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Does Leadership Still Not Need Emotional Intelligence?

Continuing “The Great EI Debate”

Abstract

The study of emotional intelligence (EI) in the field of leadership, and in the organizational sciences in general, has often been characterized by controversy and criticism. But the study of EI has nonetheless persisted by developing new measures and models to address these concerns. In a prior letter exchange by Antonakis, Ashkanasy, and Dasborough (2009), two author teams debated the role of EI in the leadership literature, but also set an agenda for research and reconciliation for the future. The present exchange revisits these arguments using evidence accumulated over the past decade. Specifically, the authors debate not only the evidence for the predictive power of EI for workplace outcomes, but also the validity of EI as a construct, the measurement of EI, and the appropriateness of analytical tests for establishing the value of EI. Although the author teams agree on the value of the study of emotions and the need for rigorous research in this area, they nonetheless propose alternative agendas and priorities for the future. Further, they conclude that the issues identified in this exchange are not unique to the study of EI; but should also serve to inform the study of other personality factors and leadership more broadly.

Keywords: emotional intelligence, debate, leadership, meta-analysis

Letter 1: Marie Dasborough, Neal Ashkanasy, & Ronald Humphrey

to Peter Harms, Marcus Credé, & Dustin Wood

Dear Peter, Marcus, and Dustin:

Much has happened in the decade that has passed since the last emotional intelligence (EI) letter exchange published by Antonakis, Ashkanasy, and Dasborough (2009) in *The Leadership Quarterly* (LQ). It is therefore our pleasure to revisit the debate with a new co-author (Humphrey) and new team of adversarial collaborators. Engaging in a scholarly debate over EI as a construct (and whether leaders need to possess it), was an invigorating endeavor back then and we believe that it will be once again. We hope that this series of letters will not only re-stimulate discussion on important issues surrounding EI in the field of leadership, but also bring light to the new developments that will promote further research on leader EI.

1. A More Mature Field of Study

Antonakis asked in the 2009 debate for us to be open to the idea that "EI might one day go the way of the *Raphus cucullatus*, the dodo bird, destined for extinction" (Antonakis et al., 2009, p. 247). In his opening arguments, however, he also stated that, despite his concern about the "flimsy evidence," he was "open to the idea of new conceptualizations of intelligence" (p. 247). Antonakis expressed concern about the lack of incremental validity (above Big Five personality and cognitive intelligence) for measures of EI. Looking back on the research available in 2009, we would have to agree with him that much of the evidence for the validity of EI at that point in time was probably "flimsy." As such, Antonakis's criticism was tough medicine to bear. Nonetheless, this medicine was a useful tonic for EI researchers, and it encouraged us and other colleagues to collect better empirical evidence.

Early on – as the field was emerging – it was rare to see any mention of EI in the top journals, and many well-cited articles were theoretical (e.g. George, 2000; Huy, 1999; Jordan, Ashkanasy, & Härtel, 2002). Following publication of Antonakis et al. (2009) however, more empirical studies began to enter the top journals within the fields of management, organizational psychology, and organizational behavior (e.g. Dong, Seo, & Bartol, 2014; Farh, Seo, & Tesluk, 2012; Law, Wong, & Song 2004; Parke, Seo, & Sherf, 2015). Moreover, the expansion of empirical publications also enabled scholars to conduct meta-analyses of EI and its effects in organizational settings (e.g. Joseph & Newman, 2010; Miao, Humphrey, & Qian, 2018; O'Boyle et al., 2011), which provide further evidence that the field has matured since the letter exchange in 2009.

In addition to the growing empirical contributions, other evidence that the field has matured comes from reviews of the EI literature (e.g. Ashkanasy & Dasborough, 2015; Côté, 2014) and reviews of EI measures (Miners, Côté, & Lievens, 2018). We have also witnessed a burgeoning stream of articles re-examining how EI is conceptualized (Elfenbein, & MacCann, 2017; Mayer, Caruso, & Salovey, 2016; Mestre, MacCann, Guil, & Roberts, 2016; Petrides, Mikolajczak, Mavroveli, Sanchez-Ruiz, Furnham, & Pérez-González, 2016; Ybarra, Kross, & Sanchez-Burks, 2014). Clearly, the picture has now changed since our prior letter exchange in 2009. We argue in this letter that EI, nurtured by growing evidence for its validity, has now matured into a *bone fide* field of study.

2. Conceptualizations and Concerns

Before we investigate new evidence for the claims, it is important for us to distinguish the different conceptualizations of EI. When discussing the empirical research, we will use the categorization of EI measures developed by Ashkanasy and Daus (2005). They argued that there are three “streams” of EI research and measurement. *Stream 1* includes ability measures based on objective right or wrong questions (similar to cognitive intelligence tests), such as the MSCEIT (Mayer, Salovey, Caruso, & Sitarenios, 2003). Stream 2 represents self- and peer-report EI measures based on Mayer et al.’s theoretical model (e.g., Jordan, Ashkanasy, Härtel, & Hooper, 2002; Schutte et al., 1998). Stream 3 involves “mixed” emotional competency measures that include a variety of emotion related skills and competencies (e.g., Bar-On, 1997; Goleman, Boyatzis, & McKee, 1997). Ashkanasy and Daus also argued that the scales in each of the three streams measure separate and distinct constructs and should not be confused with each other. Their review has since been highly cited (>800 citations on Google Scholar at the time of writing), and most Stream 2 and 3 EI researchers now recognize that the three streams are distinct constructs and use different names, such as trait EI or emotional and social competencies (ESC), to refer to their measures.

One concern with the Stream 2 and 3 EI measures is that they may overlap with the Big Five personality measures. Critics of EI argue that these measures of EI could owe their predictive power to this overlap, rather than to unique characteristics of EI. In the original debate article (Antonakis et al., 2009), Antonakis forcefully (and correctly) argued that measures of EI need to demonstrate, “incremental validity: the litmus test of validity” (p. 249). He further argued that studies needed to use rigorous research designs; for example, they need to avoid using same source data for both independent and dependent variables, which is a source of common methods bias (Podsakoff, MacKenzie, & Podsakoff, 2012). In terms of incremental validity, Antonakis argued specifically for controlling for Big Five personality and cognitive ability.

Since then, scholars have made substantial progress in demonstrating the incremental validity of EI. In addition, a newly developed measure of Stream 1 ability EI, by Schlegel and Mortillaro (2019), shows considerable promise in terms of incremental validity. Over time, we anticipate more evidence supporting the incremental validity of this new test to emerge.

3. Growing Evidence for the Claims about Employee Performance Outcomes

To revisit this issue of incremental validity, we rely largely on the results of recent meta-analyses where authors used “relative importance analysis” (Johnson & LeBreton, 2004) to establish if EI effects extend above and beyond the effects of Big Five personality and cognitive intelligence on employee performance outcomes. Researchers often look at beta-weights when comparing the importance of predictors in regression models. Johnson and LeBreton point out, however, that these are only valid indicators of relative importance when predictors are uncorrelated. To enable the accurate interpretation of relative importance, these authors developed a statistical technique to calculate the relative importance of correlated predictors, called relative importance analysis. The technique reveals what proportion of the explained variance is accounted for by each variable, thus allowing a simple and intuitive understanding of each variable’s effects. Because of this ability to handle correlated predictors, scholars who employ relative importance analysis can determine if EI measures explain meaningful variance when taking into account Big Five personality and cognitive intelligence. As a caveat, we note that there are differences of opinions about the best way to assess relative importance, and that Braun, Converse, and Oswald (2019) have observed “there are no unambiguous measures of relative importance when predictors are intercorrelated” (p. 594).

We agree especially with Antonakis’s point that cognitive ability is an important predictor of organizational performance outcomes. Nevertheless, the results of meta-analysts based on a wide range of research (Joseph & Newman, 2010; O'Boyle, et al., 2011) indicate that EI does in fact increase the ability to predict individual task performance when controlling for both cognitive intelligence and Big Five personality. Thus, although we do agree with Antonakis's point that cognitive ability is likely the most important factor when it comes to individual task performance, we note that the uncorrected correlations between EI and job performance are roughly in the same range as the uncorrected correlations between cognitive ability and job performance.

In this regard, O'Boyle and his colleagues found that EI (across all measures) correlated .24 with job performance (k = 43, n = 5,795; ability EI, r = .21; self-report r = .26; mixed r = .24). To meet the most rigorous standards, these authors reran their analysis excluding three studies that used self-report measures of job performance and found no differences in the results. These findings are comparable to what Postlethwaite (2011) found in a meta-analysis of the relationship between cognitive ability and job performance. Postlethwaite found that crystalized intelligence correlated .23 with job performance (k = 199, n = 18,619), and that fluid mental ability only correlates .14 with performance (k = 23, n = 3,273). These meta-analytical results resemble those for other studies. For example, a meta-analysis of studies done in the UK found a correlation of .22 between general mental ability and job performance (Bertua, Anderson, & Salgado, 2005), whereas, in a meta-analysis of European studies, Salgado, Anderson, Moscoso, Bertua, and de Fruyt (2003) found the correlation to be .29 (k = 97, n = 16,065).

In order to set an even more rigorous bar for proving the incremental validity of EI, O'Boyle et al. (2011) used a high estimate (.56) of the relationship between cognitive ability and task performance (obtained from the Schmidt, Shaffer, & Oh, 2008, article based on new corrections for range restrictions for mental ability). O'Boyle and his associates still found that self-report EI and mixed emotional competency measures added incremental value to the prediction of job performance when controlling for both cognitive ability and Big Five personality. In addition, they used the Johnson and LeBreton (2004) method to show the relative importance of the different predictors in the model. Results revealed EI to be the second-best predictor in the model when measured using self-report EI or mixed emotional competency measures and was more important than any single one of the Big Five measures.

In a later meta-analysis, Joseph, Jin, Newman, and O'Boyle (2015) found support for the value of ability EI measures to predict job performance (see their Table 5, page 315, and their structural equation model, Figure 1, page 302). What is particularly impressive in this study is that ability EI still relates to job performance after controlling for conscientiousness, emotional stability, extraversion, and general mental ability, plus another measure of emotional intelligence (mixed EI), self-efficacy, and self-rated job performance. Note that the standardized regression coefficient for ability EI in this research was .19, roughly the same size as the coefficient for extraversion (.21), and almost twice the size as the coefficient for emotional stability (.11). Among the Big Five predictors in this model, only conscientiousness had a substantially larger coefficient (.34). To the extent that scholars generally believe that the Big Five relate strongly to job performance (e.g., see Barrick & Mount, 1991), then these results should also lead one to conclude that ability EI is also an important and independent predictor of employee performance.

As Spector and Fox (2010) point out, organizational citizenship and counterproductive work behaviors constitute alternative independent types of behavior at work. In this regard, Miao, Humphrey, and Qian (2017a) conducted a meta-analysis where they examined the relationship of EI and these other two important employee outcomes. Again, to use the most rigorous standards, they excluded studies that used self-ratings to report organizational citizenship behavior or counterproductive work behavior (there was one exception for the incremental validity and relative weight test for stream 3 EI and CWB; because of the small number of studies one self-report of CWB was used). Miao and his team found that, "When controlling for ability measures of EI, the Big Five personality measures, general self-efficacy, cognitive intelligence, and self-rated performance, both self-report measures of EI and mixed competency measures of EI show incremental validity and relative importance in predicting OCB and CWB" (p. 144). Including EI measures increased the R^2 from .15 to .33 for self-report EI and to .62 for mixed emotional competency measures. When examining organizational citizenship behavior, these authors found the relative importance of self-report EI to account for 57.4% of the explained variance; for mixed measures it was 53.2%. These are impressive results in that including the EI measures doubled the R^2.

Tett and Meyer (1993) argue that other organizational variables, although not strictly measures of work performance, are nonetheless important in organizational behavior research. These variables include employee job satisfaction, organizational commitment, and turnover intentions. Again, the meta-analysis evidence (Miao, Humphrey, & Qian, 2017b; 2017c) unequivocally supports the notion that EI relates to these variables over and above the effects of cognitive intelligence and Big Five personality.

4. Growing Evidence for the Claims about Leadership

What about when it comes to leadership? The idea that EI relates to leadership has also been consistently supported in meta-analyses, including in your own research. In this regard, in your recent meta-analysis based on 62 independent samples (Harms & Credé, 2010a), you found a validity estimate of .59 based on studies that relied on self-reports of both leadership and EI. Although you noted that the validity was lower (.12) for studies that employed more rigorous, multi-source data collection also, the relationship is nonetheless statistically significant. Moreover, you also found that validity for transformational leadership was especially strong, ranging from .60 (same source, self-report) to .05 (multi-source, MSCEIT). Notably, you found further that validity for same-source transformational leadership and EI measures using the MSCEIT was .24. This is an especially strong result, because the MSCEIT is an ability measure of EI, not a self-report measure. Although you found lower validity indices for transactional and laissez-fare leadership, this is to be expected – because these forms of leadership do not require a strong people orientation (context matters; see Jordan, Dasborough, Daus, & Ashkanasy, 2010). These effect sizes are also comparable to meta-analytic results reported by Bono and Judge (2004) dealing with relationships between personality and transformational and transactional leadership.

University professors, because of their own perceived high intelligence, often take it for granted that there is considerable evidence that leader cognitive ability influences subordinates in a variety of positive ways. This assumption underlies the demand that EI researchers prove the incremental validity of EI over measures of leader cognitive ability and the Big Five personality measures. Nonetheless, one of the biggest obstacles to demonstrating the incremental validity of EI is the lack of evidence for the role of leader cognitive ability on follower outcomes. In many cases, the evidence for the effects of the leader's Big Five personality traits is also missing.

Perhaps the best evidence for the importance of leader cognitive ability can be found in the meta-analysis by Judge, Colbert, and Ilies (2004). This is perhaps the most widely cited article on leader cognitive intelligence, and we certainly agree with its conclusion that cognitive ability is important to leadership. This meta-analysis found that leader cognitive ability, when measured by pen-and-paper tests, had a correlation of .17 with leadership (see Table 1, page 545

"Overall Relationship between Leader Intelligence and Leadership"). This meta-analysis also reported relationships between leader cognitive intelligence and perceived group effectiveness of .22 (correlation corrected for unreliability in the predictor and criterion and for range restriction). Unfortunately, because the study included both student and employee samples, it is not known exactly what the results are for full-time working adults. Moreover, this study does not report the relationship between leader cognitive ability and a wide range of important subordinate outcomes, such as individual subordinate task performance, subordinate organizational citizenship behavior, and subordinate job satisfaction. To our knowledge, no meta-analyses have documented these effects.

Because it is not always possible to find comparable studies necessary to calculate incremental validity precisely, we focus in the following sections on demonstrating what the main effects of leader EI is on follower outcomes such as individual task performance, organizational citizenship behaviour, and job satisfaction. We also describe the relationship between leader EI and authentic leadership. We feel the main effects for leader EI compare well to effect sizes for other personality variables with regard to similar outcome variables.

Miao, Humphrey, and Qian (2018a) conducted a meta-analysis where they examined the effects of leader EI on follower task performance and organizational citizenship behavior. They found that leader's EI correlated .41 with follower's task performance and .33 with follower's organizational citizenship behaviors. As we noted earlier, employee job satisfaction constitutes another important organizational outcome variable. In this regard, Miao, Humphrey, and Qian (2016) conducted a meta-analysis where they examined the effects of leader EI on subordinates' job satisfaction. They found that leaders' EI correlated with follower's job satisfaction (r = .24).

Another recent meta-analysis found that leader EI is positively related to perceptions of authentic leadership (Miao, Humphrey, & Qian, 2018b). They found that leader EI is significantly and positively related to authentic leadership, with self-report and mixed EI having particularly large corrected correlations (self-report EI: $\hat{\rho} = 0.52$; mixed EI: $\hat{\rho} = 0.54$). This finding is important because other meta-analyses have demonstrated that authentic leadership has meaningfully important correlations with a wide variety of employee behavioral and attitudinal outcomes and is also strongly related to transformational leadership (Banks, McCauley, Gardner, & Guler, 2016).

5. Concluding Comments

In conclusion, the evidence accumulated in the years since publication of Antonakis et al. (2009) now demonstrates clearly that organizational researchers have made great strides in the field of EI and leadership over this period. Since the early article by George (2000), EI advocates such as ourselves argue that this construct is essential for effective leadership. Leadership involves, in addition to goal attainment, "generating and maintaining excitement, enthusiasm, confidence, and optimism" (George, 2000, p. 1039), and emotions are important at each stage of the leader/member relationship (Cropanzano, Dasborough, & Weiss, 2017). In this regard, Caruso, Mayer, and Salovey (2002) point out that (ability) "emotional intelligence underlies a leader's 'people' or 'relationship' skills" (p. 55). Critics of EI and its role in leadership (e.g., see Locke, 2002) maintain, on the other hand, that this view is flawed insofar as leadership is not intended to promote employee well-being, but instead to assist organizations to attain long-term profitability. Locke states that, "Organizations, other than psychotherapy clinics, are not in the 'feel-good' business" (p. 428). The problem with Locke's argumentation, however, is that "long-term profitability" necessarily must involve attending to the full range of employee performance variables, including task performance, organizational citizenship behavior, counterproductive work behavior, as well as associated variables including employee job satisfaction, organizational commitment, and turnover intentions.

It is also becoming clear from the meta-analytic results we have canvassed in this letter that the Stream 3 conceptualizations (and measures) of EI also relate to outcome variables above and beyond the effects of Big Five personality and cognitive intelligence. In this case, it seems the time is right to take a closer look at the relevance and validity of these variables, as well as the other two streams of EI research. This is exactly why it is important for scholars to re-visit this debate in the scholarly literature. Instead of dying out like the "dodo bird", organizational behavior and leadership scholars continue to study EI intensively – and there is more work to be done.

Sincerely, Marie T. Dasborough, Neal M. Ashkanasy, and Ronald H. Humphrey

Letter 2: Peter Harms, Marcus Credé, & Dustin Wood to Marie Dasborough, Neal Ashkanasy, & Ronald Humphrey

Dear Marie, Neal, & Ronald,

We are grateful to have this opportunity to share in this exchange of ideas with you. The prior letter exchange (Antonakis et al., 2009) has been enormously influential on EI-leadership research and your initial letter reiterated what we felt was the main thrust of that prior exchange, that although there had been some positive movement towards testing the validity of EI in the leadership domain, there also remained serious concerns owing to the lack of rigorous research in this area.

A decade later we are not persuaded that we have gotten particularly convincing answers to the questions raised in the original exchange. Although you have provided several examples of forward movement, we cannot help but feel that the overwhelming majority of the body of work produced in the past decade has still fallen short of what we would describe as "robust" science. In this letter we hope to outline our concerns and respond to some of the points you raised. Specifically, we would like to point out concerns that we have with the measurement of EI, problems with meta-analytic reviews of the EI literature, and specific issues with tests of incremental validity. Finally, in acknowledgement that it is always easier to criticize bodies of research than to build them up, we would like to provide our own thoughts on positive potential avenues forward for this literature.

1. Problems with Measurement of EI

Measurement is in many ways, the first, last, and most important concern in science. If we cannot measure the constructs in our theories, then we cannot test them. So, a faithful and precise operationalization of EI is essential for advancing theory and practice. Although we appreciate the efforts you made to clarify the conceptualizations of EI in your letter in terms of categorizing them into streams, we still confess to finding this approach to classifying EI research results to be confounding and counterintuitive, both in terms of the labeling and the over-reliance on the MSCEIT's theoretical model. In both personality and industrial-organizational psychology, there is a distinction made between measures of *actual* abilities, which are often recommended to come in the form of "maximum performance" tests (Sackett, 2007) and self-reports of abilities – exemplified by such differences as obtaining perfect scores on the quantitative sections of the SAT or ACT versus self-reports of being very good at math.. Self-reports of abilities are usually related to actual abilities, but this relationship is more modest than most people think (e.g. Paulhus et al., 1998), because individuals are often unaware of their actual capacities, or may have motivations for artificially inflating or deflating their scores. Such self-appraisals are generally considered to be "efficacies" (Bandura, 1977). Efficacies are, of course, interesting constructs in themselves but few researchers would be taken particularly seriously if they equated, say, academic self-efficacy with actual academic achievement or aptitude. For this reason, it is our contention that many, if not most, of the measures included in your Stream 2 and 3 categories would more appropriately be labeled as emotional efficacies rather than emotional intelligence. Further, on what basis other than longevity does the MSCEIT model assume theoretical primacy in the field? Giving consideration to more comprehensive models of emotional capabilities and interpersonal dynamics offers the potential for making significant advances in measurement and theory (see Schlegel, 2016). Perhaps it is enough to simply categorize measures into emotional efficacies and emotional abilities and leave it at that.

But beyond clarity in labelling and categorization of measures, perhaps an even bigger issue is whether EI measures actually even measure EI. Even advocates of EI have noted this issue. For example, Cary Cherniss has stated that “the models are so different from one another the concept of EI is in danger of becoming meaningless” (p. 113, Cherniss, 2010). Stephan Coté (2014) said that the problem of construct confusion is prevalent in EI research because “some conceptualizations of EI lump together constructs that meet the definitions of its constituent components – intelligence and emotion – with other constructs that do not” (p. 462). And in a review in *The Leadership Quarterly*, Rajah and colleagues argued that “research in the area of emotional intelligence is riddled with measurement issues” (p. 1111, Rajah et al., 2011). To illustrate the problem, consider items from the “Use of Emotion” scale of the widely-used Wong and Law EI Scale: “I always set goals for myself and then try my best to achieve them”, “I am a self-motivating person”, and “I would always encourage myself to try my best”. The first example item is double-barreled, rendering interpretation of its endorsement ambiguous at best (see Furr, 2011; Olson, 2008; Spector, 1992). Further, any self-respecting self-reported ability scale should be expected to consist of items containing stems such as “I’m good at…” “I find it easy to…” “I’m able to…” – that is: to ask people about either the *efficacy* or *efficiency* of performing an action when they try to do so (Wood et al., 2015). Given that, which of these items actually meets the basic face-validity criteria for an EI measure? Other EI scales rarely fare better in terms of limiting themselves to EI-specific content. Example items in the Bar-On EQi (Bar-On & Parker, 2000) include “I like my friends” and “I fight with people”. We can certainly expect liking one’s friends and fighting with people (and more exactly: self-reports of these tendencies) to be *associated* with EI, but we should not regard ourselves as having *measured* EI by measuring its expected outcomes. Similar items that tap into optimism or self-efficacy (e.g., “I expect that I will do well on most things I try” and “I expect good things to happen”) are present in the highly cited measure developed by Schutte et al. (1998). And commercial tests such as the Hogan EQ (Hogan Assessment Systems, 2013) do not even disguise the fact that the items are directly pulled from their standard personality measure. Granted that all of these examples come from self-report, trait measures of EI, but even more sophisticated situational judgment task-based measures such as the Geneva Emotional Competence Test (GECo; Schlegel & Mortillaro, 2019) and the Situational Test of Emotional Management (MacCann & Roberts, 2008) provide sample items that are not much different than assessment of effective problem-solving or time management and correlate substantially with measures of intelligence (Evans et al., 2020; MacCann & Roberts, 2008; Schlegel & Mortillaro, 2019).

Why raise this already well-established criticism of EI measurement (Antonakis, 2011; Conte, 2005)? Beyond the points raised above, we believe that fidelity in measurement is critical because of the potential for misleading results when EI researchers aggregate subscales of EI measures (which is almost always the case) – an approach that is an error for both theoretical and empirical reasons. Elfenbein and MacCann (2017) argue that ability-based "EI is an umbrella model that includes within it multiple narrow abilities that are related and yet distinct" (p. 2). Similarly, Petrides et al. (2016) has argued that trait EI lies at the "lower levels of personality hierarchies" (p. 336). That is, both ability-based EI and trait-based EI consist of multiple distinct elements and should be considered more of a category than a construct. There is no good theoretical or empirical reason why skills such as accurately perceiving emotions in others and being able to control one's own emotions should be conceptualized as reflecting a higher-order, core construct. Moreover, empirical studies testing convergence both within and between EI measures (e.g. Fiori & Antonakis, 2011; 2012; Schlegel & Mortillaro, 2019) suggest that the psychological factors assessed by EI instruments are distinct with little convergence. Because of this lack of convergence, researchers have suggested that until structural and measurement issues with EI measures are resolved, it would be wise to avoid aggregating EI subscales even within the same measures (Lopes, 2016). In sum, the concern is that without paying due attention to what goes into EI measures, we cannot know what we are actually measuring.

This problem gets compounded further when researchers aggregate across meaningfully distinct scales without ever demonstrating that this scoring approach is justified – as might be done via rigorous tests of higher-order factorial models (see Credé & Harms, 2015 for guidelines). And using proprietary scales where the item content is hidden from readers and reviewers makes this problem even worse because there is little chance of outside evaluation. Among other things, it becomes difficult to determine whether the measure of EI predicts outcomes like interpersonal aggression because the EI measure itself contains direct self-reports of engaging in interpersonal aggression! In preparing this comment, we found it distressingly difficult to actually see the content of commonly used EI measures to evaluate these types of concerns, due to their proprietary nature. We are simply left to have faith that the researchers know what they are doing, and that reported "validity" correlations are not driven by these types of tautologies. Unfortunately, we see little reason to grant this trust to behavioral scientists at this point in time, given the frequency with which widely trumpeted associations linking scales to outcomes are later found to be driven by these types of uninteresting tautologies (Mottus, 2016; van Knippenberg, & Sitkin, 2013; Wood & Harms, 2016) The result of all this is that, at the individual study level, we can rarely even be sure of what has been found.

2. Problems with Meta-Analytic Studies

There are individuals who will argue that the problems with EI measurement will "wash out" when scores are combined across scales and studies in meta-analytic reviews. The idea being that the failings and inadequacies of individual measures can be balanced with or compensated for by the strengths of other measures. Our response to this position is that all the issues raised in the previous section continue to apply when aggregating across scales as well. If aggregating across subscales within a given measure is frequently inappropriate, then why would it improve the situation to move further from accurate reflections of distinct psychological characteristics and towards more amorphous, less interpretable variables? We acknowledge that we have committed this very methodological sin in the past (i.e. Harms & Credé, 2010a). However, the gained experience and knowledge of the past decade has meant we would now be reticent to recommend this and would question the meaningfulness of analyses based on meta-analyses that have done so. We are not the only researchers who have come to this conclusion. Mattingly and Kraiger (2019) have argued that the common practice of meta-analyses aggregating across EI measures and models should be questioned because of the lack of convergence. Overall estimates, even within "stream", are simply not meaningful.

In addition, having conducted several meta-analyses ourselves, we are aware of the inherent fallibility of the technique. Although some individuals have argued that choices made in meta-analytic efforts have relatively small implications for the resulting conclusions (e.g. Aguinis et al., 2011), we believe that such choices can and do have a significant impact. Because you used the O'Boyle et al. (2011) meta-analysis on EI and job performance frequently in your letter, we would like to take that as an illustrative example. This meta-analysis was published shortly after another meta-analysis on the same subject (Joseph & Newman, 2010) which came to somewhat differing conclusions and had dramatically different reported numbers of studies. The primary authors then opted to reconcile the discrepancies by conducting a third, more rigorous meta-analysis a few years later (Joseph et al., 2015) that found an entirely different set of results than did the first two. Specifically, instead of finding some effect for mixed-EI measures, they reported a null result. And they ultimately found an effect for the ability-based measures for a relationship that they previously had dismissed as null. Why all the confusion? The primary reason was the inclusion of a very large number of student studies (among other inappropriate samples) that did not actually measure job performance in the O'Boyle et al. (2011) study. The study also did not control for potential methods effects. Having demonstrated that such effects can inflate estimated effect sizes by as much as 4x (Harms & Credé, 2010a), we would have trouble recommending interpretation of effects reported in meta-analyses that do not control for this, particularly for EI research where same-source effects are much, much more common and would tend to "swamp" the results of better-designed studies.

It is for this reason that we remain cautious about meta-analyses that do not report their coding according to best practices[1], as is the case for the Miao et al. (2016; 2017a; 2017b) studies you referenced in your letter. These meta-analyses report very large SDrho estimates, suggesting very strong moderator effects. Further, consistent with our own experience, they display enormous method effects, both in terms of measure and source. Sometimes odd decisions were made such as the one to test the incremental effects of leader EI over the effects of subordinate personality and cognitive ability (Miao et al., 2016). Further, in that same study, a meta-analytic estimate of the relationship between an individual's own cognitive ability and their job satisfaction was used as a proxy for the relationship between leader cognitive ability and follower job satisfaction. We would argue that this calls into question any purported results or conclusions. In other instances, it was not possible for us to figure out how certain numbers were generated. For example, in the Miao et al (2017a) paper, a rho of -.18 was reported for the ability-based EI – CWB relationship to calculate the incremental tests, but reported as a .01 in

[1] According to the American Psychological Association's Meta-Analytic Reporting Standards (MARS)

Table 1 of that paper. Regardless of whether clarifications or corrections are warranted, we would remain very cautious with regards to meta-analyses for the reasons we detailed above. In our opinion, the inappropriate inclusion of some samples along with the ongoing issue of poor measurement in the primary studies were ignored and, as a consequence, the overall estimates for these relationships and the tests of incremental validity are questionable.

3. Problems with Incremental Tests

During the previous exchange Antonakis made the argument that incremental validity was "the litmus test of validity" (p. 249) and you have appropriately responded by presenting a series of meta-analytic examinations documenting incremental effects of EI over other predictors such as Big Five personality and cognitive ability. However, even if we were to look past the problem of what is being fed into these meta-analyses, we would also caution against analyzing their results in the way done so in your letter. In general, we would argue that given the regular demonstration of relatively low convergence between EI measures (e.g. Brackett & Mayer, 2003), tests of incremental validity must be done at the level of the scale, not the "stream," because each model and measure of EI has unique patterns and relationships with cognitive ability and personality measures. If relationships are aggregated across scales the resultant meta-analytic estimates can underestimate the degree to which EI is a function of Big Five traits.

Consider the following hypothetical example: assume that Agreeableness, Emotional Stability, and Conscientiousness are all correlated at r=.30 with each other. Further, assume that scores on EI scale A are strongly correlated with Emotional Stability (r=.70) and more moderately correlated with Agreeableness (r=.20) and Conscientiousness (r=.30) (see Table 1a), while scores on EI scale B are strongly correlated with Agreeableness (r=.70) and more moderately correlated with Emotional Stability (r=.20) and Conscientiousness (r=.30) (see Table

1b). A meta-analyst who aggregates findings across scales A and B will report (assuming equal number of studies and samples for both scales) that EI scores are moderately correlated with Emotional Stability (r=.45), Agreeableness (r=.45), and Conscientiousness (r=.30) (see Table 1c). Regression analyses can be performed on the bases of a correlation matrix and a regression of EI scores onto the three Big Five traits using the average correlations across EI scale A and EI scale B will result in a multiple R of .57 (R^2=.32). If the EI scores had been regressed onto the three Big Five traits separately for EI Scale A and EI scale B the multiple R would have been R=.71 (R^2=.50) in each case.

Beyond issues concerning accurate estimation in incremental tests, we would also like to respond to the assertions as to incremental evidence for EI in your letter. In it, you reported results using Relative Weights Analysis (RWA) to support the claim that EI offers incremental value for the prediction and understanding of important workplace criteria. Specifically, that "…scholars who employ relative importance analysis can determine if EI measures explain meaningful variance when taking into account Big Five personality and cognitive intelligence". We question this interpretation of RWA in both your letter and in the prior analytic work that is reviewed in your letter. Indeed, it is important to note that RWA results cannot speak to the question of whether or not a variable explains variance in a criterion after controlling for other variables. Let us explain.

The weights computed for one independent variable that is part of a set of independent variables in RWA is a function of "…both its *unique contribution* and its contribution when combined with other variables" (emphasis added, Johnson, 2000, p. 2). As such, RWA is a single index that summarizes and provides more nuanced information than might be obtained via Dominance Analysis (Budescu, 1993). However, the specific question of whether a particular independent variable (here emotional intelligence) offers unique information for the prediction of an important outcome like leadership effectiveness or job performance after first controlling for personality or intelligence cannot be answered with RWA. Indeed, it can easily be demonstrated that the relative weight for an independent variable can be large even when the variable offers no incremental value whatsoever[2].

This apparent misunderstanding about the information offered by RWA has resulted in some arguably incorrect interpretations of prior meta-analytic work. For example, Miao et al. (2016) claimed[3] to examine the relationship between leader emotional intelligence and subordinate job satisfaction and interpreted a relative weight of 3.5% for ability EI as meaning that "…ability EI contributed 3.5% of the explained variance in subordinates' job satisfaction…" (p. 20) when the incremental R^2 provided by ability EI in explaining job satisfaction was in fact 0.002. Similarly, Miao et al. (2016) interpret a relative weight of 25.3% as meaning that "…self-report EI yielded 25.3% of the explained variance in subordinates' job satisfaction…" (p. 20) when the incremental R^2 provided by self-report EI in explaining job satisfaction was in fact 0.021. Similar arguably incorrect interpretations of RWA are also offered in Miao et al. (2017b) as well as Miao et al. (2018).

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[2] Consider four independent variables (X1, X2, X3, X4) that are moderately positively correlated with each other (all correlations at r=.3) and also all correlated at r=.2 with some criterion Y. As noted earlier these intercorrelations can be used to regress Y onto the four independent variables. Together the four independent variables explain 8.4% of the variance in the criterion (R=.29). Now, imagine that the four independent variables are combined using unit-weights to form a new variable X5. Using the equations provided by Ghiselli et al. (1981, p. 163 and p.169) we can compute that X5 will correlate at r=.69 with X1, X2, X3, and X4, and at r=.29 with Y (see Appendix for formulae). We can now regress Y onto X1, X2, X3, X4 and our newly formed composite variable. Not surprisingly, X5 explains no incremental variance in Y after controlling for X1, X2, X3, and X4 because X5 is a linear composite of these four variables. However, when using the syntax provided by relative weights analysis (RWA) X5 is estimated to have the largest relative weight (22.5%) of all five independent variables.

[3] As noted above, because the authors of this study imputed inaccurate information into their correlation matrix, their analyses and the conclusions they draw from them are questionable at best.

That said, as we have reviewed the literature, we also see two other major methodological issues related to concerns about tests of incremental validity. The first is that, in general, EI studies are nearly always insufficiently powered to examine the models that they purport to test (Harms & Credé, 2010b). In our meta-analysis of transformational leadership and EI, we found a corrected effect size (rho) of .13 across EI measures and .05 for more rigorous, ability-based measures (Harms & Credé, 2010a). This result implies that one would need at least a sample of 462, but preferably 3,137, if the goal was to generate results with acceptable levels of risk for making Type 1 and 2 errors (α=.05; β=.80; Cohen, 1992). The papers that you nominated as being in top journals all fell short of this standard. Some might argue that this standard is too high to be practical, but the very low direct correlations found in these studies (r=-.02 between EI and turnover intentions, Dong et al., 2014; r=.08 for EI job and performance, Farh et al., 2012; r=.18 for EI and task performance, Law et al., 2004; r= -.02 and -.03 for EI and creativity, Parke et al., 2015) suggests that it is necessary. Poor statistical power combined with journal preferences for significant effects also results in substantially inflated effect size estimates (see Gelman & Carlin, 2014).The problem is further compounded because a number of these studies required moderators to turn their null effects significant and the recommended sample size for tests of moderation is 500-3,000 (Murphy & Russell, 2017). Thus, even without consideration of construct overlap and controls, the standard practice of EI researchers, even in the top journals, seems to fall far short of what are considered to be best practices for rigorous research. Furthermore, these articles are illustrative, but typical of the field as demonstrated in the very low n-to-k ratios in the meta-analyses that you cited.

The second related methodological concern of incremental tests that we see time and again is that EI researchers have refused to design studies with robust tests of incremental validity. In the prior letter exchange (Antonakis et al. 2009), there appeared to be agreement that for EI to demonstrate its value as a construct, it would be necessary to demonstrate incremental validity over Big Five personality traits and cognitive ability. And consistent with this suggestion, in the articles that you drew our attention to, we can see examples of authors reporting doing that to some degree (e.g. Farh et al., 2012; Law et al., 2004; Schlegel & Mortillaro, 2019) in that they report incremental results over Big Five scales. However, without exception, these authors use brief or ultra-shortened personality measures that do not capture the breadth of personality traits and have been demonstrated to massively inflate the likelihood of finding misleading results, particularly for incremental tests (see Credé et al., 2012). It seems disingenuous to us to make claims about incremental validity when studies are designed in such a way as to make finding a preferred result more likely – that is by measuring the control variables badly. Again, this is not a problem limited to EI articles published in top journals. It has been a little over a decade since Antonakis (in Antonakis et al., 2009, p. 249) challenged you to find one study that shows incremental validity over a well-validated, comprehensive measure of personality and general intelligence to predict a leadership outcome, while also correcting for the effects of measurement error and using a reasonably large sample. Where are we today? We are still not aware of any such study. Studies using 10 self-report items to "control" for all of personality can no more prove the incremental validity of new constructs than blurry, low-resolution photos can prove the existence of sasquatches and alien spaceships. These statistical sleights of hand must stop.

4. A Way Forward

The previous letter exchange ended on a consensus that although the work in the field of EI had been messy and largely inconclusive, there was a way forward. Regrettably, it is our conclusion that whatever progress has been made, there is still a long, long path ahead. What can be done? Several things. We can start with methodology. Using power analyses to ensure that we have sufficient statistical power to have confidence in our results, using appropriate controls (not ultra-short personality measures) and statistical tests, using multi-source data and performance-based tests with careful attention to matching the measure to the construct (rather than allowing inclusion of items that concern the outcomes the construct is supposed to predict), reporting all the sub-facets independently, and using prospective tests wherever possible. We would also encourage an open-science friendly approach be adopted whereby researchers pre-register the hypotheses and planned analyses for their studies, make their data available for reanalysis, and report multiverse analyses or specification curves for their primary hypotheses. This brief list may seem simple to some, but it will no doubt help to improve the science in this area. For others, it may appear daunting and a bridge too far. For example, in the previous exchange, although both parties seemed to acknowledge that there were no high-quality EI studies to date (Antonakis et al., 2009, p. 258), the pro-EI author team suggested that perhaps it was too much to expect such a "perfect study" to be done and that the field could still build on messy, somewhat flawed research that had been through the peer-review process. Although we concede that high-quality research is hard to do, we would direct attention towards the current state of the field of social psychology as a cautionary warning about accepting research conducted using "questionable research practices" (Antonakis, 2017; Banks et al., 2016; John, Loewenstein, & Prelec, 2012) as a basis for building a discipline. Rather than accepting misleading results and false positive as a 'cost of doing business', the field of EI would be better served by embracing best practices in research and aiming for replicability rather than novelty.

From a theoretical perspective, our conclusion is that it is time to hit the "reset" button as well. A radical, but not inaccurate, appraisal of the field would be that there is no evidence for the validity of EI and there never will be because there is no such thing as EI. The idea of EI as an "intelligence" was a marketing gimmick that was useful for commercial purposes, but simply went too far and over-simplified a complex system of psychological factors. It is highly unlikely that there is a core, higher-order psychological factor that drives all emotional experiences. We do not see this statement as dismissing the importance of abilities related to the recognition and regulation of emotion, which are clearly essential to understanding workplace phenomena and interpersonal experiences more broadly (see Rajah et al., 2011). It is simply that EI scholars have, to a large extent, abandoned theory and let measures define their models. When asked to define the construct of interest, they speak of "streams" based on measures, not theoretical models. As noted above, EI is better conceived of as a category of variables, not a construct itself. This would be consistent with developments in the broader personality and assessment literature where scholars are moving away from structuralist conceptions of traits and towards a more functionalist understanding of individual differences whereby patterns of behavior (phenotypic traits) are understood to be driven by the interplay of motives, abilities, and perceptual tendencies (Denissen et al., 2012; Fleeson, & Jayawickreme, 2015; Harms et al., 2016; Read et al., 2010; Wood et al., 2015). We believe that this shift would allow for much greater precision in measurement and stronger theoretical models. It would also eliminate the necessity for incremental tests as the sine qua non of validity for EI. If EI were broken apart in constituent elements such as the ability to perceive emotions, the ability to control or regulate one's emotions, and so on, we instead examine how these distinct elements predict patterns in behavior (i.e. personality) without worrying whether they predicted something beyond that (see for an example, Harms & Wood, 2016). We see some evidence that the field is already embracing these new approaches in recent papers documenting the motivational (Tamir, 2016) and perceptual (Pekaar et al., 2019) components of emotions and emotional intelligence.

Relatedly, we believe that a paradigm shift away from over-simplified models of EI would prompt more exploration and examination of the nuances of emotions and emotional abilities (see also Fiori & Vesely-Maillefer, 2018). In the prior letter exchange (Antonakis et al., 2009), one interesting point of consensus was the need to explore the "curse of emotions", a hypothesized downside to high levels of EI which might lead to lower levels of performance because a leader may become distracted or inhibited by being overly sensitive their own or others' emotions. Though still in its earliest stages, the evidence is mounting that too much EI can produce negative outcomes (e.g. Fiori & Ortony, 2016; König et al., 2020; Pekaar et al., 2018; Schlegel, 2020). Just as research documenting the nonlinear nature of negative personality characteristics with leadership (e.g. Grijalva et al., 2015; Landay et al., 2019) has revealed new and interesting avenues for research, so too would we expect that abandoning the idea of EI as a rival "universal good" to cognitive ability might prompt some creative introspection on the part of EI researchers and which might ultimately lead to more effective and realistic developmental interventions for leaders.

5. Concluding Thoughts

The EI literature is something of a projective test. Depending on who you are and what you believe, you will see different things in the evidence presented. Some will see an increasingly mature field that is growing and building knowledge. Others will see a misguided and moribund field stagnating under the weight of poor measures, poor design, poor analysis, and poor theory. We fall into the latter category, but nonetheless remain optimistic that these issues can be fixed.

As a final point, we would argue that the real need in this discipline is to get back to the fundamentals of construct validity and measurement fidelity. The exaggerated focus on incremental validity reminds us of how it is possible to over-generalize from a good idea. On the surface, it makes sense that the importance of a new construct should be established in part by predicting beyond accepted constructs. However, when we consider the more fundamental nature of emotional capacities as drivers of human experiences and behaviors, and when we consider that current scale-construction practices continue to let new measures predict outcomes by including direct indicators of these outcomes within the measure, incremental validity ultimately serves as an insufficient indicator of whether something is “real” or not. In essence: at this point in the field, the test can be gamed.

Emotions matter. But it is only with better measures and better theory that we will be able to come to a more complete understanding of when, why, and how much. EI as we have known it is like a magical fairy; it can appear beautiful, but it is sick and if you stop clapping it will die. It is time to stop clapping.

Sincerely, Peter D. Harms, Marcus Credé, and Dustin Wood

Letter 3: Marie Dasborough, Neal Ashkanasy, & Ronald H. Humphrey

to Peter Harms, Marcus Credé, Dustin Wood

Dear Peter, Marcus, and Dustin:

We too are very grateful for the opportunity to engage in this scholarly debate with you. In this reply, we focus on three specific issues that you raised; (1) EI measurement; (2) problems in EI meta-analyses; (3) determining incremental validity. We argue that the EI studies we cited employed best practice with regard to handling common method variance and provide strong evidence for incremental validity. We conclude by summarizing our points of agreement and disagreement; and suggest a way forward in the study of EI within organizations.

Before we begin our response, however, we would like to acknowledge that, although we would like our letter to be focused solely on EI as defined by Mayer and Salovey (1997), we understand that this is not always the case. In particular, we recognize the ongoing debates over the construct definition of emotional intelligence (Cherniss, 2010). As such, some scholars have incorporated a broader range of competencies into the definition than others, resulting in what Ashkanasy and Daus (2005) refer to as “Stream 3” models of EI (aka “mixed models”). This stream contrasts with “Stream 1” and “Stream 2” models that on the one hand reflect the definition of emotional intelligence as an ability (Mayer & Salovey, 1997). Stream 3 models, on the other hand, represent a complementary (and valuable) set of related constructs (e.g., Bar-On’s, 2006, EQi construct includes intrapersonal, interpersonal, stress management, adaptability, and general mood). As Cherniss (2010) explains, Stream 3 conceptualizations and measures are valid, but they ought not be referred to as emotional intelligence *per se*; instead, they should preferably be labeled “emotional and social competencies.” In our response to your criticisms (and to provide a holistic overview of the EI field) we report findings pertaining to all three streams, but we wish to be clear before we begin as to our views on what EI is and is not.

1. The Fallacy of Perfect Measurement

In your reply, you argue that EI measures are inaccurate and manifestly inadequate. In essence, your position appears to be that current measures are imperfect, and that therefore we must abandon the field of EI completely or, in your words, "It is time to stop clapping." The problem here is that, were scientists to insist on such "perfection," then the likely outcome would be that *all* scientific endeavors would come to a premature end. This is referred to as the "fallacy of perfectionism," and which Van Jacob (2011, p. 12) defines in terms of, "if you cannot do something perfectly … then you should not even attempt it." Moreover, perfectionism implies "unrealistically high standards of performance" (Ocampo, Wang, Kiazad, Restubog, & Ashkanasy, 2020, p. 144). The key word here is "unrealistically." We argue that the standards you set for EI measurement are unrealistic and are not attained in any of the social sciences.

Consider, for example, the intense debate around the construct of intellectual intelligence (IQ). Like emotional intelligence, the debate as to whether IQ is a single unified construct ("g") or, as Gardner (2011) and Sternberg (2018) argue, multifaceted, is ongoing. Issues concerning measurement of IQ are today still far from settled (e.g., see Bringmann, & Eronen, 2016; Sternberg, 2019; Weinberg, 1989). In this case, perhaps is it also time to "stop clapping" for IQ? The problem here is that the concept of IQ is not exclusively "owned" by scholars. Instead, it has become a part of everyday language, deeply embedded in Western intellectual culture (Fass, 1980). We suspect that the same might today apply to EI (cf. Caruso, 2008).

As we (Ashkanasy and Dasborough) stated in Antonakis et al. (2009), we do not disagree with the proposition that, like IQ, emotional intelligence researchers need to consider the effect of the different components of emotional intelligence. As Baudry et al. (2018) comment, while we recognize that valuable insights might be found in such research, this does not negate the legitimacy of a unified EI construct – any more than consideration of the constituent components of IQ undermines the concept of "g" (see also Elfenbein & MacCann, 2017).

Another example of this fallacy may be found in the field of performance rating scales, where Landy and Farr (1980) infamously called for a "moratorium on format-related (performance rating scale) research" (p. 101). Thirty years later, Landy (2010) admitted that his call for a "moratorium" had essentially been ignored ("as if it mattered", p. 228), and that researchers in the intervening years had made enormous strides in the development of performance rating scales. It seems that Landy had not learned from his mistake, however, making a similar call in 2005 regarding emotional intelligence research.

Your call for researchers to cease their work because of perceived measurement difficulties is sure to be ignored, as was the case with intelligence and performance rating scales. Moreover, we are already seeing the emergence of new and (hopefully) improved measures of emotional intelligence, such as the Geneva Emotional Competence Test (GECo: Schlegel & Mortillaro, 2019). While the authors of this measure have shied away from describing it as an emotional intelligence test *per se*, the dimensions of the GECo correspond to three of the four MSCEIT dimensions. Ashkanasy is also collaborating with the authors of the MSCEIT in the development of an improved version. It took Frank Landy thirty years to recognize that his call for a moratorium on scale development was futile and counterproductive. We hope it will not take thirty years for you to come to the same realization.

2. In Defense of Meta-Analytical Studies of EI

In our first letter, we presented considerable meta-analytical evidence for the validity of emotional intelligence. Unfortunately, you do not seem to realize the value of these studies. Instead, you raise generic methodological criticisms that could apply to *any* study and, in some instances, you appear not to report accurately what was done in the meta-analyses we cited.

For example, you raised concerns about two meta-analytical studies (Miao et al., 2017a; O'Boyle et al., 2011), claiming an apparent discrepancy in reported numbers in the first case; and bad sampling in the other. Regarding the apparent discrepancy in the Miao et al. (2017a) article, there is in fact a remarkably simple reason for it. As the authors stated in the table for the incremental validity tests, "Observer-reported OCB and observer-reported CWB were used" (Supplementary materials, p. 13, Miao et al., 2017a). In contrast, the .01 figure from Table 1 is clearly for the *combined* relationship with CWB using both self-reports and observer reports.

We are surprised that you missed this distinction, especially given the considerable amount of attention the authors of this study gave to the differences between self- and observer-reports. Miao and his colleagues (2017a) specifically included a hypothesis on possible moderation effects caused by the use of either self-reports or observer reports. Table 1 shows the overall effects using combined observer and self-reports. Moreover, where sample sizes justify, the authors also broke down the results by observer reports and self-reports and tested for moderation effects. By conducting the tests for incremental validity using the observer reports, Miao and his associates adhered to best practice, in line with Podsakoff, MacKenzie, and Podsakoff's (2012) advice that "self-report bias may create artefactual covariance between a predictor and a criterion, thus leading to a bias in effect size estimates" (p.148).

You also discuss concerns about common method variance, and state that the Miao et al. (2017a) meta-analysis suffered from these problems too. Yet, as we already indicated, this study did in fact follow best practice for handling common method bias (Podsakoff et al., 2012). Likewise, in the O'Boyle et al. (2011) meta-analysis (that you criticize so strongly), only three out of the 46 studies used self-ratings of performance (as the authors clearly stated in Table 1); moreover, excluding these studies had no effect on the overall results.

You claim that the O'Boyle et al. (2011) meta-analysis differs from the Joseph et al. (2015) meta-analysis because of "the inclusion of a very large number of student studies (among other inappropriate samples) that did not actually measure job performance". In fact, the O'Boyle et al. meta-analysis *did not include student samples when calculating the emotional intelligence to job performance relationship*. Also, as the authors clearly state (p. 797), their findings "show that with one exception (out of 18 comparisons), EI relates to general intelligence and the FFM similarly for *both* students and workers" (emphasis added).

Moreover, given that traits are supposed to be relatively stable and enduring personal characteristics (cf. Costa, McCrae, & Löckenhoff, 2019), it makes sense that the results were the same for both workers and students. Thus, the O'Boyle et al. (2011) findings are actually quite consistent with the overall research on personality and with trait theory principles. Rather than being a weakness of the meta-analysis, the inclusion of student samples may therefore be viewed as a *strength* – since it allows for the testing of employee-student differences. Moreover, because the O'Boyle et al. study used independent ratings for employee job performance, the authors followed best practices for handling common method variance.

Instead, the reason for the differences between the O'Boyle et al. (2011) and the Joseph et al. (2015) meta-analyses is that the latter included many unusual covariates. In your letter to us, you stated that Joseph and her associates did not find incremental validity for Stream 3 EI measures, although you do admit they found incremental validity for the Stream 1 (ability) EI measures. Given the context of your prior arguments, the inference implied by your statements is that Stream 3 measures do not increment over the FFM measures and cognitive ability when predicting to job performance. In this instance (and as we noted earlier), while Stream 3 measures are not strictly EI (cf. Ashkanasy & Daus, 2005; see also Cherniss, 2010), they are nevertheless closely related constructs. As such, we should point out to you that the inference and implications you make in this regard may not in fact be correct.

Indeed, Joseph and her associates (2015) did not even test for this, owing to existing evidence. As the authors observed (p. 298), "Results further suggest mixed EI (i.e., Stream 3 measures) can robustly predict job performance beyond cognitive ability and Big Five personality traits (Joseph & Newman, 2010; O'Boyle, Humphrey, Pollack, Hawver, & Story, 2011)." Given the ambiguity about the nature of Stream 3 constructs (Ashkanasy & Daus, 2005), their stated goal (p. 299) was "to explain *why* mixed EI is so strongly related to job performance" and to answer two questions: "*What* do mixed EI instruments measure?" and "*Why* are mixed EI instruments related to job performance?"

Unfortunately, in Joseph and her colleagues' (2015) efforts to understand why mixed EI links strongly to job performance, they appear to have committed the same sort of tautology that you complain about in your letter. As covariates of "mixed" (i.e., Stream 3) EI, Joseph et al. included "ability EI, self-efficacy, and self-rated performance, in addition to conscientiousness, emotional stability, extraversion, and general mental ability" (p. 298). Thus, they used one measure of job performance (self-rated job performance) to predict to another measure of job performance (supervisor rated job performance). You also argue that items intended to measure perceived efficacy at performance also create tautologies when predicting performance. Unfortunately, Joseph et al. (2015) also committed this “sin against statistics” by including self-efficacy (in addition to self-rated job performance) as a covariate of mixed EI when predicting to supervisor rated job performance.

You also argue in your letter that it is inappropriate to aggregate across scales when doing meta-analyses unless it can be shown that there is no scale-based moderation. You go on to imply that this applies even when aggregating within streams of EI. In fact, we agree with you on this point. That is why, in one of the key meta-analyses we cite, the researchers tested for scale-based moderation effects within streams. The results of these tests support aggregating across scales within streams. In a large sample meta-analysis, Miao et al. (2017a) tested for EI scale-based moderation effects within streams and found no significant moderation effects when predicting either organizational citizenship behavior or counterproductive work behavior. We feel that these empirical results unequivocally support the theoretical arguments that scales within streams operate similarly regarding their relationships to other variables. On both theoretical and empirical grounds, aggregating across scales within EI streams appears to be justified.

3. Incremental Validity

You also imply that Miao and his colleagues (2017a) used relative weight analysis to test for the incremental validity of EI over the “Big Five” (in the five factor model of personality, or FFM) and cognitive ability. We argue that this conclusion may be incorrect. As we stated in our first letter, *all* the meta-analyses we cited that test for incremental validity used traditional methods to test for incremental validity. The authors of these studies did not use relative weight analysis to test for incremental validity. Instead, they used relative importance analysis because the predictors in their model are correlated (which makes it hard to understand the relative importance of variables). As O'Boyle et al. (2011, p. 802) explain, "This technique was specifically designed for correlated predictors, can detect patterns of dominance (e.g., complete vs. conditional), and most notably, the estimates are intuitively meaningful in that they sum to the R^2 and can be compared through ratios."

Thus, the relative weight analyses reported in these studies help readers to interpret the importance of the predictors when they are correlated. We also are not convinced by your hypothetical example, where you created a variable X5 as a linear composite of variables X1 through X4. As you point out, this X5 variable would not add any incremental variance, but could still show a substantial relative weight. In fact, if there was a real variable that strongly correlated with the other predictors as well as the outcome variable, would it not be reasonable to assign it a large relative weight? Indeed, it is possible even to conceive of this as representing a core underlying factor that explains the variance in the first four variables. Thus, if anything, we find your statistical example actually serves to *reinforce* the value of relative weight analysis. Also, we would like again to point out that the relative weight analyses in the meta-analyses we cited in Letter 1 are entirely consistent with the incremental validities results.

Moving on, you stated in your letter that, "Sometimes odd decisions were made (in the meta-analyses we cited), such as the one to test the incremental effects of leader EI over the effects of subordinate personality and cognitive ability (Miao et al., 2016)." In fact, rather than these decisions being "odd," we think it is actually most impressive that these studies found that leader emotional intelligence has a substantial effect on subordinate job satisfaction – even after including subordinate FFM personality traits and cognitive ability. One of the most important duties of a leader is to increase the job satisfaction of his or her followers, right?

You also mention that one of the studies on leader EI used a proxy measure for the effects of leader cognitive ability on subordinate outcomes. As we mentioned in our first letter, "one of the biggest obstacles to demonstrating the incremental validity of EI is the lack of evidence for the role of leader cognitive ability on follower outcomes. In many cases, the evidence for the effects of the leader's FFM personality traits is also missing." In view of this, if you could provide us with solid meta-analytical evidence on the missing correlates, we would be happy to include them in our tests for the incremental validity of leader EI. After all, you are claiming that it is incumbent upon us to show that EI increments over other well-established constructs. If there is however little or no evidence that leader cognitive ability or leader FFM personality traits influence subordinate job satisfaction, task performance, or organizational citizenship behavior, then we certainly should not be expected to include them as covariates. At this point, there is better evidence for the effects of leader emotional intelligence on subordinates' job satisfaction, task performance, and organizational citizenship behavior than there is for leader cognitive ability or leader FFM personality traits (see our first letter).

Also as we pointed out in our first letter, when comparing the effects of these EI measures with the FFM personality traits and cognitive ability, Miao et al. (2017b) found that EI is the *overall* best predictor for job satisfaction, organizational commitment, and turnover intentions, and Miao et al. (2017c) found similarly that EI is the *overall* best predictor for organizational citizenship behavior and counterproductive work behavior. O'Boyle at al. (2011) also found these EI measures to be the second-best predictor (after IQ) of job performance.

Regarding ability EI measures in particular (e.g., MSCEIT), the research indicates that they perform in similar ways to cognitive ability measures. As we stated in our first letter, ability EI measures correlate as well with job performance as do cognitive ability measures. Ability EI correlates with job performance .21 (O'Boyle et al., 2011); which is close to the correlations between job performance and cognitive ability found in a comprehensive meta-analysis by Postlethwaite (2011): .23 for crystalized intelligence and .14 for fluid mental ability. Ability EI also correlates with variables such as job satisfaction in the same way as cognitive ability: neither are good predictors. Personality variables, including Stream 3 EI, are better predictors of job satisfaction and life satisfaction. The fact that ability EI correlates with other variables in the same way as cognitive ability further supports the assertion that ability EI is in fact a type of intelligence (as its name implies).

Because Stream 1 (ability) EI measures predict job performance as well as cognitive ability tests, they might be a good choice to administer in situations where it might not be advisable to use cognitive ability measures. Also, their objective nature may help give feedback to those who lack self-insight or are resistant to subjective feedback from others. For example, somebody low on EI may not be aware of how poor s/he is at reading others' feelings and may become upset when told this directly by another person. Objective feedback from an EI measure would be very useful in this scenario to increase the individual's self-awareness. These measures may also be useful in situations, such as hiring, where applicants might be motivated to give distorted self-reports of their own EI abilities.

Although there is a lack of comparable outcomes and data for precise incremental validity tests, most of the correlations for leader emotional intelligence found in the meta-analyses we cited in Letter 1 are considerably higher than those for either leader FFM personality traits (see Bono & Judge, 2004) or leader cognitive ability (see Judge, Colbert, & Ilies, 2004). While these comparisons are not exact because of the different outcome variables, they clearly testify to the importance of leader emotional intelligence versus other leader traits or cognitive abilities.

Moreover, these results are supported by the previous incremental validity studies that have shown EI to be the best predictor across a wide range of important workplace outcomes.

We were especially surprised to see you cite Mattingly and Kraiger (2019) as if they were somehow critical of EI. Instead, their meta-analysis of 58 studies demonstrates that emotional intelligence training programs *are* effective, with a "positive effect for training on emotional intelligence scores: dc=0.45 for treatment- control designs and 0.61 for pre-post designs" (2019, pages 10-11). Their results are also consistent with those from another meta-analysis on training effects by Hodzic et al. (2018). Together, these studies clearly illustrate the efficacy of EI training programs and demonstrate that training in EI for both leaders and followers is feasible.

4. A Way Forward: Points of Disagreement and Agreement

We are pleased that you took the time to provide your thoughts on potential avenues forward for this literature. With respect to the suggestions for methodology (i.e., measures, power analyses, appropriate controls, statistical tests, multisource data, open science, replications, etc.), these are all valuable to our field and we agree that best practice should always be taught in doctoral programs. At the same time, however, these suggestions apply across *all* fields of social science research. It is not only scholars of emotional intelligence that could benefit from these methodological choices. They are generic suggestions that could apply to any construct in our field (e.g. LMX, see Gottfredson et al., 2020).

In terms of theoretical suggestions, and as we noted in our earlier discussion of the fallacy of perfect measurement, we agree that studying each dimension of emotional ability separately could be the way to go moving forward. Thus, by focusing on each ability (i.e., perceiving emotions; using emotions to facilitate thought; understanding emotions; managing emotions in self and others, see Mayer & Salovey, 1997), we should be able to generate more useful findings and therefore to develop more targeted training programs (cf. Troth et al., 2018). Nonetheless, Elfenbein and MacCann (2017) explain each of these emotional abilities are still nonetheless interconnected. To be able to manage emotions, an individual needs to first be able to perceive them, then to label them, and ultimately to understand and manage them (cf. Joseph & Newman's, 2010, "cascading model").

We also absolutely agree that scholars should abandon the idea of EI as being a "universal good." Indeed, scholars have long suggested that EI can also be employed for evil ends. Kilduff et al., (2010) first wrote about this in their article on the strategic use of EI. Later, Côté et al., (2011) referred to the "Jekyll and Hyde of emotional intelligence", explaining that EI can be used for prosocial and interpersonally deviant behaviors. Thus, while Miao et al. (2019) showed that people high on EI are less likely to be high on Machiavellianism and psychopathy, this still does not preclude the idea that some individuals who possess such "dark triad" personality traits (Paulhus & Williams, 2002) might also score high on EI – and consequently have the capacity to engage in deviant use of emotional intelligence. Clearly, research is needed to investigate when EI is used for evil ends.

There is also a "too-much-of-a-good-thing" effect, or the "curse of emotion" (Antonakis et al., 2009; see also König et al., 2020). We know that individuals who are hyper-aware of their feelings can end up suffering from burnout (if they cannot regulate them effectively) and can make sub-optimal financial decisions (cf. Li, Ashkanasy, & Ahlstrom, 2014). We call on EI scholars to investigate each of these interesting avenues in future research endeavors, and to be more balanced in their view of emotional intelligence.

Finally, we tend to agree with your comment about labelling the Stream 2 and 3 categories as "emotional efficacies" rather than emotional intelligence. This is something that two of us (Ashkanasy & Dasborough, 2015; see also Dasborough, 2019) have written about previously. We stated in the 2015 article that despite being based on the same definition of EI as the Stream 1 ability measure (Mayer & Salovey, 1997), for studies using self-reported measures of ability EI (Stream 2), researchers should refer to "emotional self-efficacy" rather than "emotional intelligence." As mentioned at the start of this letter, the broader Stream 3 measures should be labelled "emotional and social competencies" (Cherniss, 2010). Although these streams should be labelled differently, we believe that all three streams encompass valid related concepts that can add value to our knowledge of emotions in the workplace and can help leaders be effective.

5. Conclusions

Back in 2009, Antonakis asked us to "pay attention to the evidence and leave open the possibility that EI might one day go the way of the *Raphus Cucullatu*s, the dodo bird, destined for extinction" (p. 257). Similarly, in this exchange, you have said it is time for us "to stop clapping" for the "magical fairy" which appears "beautiful, but it is sick and if you stop clapping it will die". At issue here is that the methodological reasons provided for "stopping clapping" affects not only self-report studies of EI, but *all self-report measures*, which would include most of the well-accepted constructs in use in modern psychological research (including FFM personality). In fact, in a recent comprehensive survey of methods used in social psychology, Sassenberg and Ditrich (2019) found that use of self-report measures is becoming more, not less widely adopted.

To conclude, we wish to return focus to what the heading of the letter series suggests should be the focus: Does leadership need emotional intelligence? In view of the (ever-expanding) body of research evidence, three things are clear: (1) EI is a practical, theoretically sound, and assessable psychological construct; (2) EI demonstrates predictive validity across many organizationally relevant variables; and (3) the effects of EI in organizational contexts are manifest over and above the effects of other psychological variables.

The COVID-19 world we are currently living in is extremely stressful. Intense emotions have been heightened for an extended time now, especially in the USA where, at the time of writing, there is no end in sight despite the vaccine roll-out. The COVID-19 crisis has created an opportunity for leadership in all life domains (Van Bavel et al., 2020). Leaders have struggled to manage the crisis, to look after their people, and to manage their own emotions at the same time. Today, more so than ever before, we need emotionally intelligent leaders to guide us and to give us hope for the future. Through improved research methods and a new focus on unique emotional abilities, we can learn more about how emotional intelligence enables our leaders to overcome any hurdles they face.

Thank you for joining us to revisit a debate initiated over a decade ago. We were happy to review and to critique the scholarly literature that has pushed the field of EI forward considerably since the initial exchange. It is also very pleasing to report that scholars are already working toward addressing concerns you have raised in this exchange (see Kotsou, Mikolajczak, Heeren, Grégoire, & Leys, 2019; O'Connor, Hill, Kaya, & Martin, 2019). We hold great hope for the future of EI and will continue to study how EI can contribute to our understanding and development of leadership. Let the clapping and cheer-leading continue to promote our knowledge of EI, along with other constructs in the social sciences.

Sincerely, Marie T. Dasborough, Neal M. Ashkanasy, and Ronald H. Humphrey

Letter 4: Peter Harms, Marcus Credé, & Dustin Wood to Marie Dasborough, Neal Ashkanasy, & Ronald Humphrey

Dear Marie, Neal, & Ronald,

Once again, we are grateful for the opportunity to have this exchange and hopeful that this dialogue will prove useful in illuminating potential issues and future directions for the study of EI in leadership and in the field more broadly. Taking a cue from your prior letter exchange (Antonakis et al., 2009), we would like to begin with a brief review of the letters to anchor our closing comments. In your original letter (Letter 1), you argued that many advances in the field had been made and acknowledged that some concerns remained. Nonetheless, you pointed to a number of specific studies that you felt were strong candidates for documenting the validity of EI as well as some meta-analyses that you argued were powerful demonstrations of the effects of EI in the workplace. Although you covered the breadth of EI approaches, you indicated that it was your belief that models based on the MSCEIT approach (stream 1) were preferable and more valid.

In our reply, we took a more critical perspective arguing that EI measures were fundamentally flawed such that most, if not all, and even the newer ones, failed the test as to whether they were actually measuring what they purported to, a point of concern echoed by many EI advocates themselves (e.g. Cherniss, 2010; Coté, 2014; Rajah et al., 2011). We also directly questioned why the MSCEIT, with its well-known psychometric and theoretical problems (Maul, 2012; O'Connor et al., 2019), should be accorded primacy in the literature. We then went on to document several issues with the meta-analytic studies that you referenced including the inclusion of inappropriate measures and samples in addition to flawed interpretation of the results. We also criticized the use of incremental tests, though advocated for in the previous letter exchange (Antonakis et al., 2009), which we believed were inadequate tests for both statistical reasons and because they tend to generate misleadingly positive results. We finished by suggesting that the EI field embrace the practices of the open science movement, utilizing meaningful study designs, and developing measures and theoretical models that accurately reflected distinct emotional abilities and competencies.

In your rebuttal, you reiterated your preference for the MSCEIT (but did not respond to our question as to its primacy), argued that perfect measures are not necessary to advance science (a position we never held), pointed to potential issues with some of our critiques of the existing meta-analytic evidence (though left most criticisms uncommented on), made a defense of specific incremental tests in those meta-analyses, made the argument that EI as a construct has been more scrutinized than rival predictors, and finished with points of agreement and disagreement concerning the state and future of the EI field.

In this final letter, we will respond to your arguments, revisit some of our points that were left unaddressed in your Letter 3, and then offer own suggestions for both the field of EI and for organizational and psychological research more broadly.

1. Clearing up Disagreements on Prior Meta-Analytic Work

Although we are not sure how valuable readers will find our disagreements about the specific methodologies and interpretations given to individual papers on EI, we nonetheless would like to briefly respond to some of your more pointed critiques relating to our readings of published EI meta-analyses.

You criticize us about misunderstanding the apparent discrepancy between two correlations – one reported in Table 1 of Miao et al. (2017a) and one used in the computation of incremental validity estimates. We thank you for pointing out that the value in Table 1 is based on self-report CWB and other-reported CWB while the value used in the incremental validity computation is based only on observer-reported CWB. This decision seems strange for us because the combined data were already limited to only 3 studies and 639 participants. Excluding one or two of these studies for, to us, unclear reasons in a way that vastly increased the size of the EI-CWB relationship estimate bases the entire inference on, at best, only two studies, not something which would ordinarily be consistent with best practices in meta-analysis. As we noted in our earlier Letter 2 such data and such decisions do not leave us convinced that the value of EI has been demonstrated.

As for your claim as to the decision to rely only on observer-reported CWB, this seems to be contradicted by Miao et al. (2017a) themselves when they note (in Table 2) that "*we used corrected correlation between stream 3 mixed competency EI and self-reported CWB to impute this missing cell in the input correlation matrix….Berry et al. (2012) performed a meta-analysis and found that self- and observer-reported CWB have fairly high convergence and demonstrate very similar patterns and sizes of relations with a set of common correlates. Berry et al. recommended the use of self-reported CWB as a viable alternative to observer-reported CWB*." (p. 153)[4]. Of course, this also contradicts your claims that this meta-analysis was conducted in a manner consistent with best practices as outlined by Podsakoff and colleagues (2012), because the capitalization on same-source method effects (i.e., self-ratings of EI and self-ratings of CWB) would have inflated the correlations observed for the EI-CWB relationship.

You also expressed your belief that we had mischaracterized the O'Boyle et al. (2011) paper when we expressed our concern about the inclusion of student samples and other

[4] Ideally, future studies of counterproductive work behaviors would also include objective behavioral measures rather than relying almost entirely on subjective, retrospective self- and other-reports.

inappropriate samples in that meta-analysis on the EI-job performance relationship. However, the 2015 joint paper co-authored by O'Boyle (Joseph et al., 2015) clearly shows (Table 1, p. 305-306) that the O'Boyle et al. meta-analysis included as measures of job performance (amongst others): academic performance, parishioner support, subordinate-rated leader effectiveness, coworkers ratings of managerial skill, student-leader performance, self-rated overseas adjustment, hockey player performance, transformational leadership practices, and overall course assessment. We hope that readers agree that these variables may be important criteria in their own right but that they are not well-aligned with how job performance is typically conceived. Consequently, we are inclined to agree with O'Boyle's acknowledgement that his earlier work contained inappropriate measures and samples.

You also take issue with the approach taken by Joseph et al. (2015) in their meta-analytic investigation of why mixed EI measures predict performance. We might have shared your concern if the purpose of their paper had been to merely test whether or not EI predicted unique variance in job performance. However, our reading of the Joseph et al. paper is that the authors primarily intended to examine whether or not the authors of mixed EI measures may have engaged in what Joseph et al. refer to as "heterogenous domain sampling" (p. 299); that is, capturing elements of other well-known predictors of job performance and subsuming them under the common-label of mixed EI. We disagree slightly with their inference that heterogenous domain sampling actually occurred because other explanations are also possible, but do not think that your characterization of them having engaged in various conceptual and statistical "sins" is entirely accurate.

Finally, we agree that the scale-based moderation results presented by Miao et al. (2017a) are interesting and think that other researchers should routinely present such scale-based moderation results - not just in EI research. However, we disagree with your assertion that the Miao et al. paper is a "large sample meta-analysis" or that these results necessarily suggest no moderation across scales. Only two EI scales were examined for the CWB criterion and for one of these there was only data for two studies with a total sample size of 280. We are unclear how such limited results speak to effect size heterogeneity across EI scales within a stream. There are some more data for the OCB criterion but here too the data were very limited. For example, for the three stream 3 EI scales that could be examined, there were only five studies for the Emotional Competence Inventory (total N=1,086), four studies for the Genos Emotional Intelligence Inventory (total N=1,269), and only two studies for the Emotional Quotient Inventory (total N=492). Formal tests of moderation across scales is not likely to be particularly meaningful when the number of samples on which comparisons are based are so small, although the substantial variability in the relationships with criteria (range of rho=.46 to rho=.65) suggests that moderation is present. As such we do not share your confidence that differences across scales are small enough to warrant meta-analytic aggregation across scales.

2. Clearing up Disagreements on Tests of Incremental Validity

We will be brief in this section because we feel you failed to address most of our criticisms of incremental tests and of the analytical issues with the Miao et al. (2016, 2017b, 2018) meta-analyses in your prior Letter 3. You also suggested that we may have incorrectly implied that Miao et al. (2017a) meta-analysis used relative weights analysis (RWA) to test for incremental validity. This is possibly true depending on how one interprets the purpose and presentation of the analyses conducted. We drew this conclusion from page 190 of their manuscript where they explicitly rely on RWA to draw conclusions about the value of EI scores as a predictor relative to personality and cognitive ability and presented these analyses in a table that included information on incremental effects.

As for our worked example, we will admit that the effects we showed could potentially be explained by the presence of a higher order factor, but that is just one possibility and we are more inclined to align our perspective with existing evidence (Joseph et al., 2015) that EI measures represent a case of heterogenous domain sampling.

You made an interesting point considering the standards that EI has been held to in relation to proving its validity relative to established constructs. Perhaps this is the case. On the other hand, it probably should be case when introducing new constructs. Constructs like intelligence and the Big Five personality dimensions were subjected to enormous scrutiny when first introduced. That said, we did pause at your comment that there was no comparable meta-analytic work done on leader personality traits and follower outcomes. There is, of course, meta-analytic work by Scott DeRue and colleagues (2011) for leader Big Five personality traits predicting group performance, job satisfaction, and satisfaction with the leader. The meta-analytic estimates could use updating and do not cover all comparable follower outcomes, but they do exist and are even suggestive that EI may be a stronger predictor than Big Five traits for emotion-laden outcomes such as follower satisfaction. We find it curious that you did not find this paper in your search because you were able to find a rather obscure, unpublished dissertation (Postlethwaite, 2011), for meta-analytic estimates of the relationship between cognitive ability and performance in order to support your claim that the effects of cognitive ability were relatively small and comparable to that of EI. We have but two comments on that particular meta-analysis. First, most of the studies used by Postlethwaite likely only had measures of fluid and crystallized intelligence because the employees were selected using these measures.

Consequently, there is substantial range restriction (see Table 5 of that dissertation), as Postlethwaite demonstrates, which significantly reduces the uncorrected correlations. Our second related comment is that all correlations are products of the reliability and variance of the measures being used. That both EI and cognitive ability showed a small effect for one relationship and a slightly larger effect for another is *not,* as you suggest, evidence that EI is a form of intelligence. This is an example of a logical fallacy called affirming the consequent. And even though we are sympathetic to the idea that constructs can be compared using correlational patterns in their nomological networks, we would never recommend drawing a conclusion on the basis of only two data points (see Wood & Furr, 2016 and Furr, 2008 for best practices in such analyses). Nonetheless, you do raise a very important point. Cognitive ability is widely agreed upon to be the best predictor of job performance (Schmidt & Hunter, 1998), career success (Ng et al., 2005), and one of the best predictors of many important life outcomes (Roberts et al., 2007). Based on your Letter 3, we are in complete agreement that there is a tremendous need for research examining the relationship between cognitive abilities and leadership outcomes and the outcomes of their followers. We believe that the best evidence is likely to come from the U.S. military where large samples can be obtained with excellent assessment tools and where restriction of range is unlikely to be much of an issue. As reported in the original Project A data (McHenry et al., 1990), the relationship between cognitive ability and these critical leadership outcomes is likely to be around .31[5].

3. Disagreements on the Need for Improved Measurement

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[5] It should, however, be noted that these estimates are based on linear estimates of the effects of intelligence and that other scholarship has suggested that the positive relationship between intelligence and leadership may taper off or even become negative at the extreme high end (e.g. Simonton, 1985; Antonakis et al., 2017).

In your Letter 3 you argued, as you did in your last letter exchange with John Antonakis, that we were insisting on perfect measures and perfect designs. We think a fair reading of our Letter 2 would show that we did so at no time and that this is essentially a strawman argument. We agree that requiring "perfect" measurement would set an impossible standard that would leave all of science grinding to a halt. But we never made that suggestion. We simply pointed out that many of the most popular EI measures were flawed not only for theoretical and psychometric reasons, but also because of the inclusion of inappropriate content[6].

Much of the time, we simply do not know what we are actually measuring when using EI instruments. Although you spent an enormous amount of time pointing to, discussing, and defending point estimates of meta-analyses, we feel that you never really addressed what was perhaps the most important point in our paper. Until these measurement issues are resolved, the meta-analytic work you continue to base your arguments on are potentially misleading or even meaningless. These concerns are not new. They are the reason why Mattingly and Kraiger (2019) urged caution in their own meta-analysis. And these issues ultimately were the basis for the

[6] To illustrate with academic achievement, the SAT and ACT have some problems, but generally if they are administered in the correct conditions (e.g., people are prevented from cheating), scores are validly interpretable as measuring a person's "academic achievement" in certain areas (e.g., verbal reasoning, mathematics). This is despite the fact that they are not "perfect". We can reasonably anticipate that if a person is really good at geometry, but never took a calculus class that their overall SAT Math score will rise and fall in proportion to the number of test questions given from these domains. However, we would have a problem if (1) their SAT Math score was systematically influenced by how they wanted to answer True-False questions "I am good at math," as the inclusion of such questions would make their scores affected by self-presentational efforts rather than demonstrations of actual skill/achievement. And we would have a similar problem if (2) their SAT score was systematically affected by their GPA (e.g., school records of their GPA factored into their final SAT score), as this would conflate the SAT score with one of the major (distinct and distinguishable) outcomes it is supposed to predict. Designers of achievement tests understand that factoring in such items to any degree into a person's SAT score would invalidate the test scores, and so they exclude them.

The analogous situation would be if EI measures included items like (1) "I am good at recognizing emotions" and (2) "I am liked by lots of people." As we noted in our previous Letter 2, EI measures are problematic because they do include these types of items. These are not "acceptable imperfections" in scales of the kinds like the variation in domain sampling detailed above. They are problems that systematically impact scale scores in ways that make them correlated with irrelevant or inappropriate constructs. Moreover, they do so in inconsistent ways across measures.

findings of Joseph et al. (2015). Joseph and Newman (2010) perhaps provided the most damning critique of the state of the literature. They asserted that the more valid measures of EI typically did not predict work outcomes well, but that the more contaminated, less valid measures did. Thus, "the current status of emotional intelligence research presents the scientist-practitioner with a trade-off between theory and data – an ugly state of affairs" (Joseph & Newman, 2010, p. 72). Until these issues are resolved, we find ourselves agreeing with Joseph and colleagues (2015) that incremental tests that examine EI should not be the goal of EI research, but rather *why* such effects, if present, have occurred.

We think that a mature discipline (and it has been more than a quarter of a century since Goleman published "Emotional Intelligence") should have made more progress than is evident in the measurement of EI. We are, of course, also in full agreement that numerous other sub-disciplines in the organizational sciences and psychology suffer from similar measurement problems and have written fairly extensively on these issues (Condon et al., in press; Credé et al., 2012, 2017; Harms et al., 2014, 2016; Mõttus et al., 2020; Wood & Harms, 2016). Specifically, we see the over-reliance on self-report measures, and even subjectively rated questionnaires regardless of the source, for both predictors and outcomes as a major issue for ultimately establishing the validity of constructs and relationships (Roberts et al., 2006). To bolster the confidence in causal relationships, we would urge researchers to more regularly employ objective or performance-based measures in their work (Funder & Sneed, 1993; see also Fischer et al., 2020). We also believe that the abilities and perceptual tendencies that serve as the drivers of EI and other phenotypic traits would more profitably be examined and evaluated through techniques designed to illuminate psychological processes and decision-making such as elaborated situational judgment tests (Wood et al., 2019, 2020, 2021; Wood & Harms, 2018). In general, we believe that greater attention to measurement precision and quality would greatly benefit all of these sub-disciplines. Recognizing that problems exist in one's own discipline should not require that other disciplines fix similar problems first.

We also hope that EI researchers become better at cleaning their own house. Yes, new EI measures are being developed, and some of these may prove to offer more value than existing popular scales, but the continued widespread use of non-optimally developed EI scales suggests that the field may benefit from a more self-critical approach. In our earlier response (Letter 2) we noted significant validity problems with two very popular self-report measures: the Bar-On EQi (Bar-On & Parker, 2000) and the instrument developed by Schutte et al. (1998). You did not respond to our concerns about these, so perhaps you agree with our assessment, but it is worth noting that these instruments continue to be very heavily cited, and very widely used, by EI researchers[7].

4. Better Science, Better Future

During your discussion of what you felt was our mistaken demands for better measures, better designs, and more appropriate and accurate analyses, you suggested that our calls for improvement would go unheeded, but that science would march on regardless. You used the example of Landy and Farr's (1980) suggestion that research on response options had peaked and it would be more profitable for researchers to invest their time elsewhere. This example struck us as somewhat ironic because we have been trying to make the argument that EI measures could use further refinement and validation, not that they have already achieved acceptable levels of construct validity and that efforts to improve them are unlikely to bear fruit.

[7] For example, the measure described by Schutte et al. (1998) has been cited over 5,000 times as of April 2021, with close to 400 citations in each of the last five years. Similarly, the various papers describing development of the Bar-On EQi and its psychometric instruments have been cited over 10,000 times.

Regardless, we would be more than happy if our present call for improved science was even half as influential as their paper was (see Ilgen et al., 1993 for a review).

More broadly, we are unconvinced by your argument that the increased usage of self-report measures in social psychology should be used as evidence for an argument that criticisms of such measures are overblown and that efforts to improve science are hopeless. Graduate students who are fortunate enough to have excellent research methods instructors will no doubt be familiar with articles by both Fiske and Campbell (1992) and by Jacob Cohen (1990). In both of these papers, the authors look back, often frustrated, but still with good humor, at their legacies and note that despite the fact that their work is among some of the most cited in all of the social sciences, the methodological problems they were trying to correct had not been solved and perhaps had gotten worse. Perhaps that will be our fate as well. We can surely agree that it is not much fun being Cassandra[8], walking around a doomed city, and having your warnings ignored.

But the example you provided, that of social psychology, is telling. One of our favorite stories for emphasizing the importance of research methods and ethics for new graduate students is an article from *Slate* titled “Everything is crumbling” (Engber, 2016). It documents the story of Repligate and the widespread failure of many established studies and theories in social psychology to stand up to scientific scrutiny, and which features a damning quote from a leading researcher that “At some point we have to start over and say, *This is Year One*.” Why would we want this for our own discipline? Can we really ignore the signs, both in EI research and in organizational scholarship more broadly? We do not see much evidence that the field of EI research has advanced much in the decade since your last exchange. Why are we still making

[8] Cassandra was a Trojan princess cursed to utter true prophecies, but never to be believed.

excuses? What kind of example are we setting for our students? One of the papers you identified as being an example of the way forward explicitly and strongly recommends using EI measures that have been identified as being the most flawed (O'Connor et al., 2019). There is a price for tolerating poor research and hoping for the best (Rosenthal, 1994), a price that social psychology now has to pay. When is it our turn?

Fortunately, we do see signs of hope. Despite the resistance of many established and well-known figures in their field, social psychology has begun to embrace practices and principles associated with open science and robust study designs. This change is being made by the open-minded, the idealists, and those who refuse to treat science as a game. But the process is not always smooth. Yes, some calls will go unheeded, but as a discipline we will be better off if we do not ignore those imploring us to do better. We are already seeing signs of this recognition in journal policies such as those which have been recently implemented at *The Leadership Quarterly* (Antonakis et al., 2019) and the *Journal of Business and Psychology,* as well as their willingness to publish self-critical research (e.g. Alevsson & Einola, 2019; Gottfredson et al., 2020) and to make the hard decision to withdraw flawed or misleading articles even when it is unpopular to do so (Atwater et al., 2014).

5. Concluding Thoughts

Where does this leave us? As we concluded in Letter 2, we think we are in agreement on the importance of emotions and the need to study them in leadership processes. The first book of western literature, Homer's Iliad begins "Sing goddess, the anger of Peleus' son Achilleus and its devastation, which puts pains thousandfold upon the Achaians…since that time when first there stood in division of conflict Atreus' son the lord of men and brilliant Achilleus" (Lattimore, 2011, 1.1-7). It is a story about emotion (anger) and the conflict between a leader and his follower. There is a reason such stories echo throughout the ages and stick with us. They represent the fundamental elements that make us human, our psychology and our relationships. To that end, we do not foresee a future where there will not be value in studying the intersection of emotions, leadership, and their consequences.

Unfortunately, perhaps, we see many points where our understanding of how to make sense of the literature and move it forward does not align even as we agree on certain points. As you noted in Letter 3, we both agree that there is value assessing the elements of the emotional process: perception, comprehension, regulation, and display. Where we depart ways is in whether EI as a higher order construct exists. You assert that it does, we remain unconvinced. It seems to be a difference between accepting structural models of traits as opposed to embracing modern functionalist models (Denissen et al., 2012). To be clear our preferred manner of understanding is not far from that of the cascading model of emotions (Elfenbein & MacCann, 2017; Joseph & Newman, 2010) and other related process models of emotions at work (Zapf et al., 2021). Positive manifolds can arise through multiple distinct processes that might have reciprocal or self-reinforcing properties. This is likely the case for cognitive abilities (see van Der Maas et al., 2006) and it is likely to be true for emotional abilities. Specifically, that different emotional abilities will not just be related to one another but will also serve as antecedents of different phenotypic traits. For example, emotion regulation will serve as driver of neuroticism while emotion perception serves to enable agreeableness (Soto et al., 2021). Though such abilities may be correlated and lead to one another, as they do in the cascading model, that is not sufficient reason to assume a higher-order factor (Credé, & Harms, 2015; Wood et al., 2015).

We also remain somewhat more circumspect than you on the issue of the usage of meta-analyses to discern ultimate truth. Simply put, we believe that meta-analyses are only as good as the studies that go into them (Bobko & Stone-Romero, 1998; Eysenck, 1978; 1994; Murphy, 2017; Sackett, 2021; Strube & Hartmann, 1982), and agree with Ioannidis (2016) that many meta-analyses can be misleading. As noted in your prior letter exchange and now again in this one, the field of EI simply does not have enough studies with robust designs and valid measures to be making any strong claims as to validity. Aggregating into meta-analyses does not fix this problem, it only conceals it. Put another way, you can make something that, from the outside, looks like a black forest cake with strawberries and fancy frosting, but if it is full of fish-shaped crackers, dirt, and ethylbenzene, then it is not going to be the delicious and moist treat we expect when we bite into it. The cake is a lie.

You suggested that our calls to improve measures and design are likely to go unheeded. It may surprise you that we are inclined to agree. But that does not mean it is not a goal that should be pursued. We remember battles like the Alamo and Thermopylae not because they were triumphs, but because they ended up inspiring those who came after them to fight for a higher ideal. At the end of the day, we would rather be right and lose than to be wrong and win. That said, we do seem to agree that there are things that can and should be fixed in our field. Perhaps the key difference lies in the pace at which we want to see these changes occur, the perception of the magnitude of the problem, and the faith that we have in the tools that are currently at our disposal. And perhaps we will be proven wrong that things will work themselves out somehow. Should that occur, we will be happy to respond to a told-you-so letter ten, twenty, or thirty years from now. But until that time, we consider it an ethical imperative to continue pushing the field to improve itself. We think you would agree.

We have very much enjoyed the chance to have this exchange and hope that it will serve to stimulate further improvements in the field. As the saying goes, *ex clamore, fructus!* And though we may have come off as sounding very critical, we are very optimistic about the future and our capacity to learn and to make changes where needed. The problems we have debated are not limited to the field of EI, they are true of leadership and personality research, and the wider organizational literature as well. But why not start? Here, right now.

Sincerely, Peter D. Harms, Marcus Credé, and Dustin Wood

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Table 1a

Hypothetical Meta-Analytic Intercorrelations for EI Scale A

	1	2	3
1. Emotional Stability			
2. Agreeableness	.30		
3. Conscientiousness	.30	.30	
4. Emotional Intelligence Scale A	.70	.20	.30

Table 1b

Hypothetical Meta-Analytic Intercorrelations for EI Scale B

	1	2	3
1. Emotional Stability			
2. Agreeableness	.30		
3. Conscientiousness	.30	.30	
4. Emotional Intelligence Scale B	.20	.70	.30

Table 1c

Hypothetical Meta-Analytic Intercorrelations for Average of EI Scale A and EI Scale B

	1	2	3
1. Emotional Stability			
2. Agreeableness	.30		
3. Conscientiousness	.30	.30	
4. Average of Emotional Intelligence Scales A and B	.45	.45	.30

Appendix

The correlation between a unit-weighted composite and one of the components of the composite is given by Ghiselli, Campbell, and Zedeck (1981, p.169) as:

$$r_{z_1 c_z} = \frac{1+(k-1)\bar{r}_{1i}}{\sqrt{k+k(k-1)\bar{r}_{ii`}}}$$

The correlation between a unit-weighted composite and an outside variable is given by Ghiselli, Campbell, and Zedeck (1981, p.163) as:

$$r_{z_0 c_z} = \frac{k\bar{r}_{0i}}{\sqrt{k+k(k-1)\bar{r}_{ii`}}}$$

where:

k= number of components in the composite

$\bar{r}_{1i}$ = the average correlation of the component of the composite with all of the other components of the composite

$\bar{r}_{0i}$ = the average correlation between outside variable and the components of the composite

$\bar{r}_{ii`}$= the average correlation among the components of the composite