A response to Philip Hyland, Demetris Katsikis, and Chrysoula Kostogiannis on the debate point concerning the binary theory of emotional distress.

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We were pleased that our paper "Applying the REBT cognitive disputation technique to the binary theory of emotional distress" (Turner, Jones, & Wood, 2018) sparked interest and debate within the REBT community. Our original paper was conceived to draw out the opinions of those who read it, and when we approached the three commentators, Philip Hyland, Demetris Katsikis, and Chrysoula Kostogiannis, we anticipated an insightful and engaging discussion. Our expectations were exceeded, as three different types of responses were developed by the three learned commentators. Hyland's paper offers a rather categorical rebuttal of the binary theory of emotional distress (BTED), Katsikis' offers a balanced and critical perspective, whilst Kostogiannis's paper offers support for the BTED. All three approach the issue in different ways, and all three commentators draw on their own values and experiences to guide their points. It is impossible to do every element of each commentary sufficient justice, so we will focus on key points made by the commentators in our response here. In the spirit of scientific debate, we invite Hyland, Katsikis, and Kostogiannis to respond to the current paper with their further rebuttals and comments.

The notion that BTED is "intriguing and perplexing"

Hyland states that BTED is "one of the most intriguing and perplexing aspects of contemporary REBT theory", and we agree. We cannot speak to what Hyland meant by this comment specifically, but for us, part of the intrigue is that the theory makes intuitive sense and speaks to the many binary constructs that proliferate psychology (e.g., challenge vs. threat, approach vs. avoidance, positive vs. negative affect). Of course, intuition is no substitute for evidence and perhaps this partially explains why Hyland finds the theory perplexing. What we find perplexing is that BTED is a core element of REBT theory, and yet it has generated research that is limited in both its volume and its scope. It is important to

make it clear that in our original paper we did not intend to justify the use of BTED in practice, we simply tried to offer a balanced perspective on BTED using the REBT arguments of empirical, logical, and pragmatic reasoning. That the BTED was unsupported empirically is of course a core line of enquiry touched upon by Hyland, Katsikis, and Kostogiannis in their commentaries. That the BTED appears to be more pragmatically sound than empirically sound has triggered some valuable debate, that we extend in this paper.

The notion that the validity of the BTED rests on the psychometrics that measure it.

Hyland at times conflates his criticism of the Profile of Emotional Distress (PED) with his criticism of the BTED. The point Hyland quotes here, from our original paper, that "it is more accurate to consider them [anxiety and concern] to be different emotions altogether as they drive different behaviours" is indeed, a testable hypothesis as Hyland rightly points out. But this hypothesis cannot be tested using psychometrics in which adjectives are used to describe how one is feeling. Since behaviour is a key dividing factor between Healthy Negative Emotions (HNEs) and Unhealthy Negative Emotions (UNEs) (Ellis & DiGiuseppe, 1993), then behaviour must be assessed and analyzed with regard to its adaptiveness. In BTED, HNEs are associated with adaptive actions, and UNEs are associated with maladaptive actions, which clearly have to be context dependent. What is adaptive in one setting and situation, could be maladaptive in different settings and situations. In his review of BTED in 2012, Hyland wrote about the importance of context, stating that "although emotions like rage or panic will usually produce maladaptive behavioural responses and are therefore usually considered 'unhealthy', under certain circumstances such emotions may in fact lead to adaptive behavioural responses and thus in unique circumstances emotions such as depression or anxiety can be considered 'healthy'" (Hyland & Boduszek, 2012, p.52). This is a key point, and one that speaks to the importance of contextual specificity when measuring emotions in line with the BTED.

When we compare the validity of measurement scales used to assess the BTED, with the validity of measurement scales used to assess some prominent unitary constructs, there is no contest. The BTED, much like REBT in general, simply has not generated a comparable corpus of empirical research compared to, for example, cognitive therapy (CT; Beck, 1976). As a result, the application, measurement, and rigorous testing of the BTED has not been undertaken as frequently as it has been for unitary model favoured by CT (e.g., Hyland & Boduszek, 2012). But when measuring emotions in line with the BTED, we may have to play a different game that takes place away from psychometric assessments, and explore biopsychosocial markers of emotional reactivity. At the very least, we should take seriously what Ellis (1994) wrote about testing REBT theory, where he felt that this endeavor should involve real-life stressful situations. Much of the research testing BTED is cross-sectional and is done in the absence of real activating events.

In our own work, following Hyland's logical argument, we too subscribe to a unitary model of emotional distress. In one study, we found that the REBT Model-I (DiLorenzo, David, & Montgomery, 2007) was valid in predicting the psychological distress (anxiety, depression, anger) of sports participants. The positive relationship between primary irrational beliefs and psychological distress was mediated by the secondary irrational beliefs. In another study of occupational workers (Turner et al., 2016), we found that greater irrational beliefs were related to higher depression, anxiety, and anger. So, whilst we are encouraging a balanced view of the BTED, we also realize that we are hypocritically endorsing the unitary model in our research to date. However, we have adopted the unitary model for one main reason: there is no valid assessment of the BTED as it is set out in REBT theory (e.g., David & Cramer, 2010), where subjective experience, behaviors (intended and executed), and physiological responses are all considered (Ellis & DiGiuseppe, 1993). Clearly future research should strive to develop such an assessment tool, and one that meets Katsikis'

criteria of tapping implicit cognitive processes that are by nature unavailable for conscious introspection and observation.

The notion that a case-based approach should be taken to examine the BTED pragmatically

Katsikis points to the relative dearth of logical and pragmatic research examining the BTED; putting forth case-examples as an effective research method in which to do so. In our own work, we have found the idiographic, in-depth, and empirical investigation of REBT using a single-case research design (Turner, Ewen, & Barker, 2018) a valuable method to both ascertain the application, effects, and receipt of REBT in practice. This perhaps reflects a shift away from an over reliance on between-groups design towards the intensive study of single-subjects using repeated measures design (Normand, 2016). Specifically, single-subject's designs are typified by a small number of participants, using a research method considered high in both levels of internal (i.e., stable and/or staggered baseline data, across participants design) and ecological validity (i.e., field-based). Furthermore, a single-case designs would allow researchers to assess the variety and complexity of the BTED using a variety of assessment measures highlighted by Kostogiannis and Katsikis; of which examples include: motivational, behavioural, cognitive, psychophysiological, physiological, and pre-dispositional human constructs. Ultimately, single-case designs appear to offer a fruitful research method to document and make informed conclusions regarding effects of BTED in practice.

The notion that "the BTED should be roundly rejected" and that we should "abandon all belief in the BTED"

When we embarked on our research programme into REBT, we shared Hyland's sentiment here. It is only really through the process of writing the original debate paper in the first issue of this journal that we arrived at a more balanced perspective. Empiricists will of

course usually (or always, if they are dogmatic empiricists) reject that which has poor, or absent, supporting evidence. So as scholars who subscribe to the dominant empiricist ideology that has entrenched the psychological sciences, we too feel that on the basis of the scientific evidence concerning the BTED, it is looking bleak. However, to reject the BTED on the basis of lack of supporting research would be to base our final summation of the BTED not on *contradictory or lack of evidence*, but on *lack of tests*. That is, it is not so much that the BTED has been tested and disproved over and over again, it is more accurate to say that the BTED has not been tested appropriately, or often enough, to make a final judgement.

To roundly reject the BTED is to suggest that empiricism is the supreme ideology which should inform our judgements about what is and isn't "true". Hyland intimates that REBT has sought to be empirically-based, but we must remember that REBT was not developed via empirical means, in contrast to cognitive therapy (CT; Beck, 1976). Rather, the roots of REBT are imbedded within philosophy (Padesky & Beck, 2003), mostly Stoicism. The origins of binary approaches to emotional distress go back 2000 years to when the Stoics distinguished between *apatheia* (e.g., fear) and *eupatheia* (e.g., caution). Emotions for the Stoics are certainly different to what we would consider as emotions today, but nonetheless, binary perspectives on emotions are not new. Of course, just because something has a deep history does not mean that we should consider it to be true, but we would caution against placing too much emphasis on empiricism, especially when the data used to assess theoretical validity is flawed, and mostly absent.

The notion that pragmatic truth alone is "the worst kind of postmodern thinking about the nature of truth" and that we "must reject the BTED"

Only basing the acceptance of the BTED on pragmatic grounds would of course be ill-advised. Hyland suggests that we would be "abandoning science" by relying on pragmatic means alone. Of course, in our original paper this is why we address the BTED using

empirical and logical arguments too, after which the BTED was left wanting. But we would caution against the totalitarian acceptance of positivistic empiricism when making judgements about theories and ideas, that although escaping scientific proof, may be useful and sensical to those in need of psychological treatment. The use of introspective and intuitive knowledge steps into the realm of the philosophers, rather than the scientists, but we should not be so quick to sweep aside the philosopher and the humanist, as Katsikis so eloquently puts forth. For example, there is some evidence that holding pragmatic truth, in the face of a lack of empirical truth, is justified for health and wellbeing. Belief in God is a good example of a metaphorical truth, as proposed by Bret Weinstein, where a belief that may not be scientifically proven, may in practice be beneficial due to the behaviours it elicits. Even though we cannot empirically prove the existence of God, believing in God has been shown to lead to various positive wellbeing outcomes (e.g., Cranney, 2013). Evidence-based practice is a two-way street – we need our practice to inform theory, not just use theory in our practice. We should be asking, "what can we learn from our clients about how emotions emerge and are experienced?" The truth Hyland might be referring to is that which is generated through formal scientific investigation. Scientific research does not lend itself to proving or disproving truths; it is a hypothesis driven endeavour that, in modern science, is based on probability.

However, as Katsikis intimates, ecological validity is also important, whereby people testify to the utility of BTED informally against the backdrop of complex and non-linear daily experiences. The sanitized and dogmatic world of scientific enquiry, whilst clearly vital for the propagation of civilization, is limited by the measures, the measured, and the measurer. Indeed, the replication crisis (Hengartner, 2018) and questionable research practices (QRPs; Leslie, George, & Drazen, 2012) currently challenging the social and life sciences speaks to the frailty of science when it is in human hands. We should not be too

hasty to put BTED in the dustbin until we have better quality, more numerous, and more comprehensive tests of the BTED. Hyland does leave his opinions open to change in light of "emerging evidence", which the REBT research community should strive to generate.

Beyond binary and unitary conceptualization of emotions

Katsiki's emphasizes the complexity of human beings, adjunct to their propensity to be both pragmatic and functional in response to life events. In the final paragraph, Katsikis widens the scope of discussion by introducing and encouraging the readership to consider tripartite and quaternary models of health/distress within the realms of possibility. Perhaps, and more importantly Katsikis challenges the assumption that models of emotional distress are restricted to generality. Instead he suggests situational specificity (i.e., diverse life conditions) and individual differences to best determine the most appropriate conceptualization of emotional distress. This both confirms the complexity and contentious issue of emotions and perhaps points to the notion that neither unitary or binary models provide the most empirical, logical, or pragmatic conceptualizations of emotion.

Conclusion

Our final word is that we would caution practitioners against using the BTED in their practice due to the lack of appropriate tests of the theory, not because there is sufficient evidence *against* the theory. That is, in our view the BTED has neither been 'proved' nor 'disproved' since it has not been tested appropriately. In order to attempt to validate the BTED, researchers should develop comprehensive and multifaceted measurement tools that assess the core components of BTED. Practitioners who are using the BTED in their practice and finding positive wellbeing and mental health outcomes with their clients, are urged to report their findings in professional practice and or scientific literature. How can we develop an informed opinion about the BTED if we don't have the literature to help us make informed judgements? We stand by the pragmatic rationality of the BTED we posit in our original

paper, because as practitioners we have seen the BTED help clients to fulfil their potential. However, as scientists we must constantly question the theories we utilize and as an REBT community we must strive to validate, or invalidate as the case may be, prominent theory that informs and shapes our practice.

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