in the test questions, and that the answers given will be confounded by cultural biases and personal idiosyncrasies of the respondent, rather than revealing a latent and universal human ability? More generally, how can we capture the essential context-dependence of what emotional intelligence demands in complicated real-life circumstances with a battery of simple questions?

JDM: Regarding the "Debbie" item, by leaving her personality and context unspecified, we are asking people to answer the question by imagining someone of average personality in a general context.

The more we indicate specific personalities in items such as these, the greater the likelihood we measure personal intelligence rather than emotional intelligence. Remember that in our focal model, emotional intelligence involves perceiving emotions, using them to enhance thought, understanding them and managing emotions. Although no intelligence can be measured with absolute purity, we want to keep reasoning about personality and social situations to a minimum so as concentrate on emotional reasoning. Our measure requires some verbal intelligence to understand the questions and answer them and probably some personal intelligence as well. That said, we aim to keep emotional reasoning at the center, and I believe we have mostly succeeded.

Questions that veer into reasoning about motives, inner states, goals, plans, traits, and self-awareness are solidly in the territory of measuring personal intelligence. My colleagues David Caruso, Abigail Panter and I are now



using the Test of Personality Intelligence (TOPI) to cover that ground (Mayer, Panter & Caruso, 2012).

I also should note regarding the Debbie question, that each test item goes through a multistage checking process. Elsewhere, [Haladayna and colleagues \(2002\)](#) have set down a helpful group of rules and concerns for writing test items. Beyond checking that the items conform to such guidelines, experts can have a difficult time spotting which items are best. One reason for this is because items may draw on multiple abilities—and are, as a consequence, read and solved in different ways by participants (see [Ackerman, 1992](#)). During our test-development phase, therefore, we study which items perform satisfactorily and drop those items that fail to perform. Although problematic items can and do slip through, the validity of the test indicates that the larger number of items work adequately.

AS: A related issue concerns the scoring system used to assess how emotionally intelligent respondents are. The MSCEIT uses consensus scoring (the more an answer matches the consensus, the higher its score) and expert scoring (the more an answer matches the experts, the higher its score). The use of consensus scoring is not unheard of in abilities testing, but it is generally reserved for tasks in which the consensus is indeed constitutive of correctness (e.g. consensus on the grammaticality of a sentence by a representative sample of native English speakers makes the sentence grammatical). What is the rationale for thinking that consensus indicates the correct answer in the realm of emotional intelligence? For instance, couldn't ordinary people simply be wrong about what moods are helpful when meeting the in-laws for the first time, or about which specific mental practices are mood-preserving? Are we not simply tapping into 'old wives' tales' about how emotions and moods work when we rely on respondents' consensus?

JDM: Tests of verbal intelligence assesses people's ability to recognize vocabulary words and to understand the meanings conveyed by brief passages. Those tasks evaluate the respondents' answers against a consensus among experts as to what a word or brief literary passage conveys to an astute reader. Like English and other languages, emotions are a communication system and rely on people's shared understanding of the meanings they convey. Not everyone can grasp the communicated consensual meaning, and those who fail to do so are considered low in communication ability in the given realm, be it our verbal language or emotional language. That is why consensus is a pretty good scoring method.

Other experts believe that the MSCEIT can be improved by using what Roberts and colleagues have called veridical scoring. We rely wholly on veridical scoring in the newly-published MSCEIT Youth Research Version (MSCEIT-YRV) —a test normed on youth between the ages of 10 to 18 years. To score the YRV, we first examined scientific articles that spelled out the rules and meanings of emotions based on laboratory and survey work, and provided it as a reference source to a group of emotion experts (doctoral-level and Ph.D. psychologists with specialty interest and/or training in the area). Next, we asked the experts to refer to the research and to identify the correct answers to the test. Our expert panel then identified correct and incorrect answers for all the possible items. Items for which they could not agree were removed from the test. This procedure is very similar if not identical to that used on standard intelligence tests such as the WAIS: Subject-matter experts evaluate each question for the correct answer and only those items that experts agree on are retained for the test. Going forward we expect to continue the transition to veridical scoring in the next version of the MSCEIT.

AS: Another issue raised by Antonakis concerns the extremely high correlation between consensus scoring and expert scoring: "commoners" and "experts" seem to agree on the "correct" answer in the great majority of cases. The experts were 21 emotion scholars attending ISRE 2000 in Quebec. Why do you think that ISRE membership makes one an expert in emotional intelligence? Only half-jokingly, aren't academics in general a peculiar bunch whose counsel in matters of emotional intelligence we should not especially seek? And isn't the very fact that their answers largely overlap with the answers of regular folk a sign that they may be experts on, say, the amygdala, or Cartesian emotions, but not on how to use emotional information to guide one's thinking?

JDM: Academics are surely more highly intellectual than average. The psychoanalytic tradition argues that intellectualization is sometimes a defense against feeling. That acknowledged, I suspect that academics who study emotion are on the whole above average in their emotional intelligence—although a few individuals among us are no doubt impaired in these areas.

An often-overlooked finding in our study of expert and everyday respondents on the MSCEIT is that the experts

had a higher consensus among themselves than regular folk (Mayer, Salovey, Caruso & Siterenios, 2003). This finding is consistent with the idea that in many instances—such as understanding vocabulary and grammar—being an expert involves a heightened understanding of what the consensus is, along with other more rarified knowledge.

If we were developing an assessment of physics we'd poll physicists. In this case, the subject is emotions and my guess is that ISRE members are more knowledgeable about emotions than others. As noted, we have now moved to veridical scoring (asking experts to identify correct and incorrect answers) on the MSCEIT-YRV, and we will do the same for the next-generation MSCEIT.

AS: Additionally, in his article Antonakis argues that a by-product of using consensus scoring is that truly gifted individuals (call them emotional geniuses), who are by definition way above average, will score poorly. How do you answer these worries, which do not seem to apply equally to the case of IQ testing?

JDM: Yes, the consensus can be wrong at times—and for that reason we take seriously those commentators such as Antonakis (and, elsewhere, Roberts, Zeidner and others) who argue that test does less well at identifying truly gifted individuals in this area. We agree the test could be improved by adding more difficult items to help identify gifted individuals and hope to improve that in its revision.

AS: Joseph and Newman worry that EI may not neatly break down into the four sub-abilities posited by MSCEIT — perceiving, understanding, using and regulating emotions (a.k.a. “branches”) — suggesting that more or fewer sub-abilities may be needed (they doubt in particular the viability of the ‘using emotions’ branch). Why did you settle on exactly four sub-abilities? Additionally, Joseph and Newman wonder whether all and only abilities that are relevant to EI as you and Salovey originally defined it are assessed by MSCEIT. For instance, how does the MSCEIT test for one’s ability to express emotions or to perceive emotions in oneself (broader worry: are all relevant aspects of EI captured by MISCEIT)? And how is the ability to match emotions to colors and tastes relevant to the assessment of EI (broader worry: are aspects irrelevant to EI assessed by MSCEIT)? Finally, to answer an issue raised by Antonakis, what reason is there to think that the four abilities in question reflect a unified and underlying higher-order ability (EI) rather than being broadly independent?

JDM: Well, your first set of questions here ask whether the MSCEIT suffers from construct underrepresentation: What the *Standards of Educational and Psychological Testing* defines as a failure of a test to measure all that it ought to given its purpose. No intelligence test can include all possible measures of its target ability, and the MSCEIT is no exception. For example, we developed the Multifactor Emotional Intelligence Scale (MEIS) before the MSCEIT. The MEIS had a scale for measuring emotional perception in music, in which people listened to short original pieces of music and identified the emotional expressions they conveyed. A second MEIS subscale asked people to estimate the emotion of characters in brief stories. The MSCEIT excluded those scales in favor of other scales that better fulfilled our psychometric needs for the test. The issue then is whether we represent the ability with relative completeness through the specific abilities we measure on the test. An empirical argument to the contrary would require an ability-based measure of emotional-intelligence that measured factors of emotional intelligence not assessed by the MSCEIT. I am unaware of any such research or research finding so far.

In addition to construct underrepresentation, the *Standards* also point out that tests can suffer from construct irrelevant variance if they measure qualities unrelated to the construct. Some people might argue that our emotional synesthesia task — e.g. matching emotions to colors — falls into that area, although I wouldn’t agree because, in my opinion, being able to understand the sensory evocations of emotions is part of understanding and using them to think and to communicate with others.

One of the frustrating issues in psychometric work is that some of the concepts concerning test validity and the mathematical modeling of tests get very technical very quickly. For example the question of why we settled on four abilities involved our theoretical process and a number of measurement issues as we saw them in 1995-1996. The idea that we may measure too many or too few aspects of emotional intelligence has to do with theoretical issues involving what emotional intelligence is—that is, is our theoretical model a good one? The resolution of the debate is, therefore, partly theoretical. But the number of “branches” also rests on empirical matters such as the nature of our test’s items, reliability and covariance structure. To go into detail about these issues here would be beyond the scope of this interview. Those interested in the technical details can refer to a number of interesting ar-

ticles on the topic (see, for example, [Maul, 2011](#); [Mayer, Salovey & Caruso, 2013](#)). For general principles in this area, see the *Standards for Educational and Psychological Testing*, my forthcoming book, *The Elements of Mental Tests*, or any other general references in testing.

Setting technicalities aside, I can say further that I now prefer to interpret the four-branch model I developed with Salovey as representing the areas of problem-solving involved in emotional intelligence—but not necessarily a demarcation of four distinct mental abilities. That is, every intelligence has a *class of problems* it can be applied to. The four-branch model describes that class of problems as we envision it. To solve those kinds of problems, people construct a “problem space” — a mental representation of the problem to be solved, and then attempt to answer the specific questions within that problem space (see [Hmelo-Silver, 2013](#); Keren, 1984).

On reflection, there is no particular reason to expect an exact correspondence between the areas of problems we solve — what is described by the four-branch model — and the number of distinct mental abilities we use to solve them. Psychologists determine the number of mental abilities involved through factor analysis and other related means. The exact number of mental abilities involved in solving problems in the emotion domain is an unsettled issue as of now. We ourselves have noted that two- and three-ability factor models might work as well or better than four-ability factor models. Whatever the final factor structure, the four-branch model may remain useful as a demarcation of the kinds of emotional problems people solve.

Regarding our rationale for the various abilities of emotional intelligence forming a unitary intelligence, the MSCEIT tasks generally correlate $r = .25$ to $.65$; these positive correlations provide a basis for the claim emotional intelligence is a unitary ability rather than a set of independent skills (more detail can be found in the technically-oriented publications cited earlier).

AS: Much of the excitement surrounding EI comes from the assumption that measuring it will give us a powerful predictive tool we would not otherwise have with respect to various phenomena of interest, most prominently work performance.

JDM: In case any of your readers may have missed it, let me begin by saying that Peter Salovey and I thought the idea of emotional intelligence was intriguing and might explain *something* and we hadn't given much thought to the strengths of any specific relationships. So we didn't make any claims about the strength with which emotional intelligence might predict outcomes. Journalists who first reported on our ideas made their own claims. We tried to guide the field toward more realistic perspectives. Writing in the American Psychological Association's *Monitor*, I explained:

“Our own and others' ongoing research indicates that emotional intelligence may well predict specific, important life variables at about the level of other important personality variables (e.g., 2 percent to 25 percent of the variance explained)...In contrast, the popular literature's implication—that highly emotionally intelligent people possess an unqualified advantage in life—appears overly enthusiastic at present and unsubstantiated by reasonable scientific standards.” ([Mayer, 1999](#))

AS: Both Antonakis and Joseph and Newman doubt that EI scores significantly add to the predictive power of measures of general intelligence and personality traits with respect to work performance. Joseph and Newman also suggest that EI scores may be better predictors of job satisfaction or the size of one's social network than of work performance in particular. What is your answer to these worries? And what aspects of life's success do you think EI scores are better predictors of, once we have accounted for personality and general intelligence?

JDM: In the 19th and 20th centuries, the quantifiable indices of success and productivity were relatively limited—grades in school, salary level and (for businesses) the bottom line profits. As we have transitioned into the 21st century, however, we are beginning to understand that our world—and our lives—are interrelated systems that are best assessed in multiple ways rather than by one criterion or another. Our measurement practices are beginning to catch up with these ideas.

Organizations, for example, are now gauged in part according to their social responsibility as well as their profits. Nations are assessed not only as to their wealth but also according to the freedom of their press and the well-being of their citizens. Individuals' attainment is addressed not just by examining a person's salary or the size of his

family, but also in terms of his health, social relations and his longevity.

The profits of companies, the wealth of nations and the educational attainments of individuals all are very important. However, it may be that we won't be able to fully appreciate the incremental predictive value of emotional intelligence unless we are examining criteria designed with emotional intelligence in mind. Broad intelligences predict criteria that often go unmeasured as of now and that we need to focus more on these heretofore unmeasured (or less emphasized) criteria.

In my *Annual Review* examination of the EI area with Roberts and Barsade ([Mayer, Roberts and Barsade 2008](#)), we concluded that the strongest relations between emotional intelligence and life outcomes concerned not salary or leadership position but rather people's relationships with their colleagues, their social acceptance, and some aspects of job satisfaction.

AS: Joseph and Newman report that sex differences exist in abilities-based EI scores (e.g. MSCEIT), with women scoring significantly higher than men, but that such differences vanish when EI is measured through self-reports, which is to say that males and females assess themselves to be equally emotionally intelligent despite perhaps not being so. Does this result surprise you? More generally, do you expect EI to be differentially distributed across not only gender, but also socio-economic status, race, and age? Finally, do you expect psychopaths to score lower on EI than normal people do, or could their socially manipulative behavior actually rely on higher EI, as Joseph and Newman hint at? If so, is there a 'dark side' to EI?

JDM: One reason I became a personality psychologist is because I prefer to understand individuals according to the specifics of who they are, rather than inferring what they are like from memberships in groups. I realize people have legitimate research interests that lead them to study group differences. For example, as test authors, my colleagues and I have the responsibility to ensure that the MSCEIT items are as free of bias toward any given group as we could make them. Beyond that responsibility, however, my chosen way of thinking emphasizes, to draw from Dr. Martin Luther King, the content of a person's character. For those interested in group differences, MHS collects norms for different groups and these have been reported in our 2002 test manual ([Mayer, Salovey & Caruso, 2002](#)).

You also asked about psychopathy. In a [2012 article](#), Elsa Ermer and her colleagues, along with Peter Salovey, found that among incarcerated men, those with psychopathic symptoms scored lower on the MSCEIT, even after controlling for general intelligence. There probably is a dark side to emotional intelligence—emotional manipulateness—but based on these results probably not psychopathy.

AS: In their paper for this issue, Mikolajczak and Peña-Sarrionandia argue that studies conducted in their lab show that EI can be successfully taught. What is your view on whether EI is an innate or a learned ability and, in light of your expertise, what do you consider the best ways to make people more emotionally intelligent?

JDM: Intelligence researchers have traditionally reserved a very specific meaning for "raising an intelligence": It refers to *raising the ability* to carry out abstract reasoning. I don't know whether EI can be raised or not—and I don't think anyone else does either. In principle researchers could teach the answer to any intelligence test item and their students' abilities would seem to rise, but this would not speak to a real rise in their ability—it would simply reflect teaching the answers to the test.

I do believe, however, that we can raise people's emotional functioning by teaching them about emotions. Many educational curricula in this area have identifiably positive impacts. That said, their teachings often extend far beyond emotions. These programs may be effective and helpful because their teachings are very broad; we simply don't know which of their teachings are effective, or whether it is a global effect; either way, their connection to ability models of emotional intelligence are uncertain in some instances (see, for example, [Cherry et al., 2014](#)). Elsewhere I have argued that many programs allegedly focused on teaching EI are likely teaching something closer to knowledge of personality—more relevant to personal intelligence than to emotional intelligence (see [Mayer, 2014](#)).

AS: Let us get to what may be the thorniest issues of all, namely the popularization of EI and the commercialization of EI testing. Five years after you introduced the concept of EI in your joint paper with Salovey, Daniel Goleman took the publishing world by storm with his New York Times bestseller *Emotional intelligence: Why it can matter*

more than IQ. Ever since, popularizers of EI have claimed, contrary to what the scientific data appear to suggest, that EI is a better predictor of work performance than IQ. You have taken issue with hyperbolic claims of this sort in print, but in his piece Antonakis argues that the very publishers of MSCEIT in their advertising materials make scientifically unwarranted claims about the predictive power of EI in ways that closely echo Goleman's own. This, Antonakis continues, raises an ethical problem, because unsuspecting human resource managers may be swayed to spend lots of money on EI testing under false pretenses. How do you answer this charge, and, more generally, how do you think scientists who study emotional intelligence like yourself can keep science separate from popular hype, and the trail of money that goes with it?

JDM: I write generally about the need to raise test literacy in my forthcoming book, *The Elements of Mental Tests* (Momentum Press, 2015). As you mentioned, I've also been active in encouraging people to be realistic about what EI might or might not predict in numerous popular publications and also in journal articles and book chapters.

I believe the public *has* become more realistic over the years about emotional intelligence. When I speak on the topic, I often begin by saying that general intelligence is a very powerful predictor of outcomes—more powerful than emotional intelligence, though (I believe) emotional intelligence is both theoretically important and practically useful. In the late 1990s, my audiences were often shocked to hear me speak that way and felt betrayed that I wasn't joining the journalistic chorus that emotional intelligence was "the best predictor of success in life". Nowadays I think most audiences are far more sophisticated—at least in terms of their moderated expectations. I like to think that my public speaking and writing, along with that of many other psychologists who promoted more realistic thinking about the topic, had something to do with that new realism.

We are regularly in contact with the research staff at MHS who have a quite clear-eyed view of the test. As opportunities have arisen we also have engaged with members of the sales and marketing area (and they have sought us out) to try to better explain the MSCEIT and its predictions. That said, when authors sign a contract with a book publisher or a test publisher, they agree that the publisher has the final say in how a given work will be marketed.

AS: On a related note, Joseph and Newman's main complaint is that researchers interested in testing the scientific viability of EI are unable to do their job properly, due to the prohibitive cost of MSCEIT, which is copyrighted intellectual property. This adds to the ethical dilemma raised by Antonakis, because not only financial incentives may lead to hyperbolic claims about EI's predictive success, but also the very cost of MSCEIT testing may stand in the way of assessing how predictively powerful it actually is. Joseph and Newman propose that MSCEIT testing is made available for researchers for a nominal cost or for free. What is your response to this proposal?

JDM: I agree it would be desirable for MHS to offer the test to researchers for a nominal cost or for free. But what is desirable and what is practical under a given system are not always the same because of the costs involved to an organization. We have a commitment from MHS, which they have honored, to offer researchers the MSCEIT at a considerable discount. My colleagues and I are aware that even with the discount some researchers cannot afford to work with the test. MHS has, in the past, tried to go beyond our agreement to make further arrangements with researchers (e.g., offering the test for free in exchange for the researchers adding other measures that would help further validate the MSCEIT). But I realize that not every researcher has that opportunity and the system doesn't always work.

It is worth weighing those frustrations against what MHS can and does provide that my colleagues and I would have been unable to do ourselves (without quitting our day jobs). MHS standardized the test on 5,000 participants worldwide, their research department checked our work, managed the proper scoring of the instrument, and served in the role of editor and publisher for the test manual. They supervised high-quality translations of the test in over 20 languages, provided near-world-wide distribution of the test, on-line administration, and data scoring and report generation. Moreover, our partnership has continued with the recent release of the MSCEIT Youth Research Version (MSCEIT-YRV), a test that addresses some of the issues raised here about scoring. We hope and expect to improve on the tests more in the future.

AS: Well, thanks much for your availability, Jack. I am convinced that reading your answers along with the papers in this issue of *Emotion Researcher* will allow our readers to get a good idea of the state of play in emotional intelligence research, along with a sense of the main challenges and opportunities currently afforded by EI testing.

JDM: Andrea, let me thank you for the opportunity to respond to your questions. The advancement of our understanding of intelligences and intelligence measures is an important factor in understanding human personality. The first intelligence tests came out in the early 1900's and more than 100 years later the definition and assessment of general intelligence is still a work in progress.

It has been nearly 25 years since Peter and I published our theory of personal intelligence and the first demonstration of how it might be measured (Mayer, DiPaolo & Salovey, 1990; [Salovey & Mayer, 1990](#)), and just 13 years since the release of the MSCEIT. In that time, the field has greatly matured in its understanding of emotional intelligence and how to measure it. Let's conduct a follow-up interview in about 75 years to see where this notion of an emotional intelligence has gone.

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The Component Process Model of Emotion, and the Power of Coincidences

An Interview with Andrea Scarantino (March 2015)

Klaus Scherer is Professor emeritus at the University of Geneva. He has published over 200 scientific papers and over 100 book chapters, co-founded ISRE, co-founded the Consortium for European Research on Emotion (CERE), co-founded the journal Emotion, founded and co-edited the Emotion and Social Interaction series for Cambridge University Press and the Affective Science series for Oxford University Press, and founded and successfully administered the Swiss Center for Affective Sciences from 2005 to 2013. His influential theory of emotions – the Component Process Model (CPM) – holds that emotions are instantiated when dynamically varying appraisal check outcomes causally determine the adaptive synchronization of different response systems (motivation, physiological changes, motor expression). CPM has provided a fruitful framework for investigating the elicitation of emotions, facial expressions, feelings, categorizations of feelings, musical emotions and emotional intelligence.



You grew up in Germany. What are your memories of family life as a young man? Was being a scientist an early dream of yours?

After recovering from a shell-shock resulting from our bunker being bombed during an air raid in 1944 (by being submerged in very cold water by my mother), I grew up in a drab industrial city (Leverkusen) near Cologne in Germany. I was an only child, with a mixture of progenitors from very different parts of Germany, without any strong attachments to a place or a dialect (which possibly facilitated later culture hopping).

I was not destined to go to high school or university (at the time in Leverkusen this used to be reserved for the offspring of the scientists working for a big chemical/pharmaceutical company), except that, by chance, just at the right time there was an initiative for pupils of the secondary modern school to switch to the “Gymnasium” (the gateway to university). My teacher suggested to my parents to let me take an examination that would allow a transfer, and luckily, they agreed. This was the first of a large number of fortuitous coincidences that have collectively shaped my career.

So here is coincidence #1: A Spickzettel (“cheat sheet”) gets me into university.

Let me explain. Together with two other candidates I took the exam during a normal class at the high school. While we had no problem with the German and Geography questions, the Math problem turned out to be unsolvable for us. After gloomy 15 minutes, a piece of paper stealthily made its way to our benches. I never quite knew where it came from, but it had the right answers, and it opened the doors to university for me and my fellow exam takers. That door may have never opened for me had it not been for the mysterious “cheat sheet”. Sobering thought.

Other important influences on my career dating back to that time are: Being fascinated by the different voice qualities in “The Archers” on the British Forces Broadcasting Service in Cologne [► [voice research](#)]; founding and running a “tape recording club” with some of the first commercially available tape recorders in Leverkusen (producing radio plays and documentaries) [► [High-tech research](#)]; Experiencing British culture in Elm Park, Essex, Great Britain, during a pupils’ exchange and making a lifelong friend [► [Cross-cultural comparison](#)]. At this time, I had no inkling that I wanted to become a scientist – this happened gradually during my first years at the university.



Klaus' first school day in 1949

You studied economics and social science at the University of Cologne, and then left Germany to attend the London School of Economics first and Harvard University later. Why did you decide to leave your home country to pursue studies abroad?

I financed my undergraduate studies as a freelance for the local pages of a major local newspaper (the Neue Rhein-Zeitung) and had decided to become a journalist. That is why I studied Volkswirtschaft (theoretical economics), a diploma curriculum which also required taking courses in finance, law, sociology, and social psychology. This is what probably laid the ground for a permanent dissatisfaction with sticking to a single discipline. I first veered toward sociology (due to the fascinating lectures of Professor René König on Durkheim and Parsons) and then to social psychology (being attracted more by the individuals in a society rather than society as an abstract cultural concept).

I happened to go to the London School of Economics (LSE) as I was active in student associations and tried to negotiate exchange programs (for example, with the LSE). The then chancellor of the LSE didn't like the idea of exchange programs but bribed me by offering to accept me as a "special student" for a year (presumably thinking that I would stop proposing the exchange program). I mostly worked in social psychology with Bram Oppenheim and Hilde Himmelweit, but also attended lectures by the many luminaries at the LSE, such as Karl Popper and Ernest Gellner. Another faculty member of the LSE social psychology department, Norman Hotopf, kindled my interest in the relationship between language and thought, particularly Benjamin Lee Whorf, and Roger Brown (via the latter's fantastic book *Words and Things*, Brown, 1958). So I went back to Cologne to get my diploma (in Economics and Social Science) and wrote my diploma thesis, in a Whorfian spirit, on an empirical study I conducted on how different languages (German, English, French) affect scientific thinking. I found that they did but I am not sure the methods would hold up today.

I then applied for a prestigious German stipend to go to Harvard to get a Ph.D. with Roger Brown in the social psychology of language. I had been particularly impressed by his paper (with Albert Gilman) "The pronouns of power and solidarity" in which the authors provide a brilliant analysis on how, in languages that have formal and familiar personal pronouns, social structure is mirrored in the often asymmetric use of these pronouns (familiar downwards, formal upwards – even between parents and children). It all worked out and in 1967 I went to the US on the MS Europa being seasick for 6 out of 7 days crossing the Atlantic (flying was expensive then and the German Academic Exchange Service had booked me into a four-berths cabin with a bull hole). I soon found out that Roger Brown had by then focused his research interests almost exclusively on children's acquisition of language, adopting a structuralist, Chomskian, framework (which has never been my favorite approach to language).

Which scholars or classes had a strong impact on you at Harvard University, where you obtained your PhD in Social Psychology in 1970? What did you write your PhD thesis on and with whom?

My three years of graduate school at Harvard's then famous Social Relations Department (sociology, anthropology, and psychology) have undoubtedly had the most important influence on my career. It started with Soc-Rel 101, nicknamed the *parade of the stars*, where most of the famous faculty members presented their work (and had us write term papers) – among others Talcott Parsons, Alex Inkeles, Seymour Martin Lipset, David Riesman, John and Beatrice Whiting, Evon Vogt, David McClelland, Robert Freed Bales, Thomas Pettigrew, and Herbert Kelman. My interest in evolution was particularly kindled by the extraordinary enthusiasm displayed by Jerome Bruner's linking phylogenesis and ontogenesis. I still remember the glowing way in which he described the central role of the opposability of the thumb and the index finger for the evolution of human cognition.



Klaus with his wife Ursula on New Year's Eve, 1963



Young Klaus as General Secretary of the European Economics Student Association, 1963

I became a teaching fellow for Roger Brown's intro course to social psychology, enjoying his brilliant lectures, and was a tutor at one of the Harvard houses (in 1968!). When it came to choosing a topic for the Ph.D. research, I went back to my old favorite, the voice, and planned to study the voice of charisma to better understand the contribution of voice quality and intonation to rhetorical persuasion. There was little research on the expressive functions of voice at the time (except some pioneering work on voice and personality and its diagnostic use in clinical psychiatry). The major problem was to separate the effect of the person who spoke and what was said (the words) from how it was said (voice quality and intonation).

Trying to solve the problem, I developed the "randomized-splicing" technique to render speech unintelligible but keep the voice quality (low tech: cutting the audio tape into little pieces, mixing them in a salad bowl, and splicing them back together). But it turned out that charismatic speakers (Churchill, Hitler, and Kennedy) were immediately recognized even in their spliced version. So I studied the voice as an indicator of personality (as measured by peer ratings) in mock jury sessions which I organized in Cambridge, Mass. and Cologne — and in which the Randomized-Splicing technique worked beautifully (an audio file with an excerpt of a schizophrenic patient – normal and random spliced can be heard by clicking on the link below; for an overview of different masking techniques see [Scherer, Feldstein, Bond, & Rosenthal, 1985](#)).



Klaus working with audiotapes for his thesis, 1963

Audio Player

<http://emotionresearcher.com/wp-content/uploads/2015/03/schizo-normal-and-randomspliced.wav>

00:00

Use Left/Right Arrow keys to advance one second, Up/Down arrows to advance ten seconds. 00:00

00:00

Use Up/Down Arrow keys to increase or decrease volume.

Out of several traits, extraversion (but not dominance, as I had thought) had clear acoustic cues and was recognized with better than chance accuracy, being communicated by the voice more distinctly than any other trait. The thesis was jointly supervised by Roger Brown and Robert Rosenthal – for whom I worked as a research assistant and who had really become my mentor at Harvard. He has been a model for research enthusiasm, methodological rigor, and scientific integrity throughout my career.

And now we come to coincidence #2: Paul Ekman gives a colloquium at the Harvard Ed School. After his talk, I chatted with him and he invited me to visit his lab in San Francisco. This turned out to be the beginning of a long and fruitful collaboration on face and voice which has produced many collaborative studies and joint publications (and edited book series), and was ultimately seminal to the creation of ISRE (see below).

How did you transition from being a PhD student to finding a job at the University of Pennsylvania? And why did you ultimately decide to leave the US to go back to Germany? Was it easy to find a job back in your homeland?

Coincidences, coincidences, coincidences. First, a brief chat in a Porsche on the way to a party cost me a job at Stanford: How that? I had been told that I did not need to give a talk, I had the job and that they had flown me out to convince me to take it. The eminent faculty member driving me and a visiting German professor to the party incidentally asked me what I was working on. I was mistaken as a psycholinguist when I mentioned that I work on the voice. They had just hired a psycholinguist and so I was told a week later that they could not hire me! (Coincidence #3).



Klaus in Harvard yard 1970, graduation day

Then, a brown bag luncheon got me a job at the University of Pennsylvania: a fellow graduate student who had enjoyed my talk recommended me as he could not take the job offered to him at Penn having to serve in the army. I spent two formative years as an Assistant Professor at Penn, which were enjoyable intellectually and socially (great parties!), the highlight being teaching a joint course on language and nonverbal communication with the linguist William Labov, the anthropologist Erving Goffman, and the ethologist W. John Smith. But my wife and I decided to go back to Germany (among other things, it was difficult to find good bread in Philadelphia at the time), so I started actively seeking employment back in Germany.

Coincidence #5: I applied for professorships at three universities – Kiel, Münster and Giessen – offering to come give a talk at my own expense. I felt entitled to do this as I was already a “professor” (although only an Assistant one), had 8 publications in peer-reviewed journals and two submitted, as well as a small book on Nonverbal communication (in German).

Münster and Giessen took me up on the offer. I didn’t get the job at Münster because the Marxist student group Spartakus noted in the acknowledgements of one of my papers that a computer used in voice research with a collaborator at MIT had been co-funded by the US Army. This was considered war research and they opposed my getting the professorship. When I got back to Philadelphia, I found in the mail an official offer from Kiel for an Associate Professorship in Psychology. I got to Kiel in 1972 and half a year later I received an offer for a full professorship at Giessen in social psychology where I stayed until 1985.



Coincidence #6: I talked on the phone to Willem Doise (a student of Serge Moscovici’s and then professor of social psychology in Geneva) about a workshop we were going to organize at the European Laboratory of Social Psychology at the Maison des Sciences de l’Homme in Paris. After having settled the matter, we chatted a bit more and Willem asked me what I was working on at the moment. I said: “Emotion”. And he said: “We have a position for “Socioaffectivité” in the department at the University of Geneva to be filled right now”. I did not quite know what that meant in 1985 but I have been there ever since.



Your highly influential theory of emotions is labeled the Componential Process Model, which you first introduced in 1982 (Scherer, 1982). Could you summarize its main tenets, explain what led you to develop it, and illustrate what you take to be the main empirical evidence for synchronization amongst emotion components?

This was due to another coincidence, #7: In 1980 Norbert Bischof organized a congress of the German Psychological Society in Zurich. For the first time in German psychology, prestigious one-hour long keynote presentations are introduced, with competitive bidding. I thought it would be nice to give one of those and tried to think of a promising topic. I recalled my review of the primordial role of emotions in the triggering of aggressive behavior for a book on aggression (Scherer, Abeles, & Fischer, 1975) which I had written with two fellow graduate students from Harvard a few years after our Ph.D. I noted that the valence (positive vs. negative) and power (strong vs. weak) dimensions of emotional feeling (as postulated by dimensional theories of emotion ever since Wundt) corresponded directly to Richard Lazarus’ proposal of *primary (good – bad for me) and secondary (can – cannot cope) appraisal* as determinants of stress and emotion. Specifically, I assumed that the position on a particular feeling dimension was directly produced by the result of the respective appraisal check.

I reread Lazarus (1968, 1970) and decided to suggest a new conceptualization of emotion by elaborating on the nature and structure of the different appraisal criteria or checks and their consequences on the different com-

ponents of emotional responses. Briefly put, I argued that it should be possible to predict complex emotional feelings by differentiating the appraisal process sufficiently, using many different criteria or “checks”, to account for the major emotions. I imagined that this must happen in a process as situations and evaluations unfold over time. Given my exposure to evolutionary functionalism at Harvard, I speculated that this mechanism must serve some purpose, presumably the preparation of adaptive behavior (such as aggression as a result of frustration).

I elaborated these ideas on long walks during a vacation with my wife in the Swiss Alps and submitted a keynote proposal in 1980 called “Wider die Vernachlässigung der Emotion in der Psychologie” [Against the neglect of emotion in psychology; Scherer, 1981], combining the proposal of a new approach to studying emotion with an attack on the domination of the field by an exclusively cognitive orientation. The keynote was accepted, the talk well attended and well received, and the paper that appeared in the Proceedings a year later is, strangely enough, still one of my most cited papers (although mostly in German publications). I presented the theory at Stanford a bit later and discovered that Ira Roseman and Phoebe Ellsworth had developed a very similar approach, emphasizing the centrality of appraisal and proposing a list of differentiated criteria to predict major emotions (at the time documented only in the form of a mimeographed text that circulated among the cognoscenti).

The Component Process Model (CPM) of emotion was developed on the basis of evolutionary considerations. I argued that animals dealing with ever more complex environments and a social organization could not manage with simple stimulus-response chains based on innate instincts or simple learning and that thus emotions progressively replaced these rigid response modes with a more flexible mechanism without losing the advantage of very rapid response preparation. In other words, the “decoupling” of a reflex-like adaptive response from a specific class of elicitor together with the possibility of modulating and regulating the reflex-like response over time, based on constant reevaluation of an evolving situation.

The key feature of such a mechanism is the sophisticated evaluation or appraisal of important events that require a response and possibly adaptive action. For this, it is necessary that the probable causes and consequences of the event and the organism’s options for action (control and power) are assessed in an ongoing, recursive appraisal *process* (as events often unfold and change and as coping options become available). As in responding to many important events, time is of the essence, and the reactions of the organism need to be continuously adjusted to reflect changes in appraisal outcomes. In addition, it is often necessary to prepare several courses of potential action (or action tendencies) such as fight, flight or freeze when encountering, say, a predator. As many important events require the mobilization and coordination of all of the resources the organism can muster, the different organismic subsystems (or components; see Table 1 below) need to be concerted and synchronized for optimal performance.

In sum, the CPM argues that a series of constantly varying appraisal check outcomes causally determines adaptive changes in different response systems (motivation, physiological changes, motor expression) which consequently become synchronized (and desynchronized) in the process of emotion unfolding.

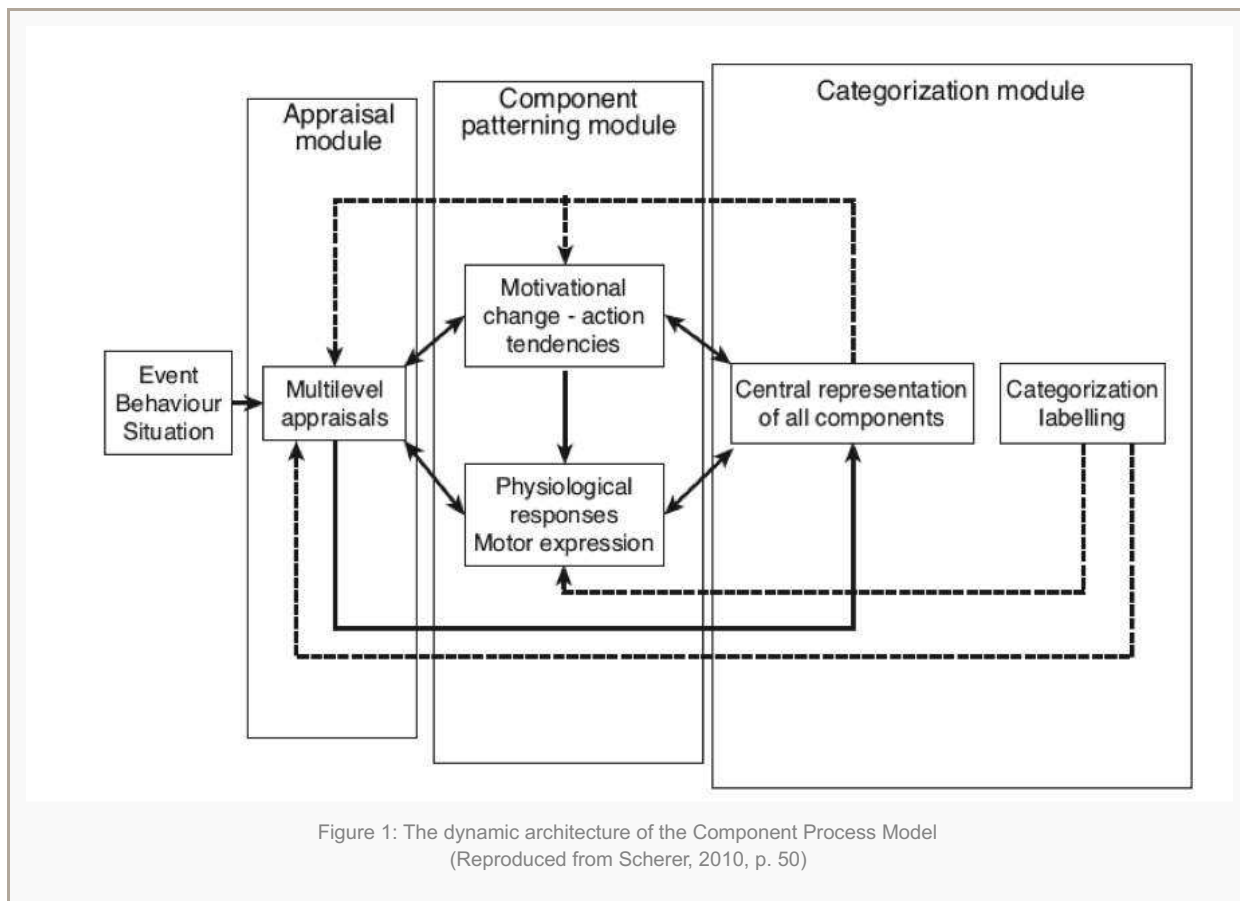
Relationships between organismic subsystems and the functions and components of emotion

Emotion function	Organismic subsystem and major substrata	Emotion component
Evaluation of objects and events	Information processing (CNS)	Cognitive component (appraisal)
System regulation	Support (CNS, NES, ANS)	Neurophysiological component (bodily symptoms)
Preparation and direction of action	Executive (CNS)	Motivational component (action tendencies)
Communication of reaction and behavioral intention	Action (SNS)	Motor expression component (facial and vocal expression)
Monitoring of internal state and organism–environment interaction	Monitor (CNS)	Subjective feeling component (emotional experience)

Note: CNS = central nervous system; NES = neuro-endocrine system; ANS = autonomic nervous system; SNS = somatic nervous system.

Table 1: Reproduced from Scherer, K. R. (2001, p. 93).

These synchronized changes are monitored by central, integrative representation systems to allow regulation (as each subsystem has essential functions of its own and needs to return to baseline). In humans (and possibly also higher animals) this central representation constitutes (often, but not necessarily conscious) emotional experience. In addition, after the acquisition of language, humans can, optionally, categorize and label these bouts of experience with words or expressions. Early on, I suggest a formal definition based on this kind of an architecture (see Figure 1): “emotion is defined as an episode of interrelated, synchronized changes in the states of all or most of the five organismic subsystems in response to the evaluation of an external or internal stimulus event as relevant to major concerns of the organism (Scherer, 1986, 2001).”



However, as there are already hundreds of definitions of emotion around, and because researchers generally preferring to develop their own, there is little chance of my proposal having much of an impact. Trying to remedy this unsatisfactory situation, which generates much confusion in the field, the philosopher Kevin Mulligan and I recently attempted to propose at least a list of elements for a partial, working definition “x is an emotion only if x is an affective episode, x has the property of intentionality (i.e., of being directed), x contains bodily changes (arousal, expression, etc.) that are felt, x contains a perceptual or intellectual episode, y, which has the property of intentionality, the intentionality of x is inherited from the intentionality of y, x is triggered by at least one appraisal, x is guided by at least one appraisal” (Mulligan, & Scherer, 2012). Maybe such a working definition can at least help to increase the mutual understanding of researchers from different disciplines...

As to the evidence for synchronization, this is probably one of the most complex research challenges in the affective sciences. But we are working on it. And there is growing attention and empirical evidence for the existence of coherence or synchrony – see the special issue of *Biological Psychology* in Volume 98 (2014) “Whither Concordance? Autonomic Psychophysiology and the Behaviors and Cognitions of Emotional Responsivity”. In our contribution to this special issue (Gentsch, Grandjean, & Scherer, 2014) we explore the link between brain activity (measured by electroencephalography, EEG) and facial expression (measured by electromyography of the facial muscles, EMG) in response to experimentally manipulated appraisal. This provides indirect evidence that the appraisal results of the goal conduciveness and power to cope checks are producing with a slight delay, the innervation of specific facial muscles (e.g., m. zygomaticus).

What is currently missing is an appropriate mathematical-statistical toolbox to empirically determine the degree of synchrony of different organismic subsystems, both with respect to the concordance of discrete events and the synchronization of time series. This is essential, at least in my book, to determine the threshold of synchronization that can be considered to determine the onset of an emotion episode and the intensity of the emotional reaction.

In your entry “emotion theories and concepts (psychological perspectives)” in the Oxford Companion to Affective Sciences you edited with David Sander (Scherer, 2009), you distinguish between three main traditions in the scientific study of emotions: the basic emotions tradition, the constructivist tradition and the appraisal tradition. The CPM belongs to the third tradition, but does it borrow insights from other traditions as well?

Of course, we are always standing on the shoulders of giants. And so am I. Aristotle was one of the first, arguing against Plato's tripartite theory of the soul (which assigned a specific compartment to emotion) and highlighting the strategic function of emotion expression in rhetoric. Descartes of course, and most importantly, Darwin, with respect to both anchoring emotion in a functional, evolutionary perspective and his trail blazing work on expression. William James had a strong impact because of his rehabilitation of the bodily components of emotion and his interest in "subtle" emotions.

Unfortunately, his abhorrence of contemporary emotion theories led him to overstate his case to the point of, in the words of Phoebe Ellsworth (1994), generating "a century of misunderstanding". He used the term "feeling", a single component denoting the subjective experience process, as a synonym for emotion, the total multi-modal component process. As a consequence, he produced a fruitless debate which is still ongoing (thanks to constructionism). Arguably, when he asked "What is an emotion?" in 1884, he really meant "What is a feeling?" For me, James has been the first appraisal theorist. Addressing his critics, James wrote, 10 years after his initial paper: "The same bear may truly enough excite us to either fight or flight, according as he suggests an overpowering "idea" of his killing us, or one of our killing him" (James, 1894).

One of your central theoretical interests is appraisal, the complex evaluative process that initiates the synchronized changes with which you identify emotions. You have introduced several seminal distinctions over the years, concerning the existence of different levels of appraisals and the structured nature of the appraisal process. Could you briefly summarize how you came to be interested in appraisal, and how your theory of appraisal differs and resembles other models of appraisal?

Given the central, causal role of appraisal in the CPM, I have attempted to develop an inventory of appraisal criteria or checks that are sufficiently general to be applied to many different situations but that are also sufficiently exhaustive to allow adequate differentiation between emotions. James' bear coming out of the woods is a perfect example: The sudden appearance of the grizzly at your picnic site is appraised as novel and relevant, two major appraisal criteria. After that it depends, if you are a bear hunter this event is very conducive to your goal, especially if you have your gun within easy reach (the coping potential check). If however, you are hiking, just enjoying nature, the bear is likely to be a major threat, obstructive to your goal of survival. The criterion of goal conduciveness or obstructiveness is obviously of central importance, followed by the coping potential criterion – distinguishing between joy and fear. Anger is a consequence of appraising that one's progress toward a goal is blocked (for example, the bear wanting to eat your food) but only when you feel stronger than the bear (appraising a high coping potential).

It comes as no surprise that most of these criteria are also found in other appraisal theories, although the labels used, the foci of attention, or the use of the concept of appraisal, may differ. I think my CPM mainly differs in that I have postulated a recursive, sequentially cumulative process for appraisal and that I have attempted to produce detailed and testable predictions about the causal effects of the appraisal results on other components, such as motivation, physiological responses, facial and vocal expression, as well as categorization and labeling of subjective feeling (see Scherer, 2001, 2009). This recursive, sequential and cumulative architecture is illustrated in Figure 2 below which shows on top the various systems involved in appraisal processing, the specific appraisal checks for each major criterion, and the efferent effects on the different organismic subsystems (i.e. the other components).

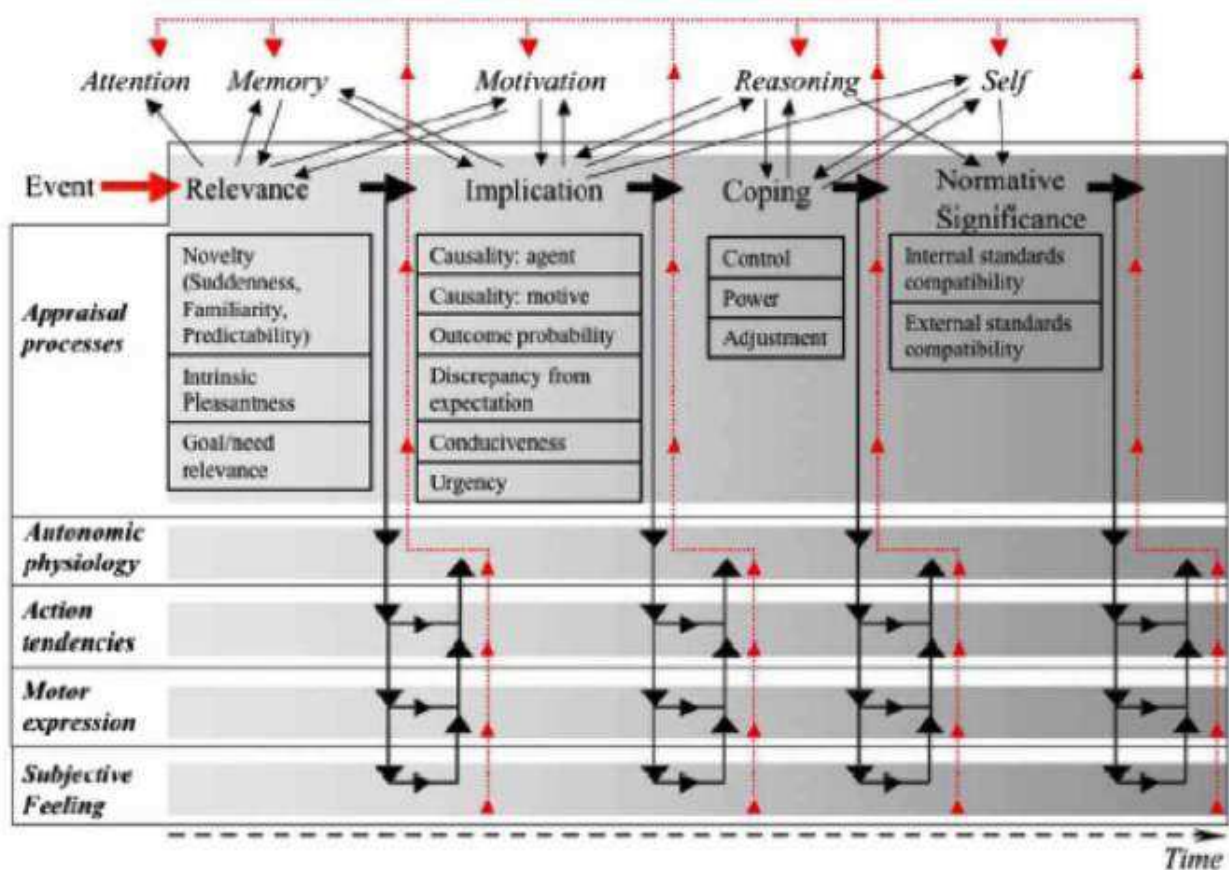


Figure 2. Schematic summary of the component process model (reproduced with permission from Sander, Grandjean, & Scherer, 2005). To view this figure in colour, please visit the online version of this issue.

Claiming such a complex architecture may have been a bit too idealistic, but I am very happy to note that, as pertinent research is progressing and literature is accumulating, many of these predictions have been confirmed. In a recent review paper for *Emotion Review* I have summarized some of the major predictions and the empirical evidence currently available (Scherer, 2013). Predictions that are not confirmed may need to be refined or revised with further evidence — after all, that is the nature of theoretically guided empirical research. The problem is that this kind of research requires the collaboration of specialists in several domains and is thus rather lengthy and costly — which explains why progress is slow.

Some have suggested that your sequential model of appraisal, which demands 14 Stimulus Evaluation Checks performed in a logical sequence, is hard to reconcile with the speed of emotional elicitation and with the fact that creatures without concept mastery like infants and animals can have emotions. How do you respond to these worries?

These “worries” have accompanied the CPM for all the over 30 years of its existence. Unfortunately, my counter-arguments and explanations are hardly ever taken into account. In 1987, I published a paper with Howard Leventhal demonstrating that the appraisal checks in the predicted sequence can occur on different levels of cognitive processing, explaining how animals and infants can have differentiated emotions (Leventhal, & Scherer, 1987).

I now distinguish four different levels of processing : (a) a low-level neural circuit, in which the checking mechanisms are mostly genetically determined and the checking criteria consist of templates for pattern matching and similar mechanisms (cf. the notion of “biological preparedness”, e.g. for snakes or baby faces; (b) a schematic level, based on memory traces from (social) learning processes and occurring in a fairly automatic, unconscious fashion (e.g., childhood memories of odors); (c) an association level, involving various cortical association areas, which may occur automatically and unconsciously or in a deliberate, conscious fashion (e.g., schemata in social perception), and (d) the conceptual level, involving propositional knowledge, and underlying cultural meaning sys-

tems, requiring consciousness and effortful calculations in prefrontal cortical areas (see Scherer, 2009). The different levels continuously interact, producing top-down and bottom-up effects. So — if the amygdala can perform relevance appraisals – how much faster can you get?

The problem is that many of our colleagues react to a connotation implying frontal cortex activity when they hear the word “appraisal”. However, the correct denotation of this word is “evaluative processing of information” — and that can be as low-level as information processing gets (involving subcortical structures). As to the “slow and cumbersome” sequence of appraisal checks, we now have hard evidence from three mental chronography studies using EEG – Grandjean & Scherer (2008), Gentsch, Grandjean & Scherer (2013), and van Peer, Grandjean & Scherer (2014). We experimentally manipulated different appraisal checks and showed with the help of precisely timed event-related potentials (ERPs) and brain activity mapping that in the initial phase after an event onset novelty, intrinsic pleasantness, goal conduciveness, and power are all processed, in this sequential order, within 600-800 msec. That seems pretty fast....

Another worry concerns the primary source of evidence for the existence of appraisal dimensions, which I take to be self-reports. Some have argued that self-reports fail to capture the sort of automatic processes involved in a great many forms of appraisal, and, most importantly, that self-reports do not necessarily unveil the causal structure of the elicitation process but rather the conceptual entailments between folk emotion concepts and other concepts (e.g. the concept of fear and the concepts of something being goal-relevant and goal-incongruent). Why do you think that self-reports provide us with reliable evidence of the relevant causal processes? Other than self-reports, which other experimental techniques are available for the study of appraisal?

From the very beginning of my publications on the CPM I have presented a big table (reproduced as Table 1 in a recent update article: Scherer, 2009) in which I made predictions on how the results of different appraisal checks will affect physiological responses as well as facial and vocal expressions. Table 2 below provides an illustration of component patterning predictions for two of the appraisal checks – novelty and intrinsic pleasantness. By the way, for reasons documented in the same paper, I have always insisted on clearly separating the appraisal checks of intrinsic un/pleasantness and goal conduciveness/obstruction, a distinction which is also centrally important for the understanding of aesthetic emotions (see below for the case of music).

<i>Stimulus Evaluation Checks (SECs)</i>	<i>Organismic / Social functions</i>	<i>Component patterning</i>
<i>Novelty (Abrupt onset, familiarity, predictability) Goal relevance (Does the event have consequences for my needs or goals?)</i>	<i>Novel and goal relevant: Orienting, Focusing/ Alerting</i>	<i>Orienting response; EEG alpha changes, modulation of the P3a in ERPs; heart rate deceleration, vasomotor contraction, increased skin conductance responses, pupillary dilatation, local muscle tonus changes; brows and lids up, frown, jaw drop, gaze directed; interruption of speech and action, raising head (possibly also preparatory changes for subsequent effort investment given relevance appraisal at this stage, in particular increased cardiac contractility as indicated by, e.g., decreased pre-ejection period)</i>

<i>Intrinsic Pleasantness</i> (Is the event intrinsically pleasant or unpleasant, independently of my current motivational state?)	<i>Pleasant:</i> Incorporation/Recommending <i>Unpleasant:</i> Rejection/Warning	Sensitization; inhalation, heart rate deceleration, salivation, pupillary dilatation; lids up, open mouth and nostrils, lips part and corners pulled upwards, gaze directed; faucal and pharyngeal expansion, vocal tract shortened and relaxation of tract walls (“wide voice” – increase in low frequency energy, F1 falling, slightly broader F1 bandwidth); centripetal hand and arm movements, expanding posture, approach locomotion Defense response, heart rate acceleration, increase in skin conductance level, decrease in salivation, pupillary constriction; slight muscle tonus increase; brow lowering, lid tightening, eye closing, nose wrinkling, upper lip raising, lip corner depression, chin raise, lip press, nostril compression, tongue thrust, gaze aversion; faucal and pharyngeal constriction, vocal tract shortened and tensing of tract walls (“narrow voice” – more high frequency energy, F1 rising, F2 and F3 falling, narrow F1 bandwidth, laryngopharyngeal nasality, resonances raised); centrifugal hand and arm movements, hands covering orifices, shrinking posture, avoidance locomotion.
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Table 2: An illustration of component patterning predictions based on the Component Process Model (CPM) of emotion

I have strongly advocated using objective measures such as brain activity, autonomic system change, or facial/vocal expression indices to provide empirical evidence for the occurrence of specific appraisals. But such research is time consuming and costly and requires an extraordinary amount of expertise. It is thus not surprising that it has only been relatively recently that massive evidence for appraisals based on EEG, facial EMG, autonomic indicators, FACS coded facial expressions, and acoustic analysis of voice quality has appeared (much of which has been produced by the members of my group). I hope that the long list of pertinent references which can be downloaded [here](#) will convince researchers who still believe that appraisal is only about self-report.

At the same time, I refuse to accept the implicit assumption that self-reports are worthless. Yes, they can be affected by all kinds of biases such as folk concepts and other beliefs, memory bias, and response styles. Yet, I don't think that there is much empirical evidence showing that such biases render self-report worthless. I think people often have good access and sufficient reporting capacity to their evaluations of relevance, pleasantness, agency, justice, morality, etc. And for some of these higher levels, particularly compatibility with norms, values and moral prescriptions, it will be very difficult to find objective indicators (for example, evaluations of music with respect to its cultural value).

However, Brosch et al. (2011), using fMRI, were able to show that context-dependent neural reward sensitivity biases reflect (and may even determine) differences in individual value hierarchies. Of course, it is hard to use the scanner in the field and so I suggest that we continue to use self-report for those cases where there is no viable alternative – of course with all due caution. The key is to have several different measures to obtain a convergent pattern of evidence.

You have spent a great deal of time trying to understand emotional expressions, in the face, posture and voice. First, which of these three sources gives us the most reliable evidence for inferring that a certain emotion is occurring? Second, what is your view on universality vs. context-dependent cultural influences in emotion production and perception? Third, what is the Geneva Multimodal expression corpus for experimental research on emotion perception?

This is difficult to answer in a few words. I will try: 1) As to the reliability of inferences, face and voice have different expression and communication affordances. The face is a very good indicator for the valence dimension (e.g., happiness or disgust) all it needs is a smile vs. pulling up the upper lip. On the other hand the voice is a more appropriate and reliable indicator for the power and arousal dimensions, e.g., in anger and fear. A loud and firm voice communicates power; a thin, high-pitched voice is a sure sign for powerlessness. Also, high pitch and loudness variation together with high speech rate are very indicative for high arousal. These differences in signal information and reliability explain why it is always best to use multimodal expressions as a source for inference as the channels complement each other.

As to 2), universality vs. cultural relativity in expression, I have always thought that the most plausible position

would be to assume that the basic mechanisms are universal, as we are all humans, having similar brains and bodies and face similar social and environmental challenges. At the same time, there clearly are many cultural differences in social behavior and so it would be odd, if emotional expression which is eminently social, would not show any differences. This hunch has been supported by a comprehensive review of emotional expression research (Scherer, Clark-Polner, & Mortillaro, 2011). The upshot: People can decode expressions from other languages and cultures, but they are best in their own. I think that the notion of *expressive dialects*, as suggested by Elfenbein and others, is very useful in this context.

As to 3), the Geneva Multimodal Expression Portrayal corpus (GEMEP) is a set of actor enactments of 18 different emotions, recorded in high-quality video and audio, based on Stanislavski-like procedures of reliving emotional experiences (Bänziger, Mortillaro, & Scherer, 2012). It is currently used widely in expression research and automatic emotion detection in affective computing approaches. It is shared with researchers in this area (further info: <http://www.affective-sciences.org/gemep>). One is frequently criticized for using actor enacted stimuli for emotion expression research but there is simply no practical alternative if one wants to have a minimum of experimental control (e.g., different emotions expressed by the same person in similar contexts). To date, there is no appropriate corpus with real-life expressions – due to ethical constraints and the enormous investment needed. Critics often forget that if the Stanislavski emotional reliving technique is used, there is only a gradual difference to “real” emotions, especially as most often, every-day emotions can also be faked for strategic purposes, or at least severely regulated.

Another one of your interests concerns the way in which emotions are impacted by music. What do you think are the key mechanisms of elicitation? Can music elicit any emotion whatsoever? What sort of music works best for you as an emotion elicitor?

I started experimenting with music a long time ago in 1972. While I was at Penn a student told me that he was working in the music department and that they had gotten one of the first music synthesizers – the MOOG. I suggested that we do an experiment to orthogonally manipulate amplitude and pitch variation, pitch level and contour, tempo, envelope, and harmonic filtration and have them rated on emotional expressiveness. The results showed that two-thirds to three-quarters of the variance in emotion attributions could be explained by the manipulation of the acoustic cues, and that a linear model of the judges' cue utilization seemed to be a good approximation to their response system (an example of two MOOG sounds can be downloaded below; see [Scherer, & Oshinsky, 1977](#))

Audio Player

<http://emotionresearcher.com/wp-content/uploads/2015/03/moog.wav>

00:00

Use Left/Right Arrow keys to advance one second, Up/Down arrows to advance ten seconds. 00:00

00:00

Use Up/Down Arrow keys to increase or decrease volume.

What do I think are the key mechanisms of elicitation? Contrary to some researchers (e.g., Patrik Juslin), I do not think that musically-evoked emotions are just like any other garden variety of emotions. Of course, music can produce “normal” anger (for example, when my neighbor plays heavy metal at 2 a.m. in the morning) – but the object of the anger is not the music but the behavior of my neighbor (see [Deonna & Scherer, 2009](#), on the importance of proper definition of the object of emotion).

I have argued that when the music is the object, the emotions are different, mostly aesthetic or epistemic emotions (see Scherer, 2004). Based on this conviction, our group has developed the Geneva Emotional Music Scale (GEMS; [Zentner, Grandjean, & Scherer, 2008](#)), using a bottom-up approach, continuously refining lists of emotion terms commonly used by listeners and evaluating them in laboratory and field studies. The scale measures the following 9 factors: Wonder, Transcendence, Tenderness, Nostalgia, Peacefulness, Power, Joyful Activation, Tension, and Sadness. The scale is now widely used in music research and the paper has, surprisingly, become one of the most cited papers in Emotion.

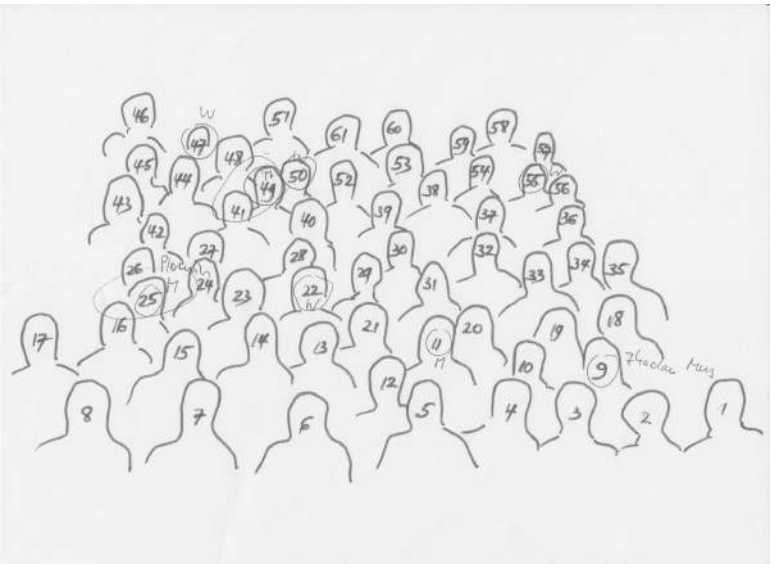
What are the key mechanisms for elicitation of these special emotions? With my collaborators (Marcel Zentner and Eduardo Coutinho), I have postulated five routes to emotion induction by music: 1) appraisal (e.g., aesthetic judgment of beauty due to harmony); 2) memory (e.g., fond memories of the Mendelsohn wedding march); 3) en-

trainment (e.g., a beat of $\frac{3}{4}$ time inducing waltzing); 4) emotional contagion (e.g., weeping during a mournful adagio); and 5) empathy (e.g., with the emotion expressed by a singer). These mechanisms are mediated by many context factors, such as the place and the public, the piece and the composer's intention, the interpreter's excellence and execution, the listener's personality and mood) that we have tried to summarize in rule systems. (for further details see Scherer, & Coutinho, 2013).

What music is best for what purpose? I tend to believe that classical music has the most potential to produce subtle and highly differentiated aesthetic and epistemic emotions. For pure entrainment, marches and heavy metal might be preferable. And so on. I think it depends on the route.

In 1984, you joined forces with several other prominent emotion researchers, including Paul Ekman, Nico Frijda, Joe Campos and Robert Zajonc, to found ISRE. Looking back at the past 30 years, are you happy with how ISRE has developed since? How would you like to see it developing in the future?

As mentioned above, I was a member of the European Laboratory of Social Psychology (LEPS) at the Maison des Sciences de l'Homme [with Serge Moscovici, Henri Tajfel, Willem Doise, Augusto Palmonari, and Mario von Cranach] and used to spend quite a lot of time in Paris. The idea to create ISRE emerged while walking the streets of Paris with Paul Ekman. We asked Joe Campos whether he would be willing to become the first general secretary and organize the first meeting (held at Harvard University in 1985 – see the group photo of the participants below).



Identification of participants on ISRE 85 picture

1	Joe Campos	32	Steve Gordon
2	Linda Camras	33	James Averill
3	Nathan Fox (to be confirmed) <i>ys</i>	34	Bella DePaolo
4	Richard Davidson	35	Anthony Manstead
5	Howard Leventhal	36	James Laird
6	Helen Block Lewis	37	John Lanzetta
7	Robert Plutchik	38	Richard Lazarus
8	Candace Pert <i>?</i>	39	Paul Ekman
9		40	Carolyn Saarni
10	Jeannette Haviland (now Haviland-Jones)	41	Bernard Weiner
11		42	Joe deRivera
12	Jean **** (NSF program officer) <i>?</i>	43	Carol Malatesta (now Magai)
13	Nico Frijda	44	Paul Harris
14	Carroll Izard	45	Herman Brandstatter
15	John Gottman	46	Karl Heider
16	Jaak Panksepp	47	
17	Program officer from some grant agency	48	Robert Levenson
18	Peter Stearns	49	
19	Carol Z. Stearns (to be confirmed) <i>?</i>	50	
20	Shula Sommers	51	Hartvig Dahl
21	Peggy Thoits	52	Theodore Kemper
22		53	Suzanne Retzinger
23	Stephanie Shields	54	Klaus Scherer
24	Manfred Clynes <i>Plausibly</i>	55	Karin Grossmann (to be confirmed) <i>NO</i>
25		56	Bernard Rimé
26	Hugh Wagner	57	Ross Buck
27	James Russell	58	Klaus Grossmann
28	Matty Chiva	59	Victor Denenberg
29	Phoebe Ellsworth	60	Thomas Scheff
30	Seymour Epstein	61	Robert Kleck
31	Harriet Oster		

When he agreed, we drew up a list of leading emotion researchers from different disciplines and with Clemens Heller, the director of the M.S.H., and Adriana Touraine, the secretary of the LEPS, we called the founding meeting (see document posted by Arvid Kappas here <http://emotionresearcher.com/isre-matters-disgust-issue/#>). I think that without ISRE, the development of emotion research would have been quite different. The society managed to build a truly interdisciplinary spirit among its members and several excellent conferences, in addition to personal exchanges, have created contacts and collaborations that would not have materialized otherwise.

Having been a staunch advocate of the interdisciplinary affective sciences for a long time, having co-founded a book series with that title in 1993 and having created the Swiss Center for Affective Sciences (a Swiss National Competence Center for Research) in 2005, I am disappointed that ISRE missed the chance to broaden its scope in the direction of the affective sciences and to involve the affective neurosciences which have been largely absent from the ISRE activities. It may not be too late, especially on the international level, and I would certainly like it to develop in this direction. Based on the discussions on ISRE-L, it seems that some of our colleagues in the humanities are reticent to use the term “science” with respect to their activities claiming that, at least in English, the term is strongly connoted as “natural sciences”. This is historically true, of course, but again, I think this is an undue perseveration of a connotation that has lost its raison d’être (given the proliferation of labels such as “social and behavioral sciences”, “literary science”, etc.).

Another one of your (many!) research interests is Emotional Intelligence. Since this issue of Emotion Researcher is devoted to EI, I thought I should ask you what you think of the ongoing controversies over whether the construct has scientific validity and is predictively relevant.

Much of the copious EI research in past years has, to my mind, started from misconceived preconceptions, focusing too strongly on the intelligence or the personality/adjustment model. I have suggested to conceptualize and measure EI as “emotional competence” (EC) involving ability, genetically endowed or acquired in the course of socialization, and skills that can be learned in a relatively short lapse of time (Scherer, 2007). The concept of EC should not only be compatible with widely accepted theories of emotion; it should in fact be directly based on theoretical models of the emotion mechanism and the available empirical evidence.

I propose to use the CPM as the basis of competence conceptualization, as it is one of the broader theories, encompassing many of the pertinent components, including cognition, and highlighting the dynamic nature of the emotion process. As key constituents of EC, I suggest 1) *appraisal competence* (evaluating pertinent events in an accurate fashion with respect to both the personal implications of the events and one’s ability to cope with the consequences), 2) *regulation competence* (the capacity to react in an appropriate fashion both with respect to promising action tendencies and situational contingencies), and, 3) *communication competence* (the ability to produce emotion signals in accordance with strategic aims and cultural norms and to correctly infer the emotions of others on the basis of outward expression and to empathize with others under appropriate circumstances). Our own work has mostly focused on emotion recognition as part of the communication competence constituent (e.g., Bänziger, Grandjean, & Scherer, 2009; Schlegel, Grandjean, & Scherer, 2014)

A high level of competence in these three key constituents ought to produce adaptive outcomes; in other words, high EC persons are likely to be resilient in stressful situations, unlikely to suffer from emotional disturbances, and likely to enjoy a high level of life satisfaction. In consequence, high EC should correlate at least moderately with a number of affect-related personality traits and measures of emotional stability. In consequence, it is probable that EC contributes to adaptation to real-world social environments (for a nice nomological network see Schlegel, Grandjean, & Scherer, 2013). However, it would be erroneous to deduce from this correspondence that it is sufficient to measure personality and satisfaction constructs with the help of self-report, as it is generally not advisable to measure outcomes in order to understand determinants.

Most importantly, apart from the serious biases and artifacts affecting self-report, EC is only one determinant of adaptive adjustment and the degree of contribution to professional success and well-being is likely to vary over individuals. If one wants to improve EC through appropriate training, it is essential to diagnose EC directly by assessing emotional performance in order to identify the sources of insufficient competence and to suggest appropriate remedial training. More on our activities and some tools that we share with other researchers can be found on our new EC webpage (<http://www.affective-sciences.org/ec>).

You have been critical of psychological constructionism, the recent wave of theories that reject discrete approaches and offer a ‘core affect’ based account. What are your main critiques of psychological constructionism?

I have discussed my misgivings about the constructionist approach in several places. Here is a brief summary in four points:

(1) As shown in Figure 1, the CPM model makes it clear that feelings are just one of the components of the emotion process (albeit an important one, as feelings monitor the emotion episode through integration and representation of changes in the other components). Furthermore, verbal labeling is an additional, and not even necessary, step.

In some of the writings by psychological constructionists, there is a tendency to blur the distinction between emotions, feelings and verbal labeling. For example, Lisa Barrett (2012, 420) writes with respect to anger, “[i]f some people do not have a concept of anger, then [a] constellation [of components such as a scowl, blood pressure increase, and a feeling of offense] will never exist as anger for those people (i.e., it is not that they are truly angry and don’t know it)”. On my view, we should distinguish between anger, the feeling of anger and the categorization of oneself as angry. These distinctions are admittedly present in the writings of other psychological constructionists (e.g. Russell distinguishes between being angry and having the meta-experience of anger once the concept of anger is applied). My point is that failing to clearly distinguish between ‘emotions’, ‘feelings’ and ‘categorized feelings’ muddles important conceptual distinction and leads to rampant confusion.

(2) Psychological constructionists claim that “core affect”, a point in a low dimensional valence x arousal space, is a central psychological “primitive”, and that it is a core ingredient of prototypical emotion episodes. It is true that people can reliably describe their feelings on these dimensions but this is easily explained by a projection from a high-dimensional qualia space to a lower dimensional space, especially if provided with explicit valence and arousal scales. It is not clear in what sense and why valence and arousal feelings are considered as more “core”, “primitive”, or “basic” than other internal representations (for example, of appraisal configurations).

These dimensions are not verbalized spontaneously. We asked a quasi-representative sample of the Swiss population what emotion they experienced on the previous day. They described the situation and labeled their subjective experience, their feelings, in their own words. Only a very small percentage of the more than 1000 respondents used general or positive valence labels (5.8%), and almost none used direct arousal terms (Scherer, Wranik, Sangsue, Tran, & Scherer, 2004). Apparently, the low-dimensional description does not spontaneously come to mind – although this is what one would expect if it were a primitive.

Maybe Russell and Barrett mean the word “core” in the sense of the most important dimensions in low-dimensional space which they take to be valence and arousal. However, our recent work on the semantics of emotion terms, using a theoretically anchored feature profile based similarity assessment, yields *four* reliable factors in over 25 languages, with arousal coming in only third, after valence and a control/mastery/power factor (Fontaine, Scherer & Soriano, 2013). It seems reasonable to assume that control/mastery/power are very prominent criteria for adaptive responses and should be part of a primitive or core feeling read-out. The same is true for unexpectedness or novelty which weighs in as the fourth factor. Indeed, there is an enormous amount of literature showing the “basicness” and “primitivity” of novelty detection in all organisms. Why should this central factor in perceiving and evaluating the world not be represented in “core affect”?

Furthermore, constructivists do not spend much effort to describe the mechanisms whereby core affect is produced, except providing a laundry list of factors possibly involved in this process, including appraisal. However, as far as I can see, to date no specific, experimentally testable hypotheses or mapping rules, comparable to the appraisal based predictions, have been generated by constructivist theory. It is thus not clear how core affect is differentiated. This omission is particularly worrisome, as an enormous amount of information needs to be compressed and integrated to yield a single point in low dimensional space, reflecting only evaluation (valence) and (arousal) response information. How does this work? One also wonders what happens to the large number of factors that are supposed to influence core affect. Are they not represented in feeling space or do the representations get lost once projection into low-dimensional space has occurred? And would those not be important to fine tune the adaptive action?

It is equally unclear how, out of the constant flux of valence by arousal variation, an attribution to an object or the assignment of a conceptual category occurs. As core affect is supposedly primitive and primary, waxing and waning, there must be some quality of core affect, a threshold or another criterion, that triggers the attribution and categorization processes. What are these? It cannot be the evaluation of the objects or events, because if it were, it would not be clear how the theories differ from appraisal theories, except for an underspecification of the respective mechanisms.

(3) I have often been unable to find concrete predictions, susceptible to targeted experimental testing and falsification, in constructivist writings. Barrett (2009, p. 1294) claims that her conceptual act model generates “four specific hypotheses that are currently being operationalized and tested in the lab”, namely 1. There are psychological primitives (e.g., core affect), 2. Emotions are like books of recipes (not mechanisms) with psychological primitives as elements in a well-stocked pantry that can be used to make any number of different dishes, 3. Since emotional primitives partake in both emotional and non-emotional processes, emotion, cognition and perception are not distinct in kind, and 4. Emotion words are powerful in affecting experience. While the latter can be taken for granted, there are serious issues with the first three. It is difficult to consider these as “specific hypotheses”. How can they be falsified, especially as much seems to rest on definitional matters? What does it mean that emotion, cognition and perception are not distinct in kind? And how can this be tested in the lab?

(4) The tendency of constructionism to avoid committing to specific causal mechanisms is no accident: it results from the programmatic attempt to make each emotion episode the result of a unique, on-the-fly, idiosyncratic, in-

dividual construction. Although it is certainly the case that the categorization of immensely variable qualia feeling involves many idiosyncratic features that will be difficult to predict and examine empirically, the abandonment of the nomothetic approach threatens to lead to the abandonment of theory-guided empirical investigation, the hallmark of a scientific approach. Barrett (2009) uses the metaphor of a “book of recipes” for her theory. This is helpful, but recipes generally imply rules for combining ingredients, not free construction. And these rules can be investigated: The proof of the pudding lies in the testing.

You have been exceptionally productive as a scholar and exceptionally adept as an editor, program builder and administrator. Just to mention a handful of your many achievements, you have published over 200 scientific papers and over 100 book chapters, co-founded ISRE, co-founded the Consortium for European Research on Emotion (CERE), co-founded the journal Emotion, founded and co-edited the Emotion and Social Interaction series for Cambridge University Press and the Affective Science series for Oxford University Press, and founded and successfully administered the Swiss Center for Affective Sciences from 2005 to 2013. How on earth did you manage to do all this? What is your daily routine? Do you have any organizational secrets to share with the rest of us?

No secrets. I like research, especially theoretic elaboration of plausible mechanisms and subsequent empirical testing. I work hard. And I had and have many excellent collaborators.

If you compare the incentive structure for scientists 30 years ago and today, do you think things are better off now? What would be your advice for young scholar who want to become successful emotion researchers?

The good news is that finding pertinent literature has become much easier with web searches and PDF downloads rather than scouting around in libraries. And data analysis and manuscript processing is a cinch with the software available now (when I did my thesis at Harvard I had to bring a box of punched cards to the computer center in the evening and come back for the results the next morning, often only to find that I had made an error on a control card...). The bad news is that there is much more literature these days and it is often difficult to know what is really relevant. And then there is the unhealthy pressure to publish a lot, fast, and only spectacular results with wide appeal— which does not necessarily improve quality. My advice: Find a topic you are really enthusiastic about. Try to approach it from a theoretical vantage point or at least elaborate the mechanism that you expect to drive the phenomena you study. And brush up your competence in methodology.

What are your hobbies?

Although it is part of my work, I love statistical data analysis. It is so satisfying to search for interesting patterns. I also like to travel, particularly exploring historically and artistically interesting towns, churches/temples, and museums. As to outdoor activities, I am not much of sports person but I love hiking, particularly in forest and mountains. I always liked to go to the theater, but I must confess that good new plays are few and far between and most classic plays are butchered by egomaniac directors. The same pattern is increasingly found in opera, unfortunately. What remains is pure music!

You now live in Geneva, Switzerland. What do you like and what do you dislike about the city? What are a handful of your favorite restaurants in Geneva (this may come handy for attendants to ISRE 2015!)? Do you enjoy cooking, and if so do you have a favorite recipe to share?

Geneva: Like – its location near the mountains and easy to reach France, Italy, and Germany; its cosmopolitanism despite remaining basically a small city; the rural area and villages around, great for easy hiking. Dislike – the traffic! Below is a short list of restaurants in Geneva. It comes from my address book where I have kept the addresses of restaurants we have been to several times and where we may go again. These are not cheap but also not expensive restaurants – for Geneva, where restaurant food (and everything else) is more expensive than elsewhere.

- Restaurant du Parc des Bastions, 310 8666 , nice terrace in a park
- Le Lyrique 328 0095, on Place Neuve, crowded at lunch and especially on opera and concert evenings, very Genevan atmosphere on those nights

- Café des Négociants 300 3130, on tram 12 in Carouge, spin off of a Michelin starred restaurant (Châteauvieux),
- Olivier de Provence 342 0450, close to tram 12 in Carouge, go to the bistrot part, nice atmosphere on the terrace
- Perle du Lac 731 7935, on the lake, with a nice terrace, beautiful view, good food, quite posh. Bistrot less interesting dishes but more affordable prices

For all of these restaurants it is best to make reservations. And each phone number starts with 022. You will find restaurant descriptions and ratings here <http://www.resto-rang.ch/>. The website is in French and English.

Cooking: I have never done any serious cooking (I don't think I have the knack) but I am very good at appreciating and criticizing food. If forced to I could perhaps manage to prepare a dish that I like very much and that my wife has served to guests for a very long time now:

Papillote de poisson à l'aneth

Ingredients:

- Per person about 150 gr. of white firm fish (sole, monkfish or others), cut into pieces
- 1-2 mushrooms, sliced thinly
- ½ tomato, skin and kernels removed, sliced into small cubes
- a few tiny pieces of shallot
- a small piece of butter
- salt, pepper
- a little dry white wine
- a piece of aluminum foil

Procedure:

- Lay out the foil
- Arrange the fish on the foil lengthwise
- Distribute the butter in pieces over the fish
- Salt and pepper
- Sprinkle the shallot pieces over the fish
- Arrange the mushrooms and the tomato on top
- Salt and pepper again
- Fold the foil over the fish
- First close the long side, then one of the short sides
- Fold the other short side over the packet so that you can open it again
- Put the packet into the fridge
- Prepare a sauce
- Boil down (at medium to low heat to avoid spluttering) some cream (normal or double), in the end you should have 2 to 3 tablespoons per serving
- Chop some dill, reserve it for later
- Season the sauce with salt, pepper, and lemon juice
- Heat the oven to 180 degrees Celsius

- Pour wine into the packet and close the fourth side
- Put into oven for 10 to 15 minutes depending on the thickness of the fish and your Preference for medium or well done, open one of the packets after 10 mins to check
- If you serve the dish to friends you can now sit down with them and relax
- Heat the sauce, stir in dill just before serving
- Put packet on warm soup plate,
- Open it and shove the contents into the plate, the dish has the consistency of “poisson à la nage”, “swimming fish”
- Pour a little sauce over it and eat with a spoon

Bon appétit

What are you working on these days?

The main effort of my current work is directed at empirical testing and further development of my CPM model. Much of the past work has been devoted to empirically demonstrating the predicted effects of appraisals on other components, using laboratory experimentation. Currently, we are putting an emphasis on defining and analyzing component synchronization or coherence in the data sets we gathered over the years. In addition, we are trying to empirically assess appraisal bias as a disposition, especially with the perspective to identify risk factors that can account for the difference between normal and abnormal emotions, particularly depression. Another topic generated by the CPM is the effort of refining the concept of emotional competence and developing appropriate tests. I am also continuing work on facial and vocal expression and we are currently using path analysis to testing a comprehensive model of emotion expression and recognition. Apart from this, I am working with philosophers, literary scientists, and musicologists on the definition and assessment of aesthetic and epistemic emotions and developing tools and running studies on the mechanisms underlying emotion induction by music

Please list five articles or books that have had a deep influence on your thinking

Roger Brown – Words and things, Glencoe: Free Press, 1958

Charles Darwin – Expression of emotion in man and animals, London: John Murray, 1872

Leonard Berkowitz – Aggression, NY: McGraw Hill, 1962

Neil Smelser – Collective behavior, Glencoe: Free Press, 1958

Lazarus, R. S. Emotions and adaptation: Conceptual and empirical relations. In W. J. Arnold (Ed.), Nebraska Symposium on Motivation (Vol. 16). Lincoln: University of Nebraska Press, 1968.

What do you think is the main question that future affective science should be focusing on?

I don't think that there is one main question. The phenomenon of emotion is incredibly complex as it integrates so many organismic systems and functions, involving many different social contexts. In consequence, the issues need to be attacked from several angles, using many different approaches, involving different disciplines and sub-disciplines. The problem is that at some point the results of all of these efforts need to be integrated and in order to do this, one needs a comprehensive perspective that allows to accumulate the findings and to relate them to each other. Unfortunately, that common perspective is exactly what is missing at the moment, given that we can't even agree on a convergent definition. I believe that the challenge will be to focus on the *mechanisms* of emotion and to adopt a perspective that highlights *recursive processes*!

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Disgust, Racism and the Moral-Conventional Distinction

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Introducing Empirically Informed Philosophy of Mind

March 2015 – The bulk of my work on emotion thus far has focused specifically on disgust, but my interests and approach are more eclectic. I am a philosopher of mind and moral psychology with a decidedly empirical orientation, and am interested in understanding distinctively human minds, emotions, and activities from a broadly naturalistic point of view, and then thinking through the philosophical and ethical ramifications of that understanding. I'm also particularly attracted to the perspective provided by evolutionary theory, and the light it sheds on the ways humans are continuous with other animals, as well as how we and our minds are unique.



The discipline-spanning character of evolutionary theory also fits well with my impulse to integrate. I often find myself searching for points of contact and common ground between different insights and theoretical approaches found in philosophy, psychology, anthropology and other areas of research, and looking for ways in which they can be synthesized to paint a more complete picture of the Who We Are and How We Got Here and to formulate and address pressing questions about What It All Means.

Obviously this all covers a lot of ground, and in practice taking this interdisciplinary angle can be demanding. For instance, doing it right requires that one be conversant in each of the different disciplines on which one is drawing, and able to competently navigate their proprietary concerns, methods, and dialects. It also means taking on the typical dangers associated with pursuing breadth rather than aiming primarily for a narrower depth. But I believe that this kind of integrative and interdisciplinary research is crucial, and so worth the effort and risk.

I also believe that philosophers, with their distinctive training on theoretical foundations, conceptual precision, and argumentative clarity, are particularly well equipped to make exactly these kinds of contributions. Indeed, many of my early influences were exemplary practitioners of exactly this style of research (especially Dennett 1995, Simon 1996, Griffiths 1997, Mallon and Stich 2000) and I have tried to follow their example in my own work on social norms, disgust, and implicit bias.



Norms and Emotions: Questioning the Moral/Conventional Distinction

As a graduate student I was lucky enough to become a founding member of the [Moral Psychology Research Group](#), and thus had the opportunity to think about and participate in some of the most exciting and important work in that thoroughly interdisciplinary field as it began to take off – and emotions were center stage (see [Doris et al 2010](#)). Of particular note was a tradition of experimental work initiated by developmental psychologists suggesting that there exists a crisp, stable, psychologically important and culturally universal distinction between moral and merely conventional norms (Turiel 1983, Nucci 2001).

In this literature, examples of prototypical moral norms included injunctions against pulling someone else's hair or stealing someone's scissors, while examples of prototypical conventional norms included rules specifying where to sit during class, or whether it is okay to wear pajamas to school. The research suggested that experimental participants at least tacitly draw a moral/conventional distinction. Violations of moral norms are typically judged to be authority independent, applying generally and to everyone, comparatively serious, and justified by appeal to harm, justice or rights.

Violations of conventional norms, on the other hand, are typically judged to be authority dependent, applying only

in certain situations or to certain groups of people, comparatively less serious, and justified by appeal to factors other than harm, justice or rights (such as, for instance, the need for organization and coordination, the smooth running of the classroom). In addition, empirical research also suggested what kinds of violations tend to elicit the two kinds of responses: norms whose violations result in harm, unfairness or the infringement of someone's rights elicit the moral response, while norms whose violations did not result in harm, an unfair outcome, or the infringement of anyone's rights elicit the conventional response. Finally, initial cross-cultural research suggested that these generalizations about moral cognition emerge relatively early in development, and hold universally rather than being parochial to a certain age, group, religion, or culture.

This work was grabbing the interest of many philosophers, perhaps most notably Shaun Nichols, whose *Sentimental Rules* view (2004) posits a psychological explanation of the results of experiments on the moral/conventional distinction. Nichols holds (very roughly) that the moral response is the result of the perceived transgression of a social norm together with an emotional response to that violation, while the conventional response is the result merely of the perceived transgression of a social norm, without any accompanying emotional response.

However, in a series of papers, my co-authors and I argue that the picture painted by the experiments on the moral/conventional distinction is misleading, and that the generalizations drawn from these experiments are in fact false, and so Nichols' emotion-based explanation of those generalizations may be on the wrong track as well (Kelly et al. 2007, Kelly and Stich 2007, Stich et al 2009). Indeed, my own skepticism was hatched when I noticed that many counterexamples to and complications for theorizing about the putative moral/conventional distinction arose from cases that elicited disgust. For instance, work by Jon Haidt and colleagues (1993,

2006) shows that many people give the moral response to norms whose violation elicits disgust, even though no one is harmed, no injustice is committed, and no rights have been violated; examples include masturbating with a dead chicken, eating the family dog, cleaning the toilet with the national flag, and consensual sibling incest.

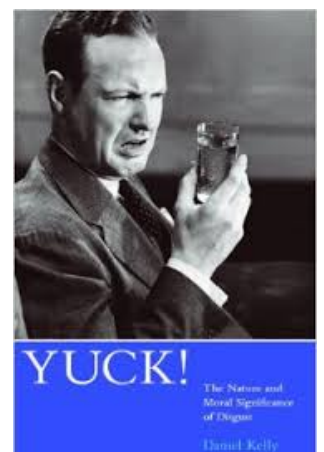
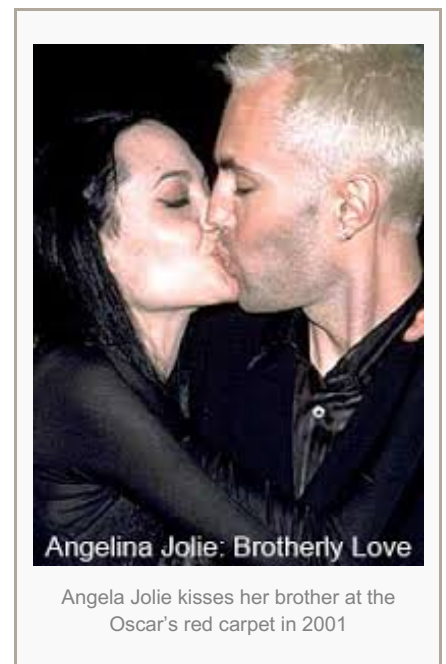
Understanding Disgust

As mentioned earlier, I initially became interested in disgust by considering the different ways in which it had been found to influence moral judgments and social norms. As I looked further into the matter, it seemed to me that disgust had many other intriguing features, and that it was an emotion whose time had finally come – after years of waiting in the wings it was finally attracting the attention of scores of researchers. Disgust also presented exactly the kind of topic I was looking for, one whose many facets required a thoroughly interdisciplinary perspective to fully explain.

Part of what made disgust intriguing was that experimental data seemed to be coming in faster than theory was being developed to account for it.

There were a number of competing theoretical proposals in the air, and it wasn't clear which was correct or whether any were compatible with each other. To the extent that there was a received view, it was probably Paul Rozin's. Rozin holds that although disgust is different from, and more conceptually sophisticated than, mere distaste – one can be disgusted by things that one had never tasted, or has no intention of ever putting in one's mouth – disgust is primarily an orally based emotion. He also holds that cultural evolution broadened the scope of the emotion until it also came to function in a number of other domains. For instance, he showed that it can be triggered by reminders of our animal nature like corpses and blood (Rozin et al. 2000).

Alternatively, another rising school of thought saw disgust simply as the human version of the kind of behavioral immune system found in a number of other animals, helping to prevent infection from contagious pathogens by monitoring for, and producing aversion towards, likely sources of disease (see Curtis et al. 2001 and 2004 for arguments and data supporting this view). Together with the research connecting disgust to social interaction and moral judgment discussed



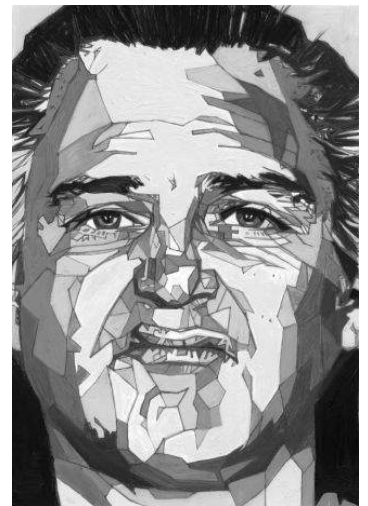
above, a more complete view of a surprisingly complex emotion was emerging.

In my work, I attempt to articulate a broader theory that can capture the evidence and insights supporting such views and to show how the different approaches each have an important piece of a complicated puzzle. At the heart of my theory is a dual origins account of the evolutionary history of disgust that I call the *entanglement thesis* (see Kelly 2011 chapter 2). I draw on a wide range of evidence ranging from anthropology, through developmental, evolutionary, and comparative psychology to support the claim that the emotion was initially formed in our phylogenetic past when two initially distinct cognitive mechanisms – one dedicated to monitoring food intake, the other to protecting against infectious diseases – became functionally entangled to form the single emotion we now call disgust.

I argue that mechanisms performing functions similar to each of these can be found in a wide range of other animals, but that in those other animals they are not merged in the way they are in human beings; thus, there is indeed a sense in which disgust is a uniquely human emotion (as other theorists have previously claimed (Miller 1997, also see Rozin et al. 2000)).

Another intriguing aspect of disgust is that it appears to include features that are universal and largely innate, on the one hand, while other aspects of the emotion appear to be sensitive to social influence and exhibit significant cultural variation, on the other. A central example of a universal feature is *contamination sensitivity*: when we are disgusted by something, we also tend to become disgusted by whatever else that thing comes into contact with.

Other examples of universal aspects include the capacity to naturally recognize the disgust response in others, particularly the associated facial expression often called the gape, and the tendency to become disgusted by a core set of innate disgust elicitors, typically cues that are highly correlated with the increased risk of infection (coughing, open sores, blood and other bodily fluids, decomposing organic material) or the likelihood of causing gastrointestinal distress if consumed (spoiled milk, rotting meat, moldy fruit). However, beyond that core set of innate elicitors, individuals can learn to be disgusted by additional things as well, and individuals raised in different cultures typically learn to be disgusted by different things. Even in the case of food, what is considered a delicacy in one culture is often considered revolting by members of other cultures (even if they haven't tasted it!): prominent examples include Australia's vegemite (a spreadable paste made from vegetable and spice additives), France's escargot, Japan's sushi, America's deep fried baconnaise.



Many cultures find forms of “deviant” sexual behavior both disgusting and wrong, but what falls within the range of “deviant” behavior exhibits considerable variation from one culture to the next. More generally, different cultures have their own norms regulating rites of passage, proper behavior in ritual settings, acceptable ways of interacting with members of different classes, castes, or cultures, and many other social and moral issues. Purity norms like these delineate the culture's “way of life”, and violations of them are considered not just wrong but also often disgusting – morally contaminating and spiritually polluting (Rozin et al 1999). Here again we see significant variation in what elicits disgust, this time in the social and moral domain.



Another aim of my book is to explain this type of variation, and sketch the psychological mechanisms responsible for producing it. Here I first turn to recent work on research on facial expressions. In short, I canvass recent evid-

ence that disgust recognition is *empathic*: we easily “catch” the emotions expressed by those around us, and in recognizing that someone nearby is disgusted by something, we ourselves are primed to be disgusted, sometimes actually coming to be disgusted ourselves.

Placing these insights within the context of recent work on cultural evolution, I argue that they reveal an instinctive, non-verbal signaling system that gives disgust an important kind of flexibility, showing how the emotion can admit of the sorts of cultural variation described above. The signaling system allows us to socially transmit information to one another about what is disgusting and important to avoid in local environments, so an individual can easily learn what it is appropriate to be disgusted by in whatever culture they are raised. Thus, differences between cultures about what is considered disgusting can accumulate over time as they are socially transmitted from one generation to the next (see Kelly 2011 chapter 3,).

While disgust originates in and remains closely attuned to the concrete realities associated with poisons and parasites, much of the experimental work cited earlier suggests that it is able to exert influence, sometimes covertly, other times more openly, over certain aspects of social and moral cognition. Though many details remain to be learned in this area, I offer a broad theoretical framework through which such findings might be interpreted. I first develop the gene cultural co-evolutionary account of uniquely human tribal social instincts, with special attention to the central role it gives to our capacities to socially learn and comply with the social norms of our group, and the corresponding importance we assign to signaling group membership and monitoring others' behavior for commitment to the group and compliance with its norms (Richerson and Boyd 2001, Boyd and Richerson 2005).

I then argue for what I call the *co-opt thesis*: as human social life became more complicated, disgust was recruited to provide the motivational component associated with certain tribal social instincts (see Kelly 2011, chapter 4 and Kelly 2013). I point out that its signaling system and susceptibility to social influence made it easily exploitable for these new purposes, and thus disgust acquired auxiliary functions related to certain types of social norms, the construction of social identities, and the monitoring of group membership and tribal boundaries. Moreover, in those cases where disgust is the emotion recruited to provide motivation, activities forbidden by the social norm will be avoided because they will be considered not just wrong but also disgusting, and violators of the social norm can become objects of disgust themselves, morally contaminated by their actions. Likewise, motivation to avoid interactions with members of certain outgroups can come to be infused with disgust, and as a result those individuals and the very symbols of their group can come to be considered tainted and polluting.

Finally, I have used this empirically and evolutionarily informed account to provide new theoretical foundations for conclusions about the value of disgust in moral deliberation and justification. More specifically, I have argued that as vivid and aversive as feelings of disgust can be while one is in their grip, there is no reason to think that the emotion is morally “wise” or that it is an intrinsically reliable guide to the moral status of the practices and actions that might elicit it. In other words, merely finding some behavior disgusting is itself never a good reason to think that behavior is morally wrong. Rather, given what we now know about its evolutionary history, psychological functioning, and capacity for variation, we should be skeptical about the idea that the emotion is uniquely sensitive to genuine ethical boundaries, and should do what we can to minimize its role in our social and legal institutions. (Kelly 2011 chapter 5, Kelly and Morar 2014).

Racial Cognition and Implicit Bias

A final strand of my research centers on issues of race and racism where they intersect with psychological work on racial cognition and implicit bias (see <http://biasproject.org> for further information). The strategy my co-authors and I pursue begins by considering recent empirical research about racial cognition having to do with, for instance, how humans learn racial categories, how we intuitively assign racial membership, and how racial biases can take both



explicit and implicit form.

We explore the ways in which such findings interact with issues discussed in the philosophical literature on race. These include issues concerning whether or not racial categories should be eliminated or preserved in an ideal society (short answer: it depends, [Kelly et al 2010a](#)) and whether or not psychological explanations depict racism as inevitable (short answer: no, [Machery et al. 2010](#), [Kelly et al. 2010b](#)). We also show how social constructivist accounts of race and institutional accounts of racism can be strengthened by incorporating insights from empirical psychology ([Mallon and Kelly 2012](#)). Finally, my most recent work in this vein has considered the putative problems implicit biases raise for moral responsibility, especially the fact that those biases can operate automatically and outside the awareness of people who harbor them, and can be diametrically opposed to their considered values.

In response to these problems, my coauthors and I argue that since implicit biases are still knowable and controllable, albeit in non-traditional ways, behaviors driven by them still fall squarely within the realm of moral assessment. We elaborate upon and defend this idea by showing how common norms governing praise and blame can be reasonably extended to apply to such behaviors, and argue that we can and should take responsibility for the effects of implicit biases ([Holroyd and Kelly, forthcoming](#), [Washington and Kelly, forthcoming](#)).

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