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Affect circumplex redux: the discussion on its utility as a measurement framework in exercise psychology continues

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It has been suggested that the circumplex model could be a useful conceptual and measurement framework for investigating the effects of exercise on affect. This paper reviews three critical issues pertaining to this model, namely (a) the fit of the circumplex model to self-reported affective data, (b) the notion that postulated separable neural substrates of pleasure and displeasure challenge the principle of a bipolar relationship between these states, and (c) the idea that assessments of perceived activation are problematic. For each criticism, the evidence is reviewed and caveats that must be taken into account in evaluating the data and conceptual arguments are identified. The circumplex model has both considerable strengths and important limitations. For the application of the model in exercise psychology to be fruitful, researchers should recognize both aspects of this model.

Keywords: circumplex; goodness-of-fit; bipolarity; pleasure; displeasure; activation

The study of the affective responses that accompany single bouts of physical activity is an area of research that has seen a resurgence of interest in recent years (Reed, 2005; Reed & Ones, 2006). Researchers have begun to explore important issues, including the dose-response relationship (Ekkekakis, Hall, & Petruzzello, 2008) and the role of individual differences (Ekkekakis, Hall, & Petruzzello, 2005a). The data emerging from the recent studies have meaningful practical implications. For example, as a result of the data that have emerged from this research, it is reasonable to expect that, in the years to come, the optimization of affective responses might become one of the key considerations in the process of developing exercise prescriptions, right alongside the maximization of effectiveness and safety.

Arguably, the driving force behind the recent advances in this area of research has been the transition to a new conceptual and methodological platform (Backhouse, Ekkekakis, Biddle, Foskett, & Williams, 2007; Ekkekakis, & Acevedo, 2006; Ekkekakis *et al.*, 2008; Ekkekakis & Petruzzello, 1999; Hall, Ekkekakis, & Petruzzello, 2002). The pillars of the new platform are (a) the assessment of affective responses along broad dimensions rather than distinct affective states (i.e., state anxiety, vigor, depression); (b) the assessment of affect at multiple time points, before, during, and after the activity bout, instead of using a pretest-posttest protocol; (c) the standardization of exercise intensity in terms of metabolic landmarks (i.e., the ventilatory or lactate threshold) instead of arbitrary percentages of maximal capacity; (d) the analysis of affective change not only at the level of the group

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aggregate (i.e., mean) but also at the level of individuals and subgroups; and (e) the examination of theory-based hypotheses.

What are the main challenges in measuring affect in exercise psychology?

Within the new investigative platform, the method of conceptualizing and assessing affect is of central importance given its critical role in determining the quality of the acquired data. Beyond the standard psychometric concerns of validity (i.e., having measures that faithfully represent the content domain of interest) and reliability (i.e., minimizing distortions due to measurement error), an important issue is that of content under-representation. This term refers to 'the degree to which a test fails to capture important aspects of the construct' (American Educational Research Association, American Psychological Association, & National Council on Measurement in Education, 1999, p. 10). This issue is extremely important for studies on the exercise-affect relationship and, in particular, those aimed at deciphering what effect different exercise stimuli have on affect. Unless the measure of affect that is employed offers a comprehensive depiction of the global domain of affect, it is possible that a certain combination of experimental conditions may induce changes in a sector of the affective space that is not covered by the measure. In that case, the researcher would be led to the erroneous conclusion that no changes in affect occurred when, in fact, a change did take place but went undetected due to the inadequate or inappropriate measurement approach. This issue was identified as early as 1984 by William Morgan, who wrote:

> Much, perhaps most, of the literature dealing with the psychologic effects of exercise has relied on the use of objective self-report inventories designed to measure constructs such as anxiety and depression ... The extent to which these inventories can tap the psychometric domain of significance to the exerciser has not been evaluated. In other words, an investigator may employ an objective, reliable, valid test of anxiety or depression to quantify the psychologic effects of exercise, only to find that no 'effects' have taken place when, in fact, there may have been numerous effects (p. 134).

Finding a solution to this problem is not easy. How could one possibly anticipate all the variants of affective experience that may result from the infinite combinations of experimental conditions (i.e., physiological and psychological participant characteristics, physical activity stimulus properties, physical and social environmental variables)? Researchers in exercise psychology have grappled with this problem in mainly two ways. One way has been by simply ignoring Morgan's (1984) warning and continuing to use self-report measures of a few distinct variables, such as state anxiety as assessed by the State Trait Anxiety Inventory (STAI; Spielberger, Gorsuch, & Lushene, 1970) or the six mood states assessed by the Profile of Mood States (POMS; McNair, Lorr, & Droppleman, 1971). Another way has been by assuming that all affective states that can occur in response to all conceivable combinations of experimental conditions that involve physical activity constitute a finite set, variously described as 'exercise-related affect' (Gauvin & Spence, 1998), 'exercise-induced feelings' (Gauvin & Rejeski, 1993), or 'subjective exercise experiences' (McAuley & Courneya, 1994). This assumption has led to the development of various measures of this 'exercise-specific' domain of content, including the Exercise-induced Feeling Inventory (EFI; Gauvin & Rejeski, 1993), the chronic form of the EFI (Rejeski, Reboussin, Dunn, King, & Sallis, 1999), a brief version of the EFI (Annesi, 2006), the Subjective Exercise Experiences Scale (SEES; McAuley & Courneya, 1994), and the Physical Activity Affect Scale (PAAS; Lox, Jackson, Tuholski, Wasley, & Treasure, 2000), which emerged by combining items from the EFI and the SEES.

Arguably, both approaches present considerable problems. The former clearly does not constitute a 'solution' *per se*, since it does not address the problem at all. It simply rests on the untenable assumption that state anxiety and/or a set of distinct mood states somehow can be an adequate 'proxy' for all variants of affective experience. The latter approach is also problematic since the item pool that was developed to represent the exercise-specific content domain was based only on the experiences of healthy and physically active people (Ekkekakis & Petruzzello, 2001a,b). Thus, the risk of domain under-representation was increased. The implications of this problem can be found in studies involving previously sedentary adults (Gauvin, Rejeski, Norris, & Lutes, 1997) or older, obese patients with knee osteoarthritis (Focht, Gauvin, & Rejeski, 2004). In these samples, it would have been reasonable to expect that exercise might have produced some degree of discomfort, displeasure, tension, or distress. Yet the researchers could only comment on the absence of widespread positive effects. Since no negatively valenced states were assessed, whether any negative affective changes occurred could not be determined.

Given the central importance of measurement in any research endeavor and the complexity of the issues involved in the particular context of exercise, it is not surprising that the conceptualization and measurement of affect has been a persistent source of debate and controversy in the field of exercise psychology (Ekkekakis & Petruzzello, 2000; Gauvin & Spence, 1998; Smith & Crabbe, 2000). However, since the 'head-in-the-sand' approach is unlikely to prove fruitful, the open exchange of ideas, arguments, and evidence is the only avenue that can reasonably be expected to lead to a solution or, at least, to some meaningful insights that can facilitate progress.

Where does the circumplex fit in?

A proposal advanced by several authors (e.g., Biddle, 2000; Biddle & Mutrie, 2001; Buckworth & Dishman, 2002; Ekkekakis & Petruzzello, 1999, 2002; Gauvin & Brawley, 1993; Lox *et al.*, 2000; Mutrie & Biddle, 1995; Rejeski *et al.*, 1999) has been to consider the potential offered by a dimensional model, namely the affect circumplex (Russell, 1978, 1980, 1989, 1997). According to this model, the differences and similarities between affective states can be accounted for in a parsimonious manner by two orthogonal and bipolar dimensions, namely affective valence (pleasure-displeasure) and perceived activation. The different affective states are, therefore, essentially considered as mixtures of different amounts of these two basic dimensions. For example, energy, excitement, and enthusiasm would be combinations of pleasure and high activation; tension, distress, and anger would comprise displeasure and high activation; calmness, relaxation, and tranquility would combine pleasure and low activation; and tiredness, boredom, and depression would consist of displeasure and low activation.

The proposal for the adoption of the circumplex model as a measurement platform in exercise psychology was based on several arguments (for a detailed presentation, see Ekkekakis & Petruzzello, 2002). Firstly, the circumplex focuses on the broadest construct in the affective hierarchy, namely basic affect, thus providing an appropriately broad investigative framework for the present stage of knowledge development in the field of exercise psychology. Secondly, as a dimensional model, it offers unparalleled breadth of scope and parsimony, theoretically permitting the assessment of the global domain of affect by tapping only two constructs, namely valence and activation. Thirdly, it is domain-general and, therefore, unlikely to yield assessments biased in favor or against a certain experimental condition or treatment (e.g., provide assessments that are more 'sensitive' to exercise as opposed to sedentary control or comparison conditions). Fourthly, it is supported by extensive empirical research and theorizing, thus allowing the development of measurement models based on a deductive or theory-driven approach.

Although these are considerable potential strengths, it has also been suggested that exercise psychology researchers should take into account certain important caveats. The circumplex should not be seen as a panacea, appropriate for all research questions and contexts, since its strength lies in its parsimony and not its specificity. In other words, the model's breadth of scope comes at a cost, namely that it cannot distinguish well between affective states within the same quadrant (e.g., both *anxious* and *angry* are high-activation, unpleasant states). Furthermore, the circumplex is a model of basic affect, not a model of emotion and mood (for more on the distinctions, see Ekkekakis & Petruzzello, 2000; Russell, 2003, 2005). Finally, with few exceptions (Ekkekakis, Hall, & Petruzzello, 2005b), the structure of most measures based on the circumplex model has yet to be formally evaluated by appropriate and rigorous statistical methods (Ekkekakis & Petruzzello, 2002).

These points have been acknowledged by several researchers in exercise psychology, who have started using the circumplex in a productive manner (e.g., Backhouse, Ali, Biddle, & Williams, 2007; Focht, Knapp, Gavin, Raedeke, & Hickner, 2007; Kilpatrick, Kraemer, Bartholomew, Acevedo, & Jarreau, 2007; Parfitt, Rose, & Burgess, 2006; Rose & Parfitt, 2007; Welch, Hulley, Ferguson, & Beauchamp, 2007). On the other hand, as expected, numerous studies continue to be based on either the STAI/POMS approach or on one of the exercise-specific measures. In these studies, the selection of measures is usually presented as a *de facto* decision, unaccompanied by a rationale. However, a critical reader could pose questions such as the following. Why would state anxiety be of interest in studies that did not involve trait-anxious participants and state anxiety was not experimentally manipulated? Why would one assume that the six mood states tapped by the POMS would suffice to capture the entire domain of mood? What evidence is there that the items or the scales of the EFI or the SEES provide a comprehensive representation of what Morgan (1984) called the 'psychometric domain of significance to the exerciser' (p. 134) under any given set of experimental conditions? These important questions remain unanswered. In lieu of a rationale, authors typically resort, explicitly or implicitly, to the justification that their measure of choice was selected because it 'has been used in previous research'. To state the obvious, this cannot be considered an adequate reason.

A quality-control checklist: a brief guide for authors, reviewers, and editors

It has been argued that, for real progress to be made in the study of the exercise-affect relationship, researchers should first 'begin to present theory-grounded rationales for selecting a particular measure of affect in their studies' (Ekkekakis & Petruzzello, 2000, p. 84). To fully articulate such a rationale, authors should provide enough information in their articles to answer the following three questions. Firstly, why is one focusing on a particular affective construct? To answer this question, researchers must have a good grasp of the conceptual distinctions between emotions, mood, and affect (Ekkekakis & Petruzzello, 2000; Russell, 2003, 2005). For example, it probably makes little sense to focus on a specific emotion (e.g., state anxiety), which assumes a specific model of causation (i.e., an interaction of trait characteristics, situational factors, and a precisely defined type of cognitive appraisal, involving a comparison between perceived demands and capabilities) when the purpose of the investigation is to describe 'how people feel' in response to an exercise bout (see Ekkekakis, Hall, & Petruzzello, 1999, for more on this).

Secondly, why was a particular conceptual model of the domain of interest selected? For each domain (i.e., emotion, mood, or affect), several conceptual models have been proposed to describe its content and structure. Each one is different and each presumably has strengths and weaknesses. For example, should one select a categorical model (in which each state is considered as a distinct entity) or a dimensional model (in which different states are considered systematically interrelated in a way that permits their relationships to be described by a set of underlying dimensions)? Importantly, can compelling evidence be presented to show that, by adopting a certain conceptual model, one is addressing the issue of content underrepresentation? For example, what evidence is there that tension, depression, anger, vigor, fatigue, and confusion adequately represent the content domain of 'mood'? Or what evidence is there that positive engagement, revitalization, physical exhaustion, and tranquility comprehensively represent the content domain of 'exercise-induced feelings'?

Thirdly, given the choice of a content domain of interest and an appropriate conceptual model for that domain, why was a specific measure selected? Obviously, this is where the traditional psychometric criteria of validity and reliability become relevant (but notice that this is only one out of three fundamental considerations in selecting a measure, not the sole consideration). How well does the content and structure of the measure match the content of the domain of interest and the structure postulated by the conceptual model? What evidence is there that measurement error is minimized, such that unreliability does not become a limiting factor to the validity of the measure?It is important to recognize that these questions, although they may seem difficult, are answerable. When they are answered (e.g., Backhouse *et al.*, 2007; Ekkekakis *et al.*, 2008; Ekkekakis, Hall, Van Landuyt, & Petruzzello, 2000; Hall *et al.*, 2002; Rose & Parfitt, 2007; Welch *et al.*, 2007), they help to clarify and strengthen the rationale upon which a study is based. Obviously, if all authors felt compelled to answer these three questions, the unfortunate cliché of 'this measure was used because it has been used before' would become a thing of the past.

The discussion continues

The purpose of the present paper is to contribute to the ongoing discourse on the important topic of the measurement of affect in exercise psychology by attempting an in-depth, critical analysis of three issues that pertain to the affect circumplex. The first issue deals with whether the circumplex model meets acceptable standards of goodness of fit. The second issue deals with the theorized bipolar nature of the dimension of affective valence (pleasure-displeasure). Emphasis will be placed on arguments in which the objection to the notion of bipolarity is based on claims of separable neural substrates for pleasure and displeasure. The third issue pertains to problems associated with the assessment of the dimension of perceived activation.

The central premise on which the present analysis is based is that, as is the case with most issues surrounding the measurement of affect, the answers to these questions are 'out there' in the literature, if one cares to seek them (Gray & Watson, 2007; Larsen & Prizmic-Larsen, 2006; Watson & Vaidya, 2003). This premise is consistent with what Dishman (1991) criticized as 'secondary ignorance', the ignorance resulting from the refusal or reluctance to delve into the knowledge base of related disciplines to find answers to technically complex questions, opting instead to perpetuate ideas and practices that scientific advances have rendered obsolete.

On the circumplex and goodness of fit

Echoing some authors in general affective psychology, Gauvin and Rejeski (2001) have argued that the 'major versions of this circular structure [i.e., the circumplex] ... fail to meet generally accepted standards for goodness of fit' (p. 75). In support of this statement, they cited an article by Watson, Wiese, Vaidya, and Tellegen (1999). In this article, Watson *et al.* reported a reanalysis of some of their data sets using Browne's (1992) CIRCUM software, which applies one of the very few available confirmatory procedures for testing circumplex structure (for others, see Tracey, 2000).Three levels of constraints can be tested with CIRCUM. First, the model can examine whether a circle can fit the data. If so, the correlations between the common score variables should be a function of the angles of separation between the common score variables. For example, in the theoretical case of error-free data, the correlation between two common score variables could be estimated by the cosine of the angle of separation between these variables (e.g., a correlation of .707 for variables that are 45° apart). Second, researchers can test one of two additional constraints, namely whether the variables that are 180° apart have a perfect −1.00 correlation and whether the variables are equally spaced around the circle (i.e., every 45° in the case of a circumplex divided in eight octants). Note, however, that, although these constraints are offered as options in CIRCUM, neither one is necessary for a circumplex. Regarding the former constraint, for example, Russell and Carroll (1999) demonstrated that, for a strictly unipolar response format and error-free data, variables that are 180° apart (i.e., the two poles of a bipolar continuum, such as pleasure-displeasure) show a −.467 correlation, not -1.00. Regarding the latter constraint, Russell's (1980) initial conceptualization and analysis of circumplex data provided no indication that affect terms should be expected to cluster at equally spaced intervals (i.e., at 0°, 45°, 90°, 135°, 180°, 225°, 270°, 315°, and 360° angles). Instead, the terms can occupy a variety of angular locations along the circle; the important question is whether the relationships between the variables can be represented as a circle. In CIRCUM, researchers can impose all the aforementioned constraints simultaneously, although this model falls under the category of what Gurtman and Pincus (2000) called 'unrealistically restrictive and hence unlikely to be confirmed' (p. 376).

D. Watson *et al.* (1999) used CIRCUM to test models with and without the two additional constraints. For ratings of current, momentary affect, the root mean squared error of approximation (RMSEA) was .094 and .157 for two sets of items without the aforementioned additional constraints and .234 and .292, respectively, with the additional constraints. Gauvin and Rejeski (2001) noted that these indices of model fit range from marginal to poor. In evaluating these results, however, researchers should take into account the following important points.

The first issue should already be apparent from the foregoing discussion. Only the .094 and .157 indices are relevant for assessing the fit of the circumplex, since the additional constraints of equal spacing and −1.00 minimum common score correlation are not necessary for a circumplex.

The second issue has been recognized for many years in the psychometric literature on circumplex models. According to Wiggins, Steiger, and Gaelick (1981):

> Any complex structural hypothesis ... which imposes a large number of constraints is, strictly speaking, almost certainly false. Thus, it is highly likely that a given sample correlation matrix will exhibit some departure from circulant form and ... 'rejection' of a complex correlational hypothesis is inevitable. When interpreting the goodness of fit results, one should, realistically, be more concerned with the severity of departure from a circulant pattern than with whether there is *any* such departure (pp. 270–271).

In other words, it is unrealistic to judge the goodness of fit of a circumplex model by the same standards used to judge the outcome of most conventional confirmatory factor analyses, which are typically much less restrictive by comparison.

Third, it should be noted that the results reported by D. Watson *et al.* (1999) are not representative of others in the literature. For example, a CIRCUM analysis of an earlier data set (Russell, 1980) yielded an RMSEA of .06 and a minimum common score correlation of −.86 (Fabrigar, Visser, & Browne, 1997). It is also important to point out that several factors may influence the goodness of fit of circumplex models. In the most systematic effort in this direction to date, Remington, Fabrigar, and Visser (2000) reanalyzed all the data sets that they were able to retrieve and examined the influence of five factors: (a) whether judgements referred to states or traits, (b) whether ratings referred to frequency or intensity, (c) whether data were based on single-item or multiple-item measures, (d) whether the measures included items that were ambiguous (not affective or not relevant to the circumplex), and (e) whether items represented some or all octants of the circumplex space. Based on 47 analyzed correlation matrices, RMSEA ranged from .000 to .242. Importantly, the analyses showed that better fit is attained when judgements are based on affective states rather than traits, the intensity of affect, single-item measures, items sampled for all octants of the circumplex space, and when measures included only a few ambiguous items. When the analyses focused on the 10 matrices that satisfied these criteria, most RMSEA indices showed a satisfactory fit (.047, .048, .053, .060, .065, .072, .075, .085, .091, .121). This is an important finding, since studies examining affective responses to acute exercise involve states rather than traits and ratings of intensity rather than frequency of affect. Furthermore, it is only reasonable that better fit is obtained when all the sectors of the circumplex are represented and when there are few ambiguous items. It should be recognized that, if these conditions are not met, the responsibility for the lack of fit does not lie with the model itself, but rather with the poor methodological choices made by researchers.

The fourth issue is an extension of the previous one. Most of the models tested in the literature until now have been based on lists of affective terms that have been assumed, on the basis of their literal meaning, to 'belong' to certain sectors of the circumplex (e.g., Larsen & Diener, 1992; Watson & Tellegen, 1985). It is crucial to keep in mind that, for most of these terms, the assumption regarding which octant each item belongs to has not been verified by empirical study. For example, the item *active* has been assumed to belong to the *high activation* octant, denoting neither positive nor negative valence (e.g., Larsen & Diener, 1992). However, it should not be surprising that most people perceive it instead as indicative of *pleasant high activation* (e.g., Remington *et al.*, 2000). Again, it is reasonable to suggest that, if an item like *active* is not found to be consistently associated with a hypothesized location in circumplex space, the fault may not be in the theoretical model itself but rather in the items chosen to represent parts of the model and in the erroneous assumption made by researchers regarding the meaning that the item conveys in colloquial language.

Remington *et al.* (2000) used CIRCUM to examine the angular location of 71 terms derived from 10 correlation matrices from the circumplex literature and compared these empirical locations to 'commonly predicted locations' (p. 294) on the basis of previously published schematics (e.g., Larsen & Diener, 1992; Russell, 1980; Watson & Tellegen, 1985). They found that none of the items 'commonly predicted' to belong to three of the eight octants (low activation, high activation, pleasant high activation) were found to consistently fall within the predicted sectors in the 10 matrices studied. This means that, of the terms that were examined, there were no consistently reliable markers for these sectors.

As noted earlier, one plausible interpretation is that the 'commonly predicted locations' might have been incorrect. For instance, the items *energetic* and *wide-awake* were predicted to denote *high activation*, with neither positive nor negative valence. However, other authors (e.g., Thayer, 1989) regard these terms as indicative of *pleasant high activation* and the empirical findings supported this prediction. Likewise, the item *intense*, which was predicted to belong to the *high activation* octant on the basis of the opinion of some authors (Larsen & Diener, 1992) was found to belong to the *unpleasant high activation* octant, as others had predicted (Thayer, 1989). Furthermore, the analysis by Remington *et al.* was limited in several other respects, including the fact that (a) some items were contained in only one matrix and (b) one problematic octant (low activation) was represented by only three possible items.

Remington *et al.* (2000) offered two potential explanations: 'One possibility is that the model is fundamentally wrong in that no affective states exist that fall in these regions. Alternatively, such states might exist but were not included in the matrices' (p. 296). In their critique of the circumplex, Gauvin and Rejeski (2001) adopted the former interpretation, noting that Remington *et al.* 'failed to find any affective states that consistently fell in three of the eight regions of the circumplex and other states were not as stable as one might expect if the circumplex model is correct' (p. 76). It could be argued, instead, that Remington *et al.*'s first interpretation is misleading. There is no theoretical or mathematical reason to believe that, for the circumplex model to be valid, there must be items in all of its regions. In other words, not all theoretical states within the circumplex need to have their lexical equivalents (although this would be convenient for measurement purposes). It is also important to remember that, even if such lexical equivalents did exist, they need not be parts of the working vocabularies of a given sample of respondents. In sum, several aspects of the analysis and interpretation by Remington *et al.* are debatable and, their study, by itself, cannot be considered a substantial challenge to the circumplex model.

The fifth issue is also closely related to the previous ones. Gauvin and Rejeski (2001) correctly characterized the circular stochastic process model with a Fourier series (Browne, 1992), the statistical model on which the CIRCUM software is based, as 'the best statistical model currently available to test the circumplex' (p. 76). However, it should be clear that the outcomes of CIRCUM analyses can only be as good as the data entered in these analyses and the theoretical predictions involved. In other words, just like factor analysis, CIRCUM analysis follows the infamous GIGO (garbage in, garbage out) principle. The merits of the analytic model cannot counterbalance problems created by questionable methodological choices. Some of the implications of this issue were examined in the previous paragraph but one could point to a few more examples. First, Watson *et al.* (1999) used total 'octant' scores, based on three or four items per octant, rather than individual items, in their analyses. Again, however, the classification of items in different octants was based on subjective judgements, leading to a number of potential problematic choices. For example, the items *alert* and *wide awake* were classified as *activation* items, although there is evidence that they are perceived as indices of *pleasant high activation* (e.g., Remington *et al.*, 2000). The item *distressed* was classified as a *unpleasantness* item, although others view it as an index of *unpleasant high activation* (e.g., Larsen & Diener, 1992; Remington *et al.*, 2000). The item *downhearted*, a synonym of *depressed*, also classified as an *unpleasantness* item, could be perceived as an index of *unpleasant low activation*. The items *sad*, *gloomy*, and *blue* were all classified as *unpleasant low-activation* items, although they are considered by others as indices of *unpleasantness* (e.g., Larsen & Diener, 1992;

Remington *et al.*, 2000). It should be obvious that these subjective decisions could have had a decisive impact on the outcome of the analysis.

An additional problem is also worth pointing out. Remington *et al.* (2000) reported finding a group of items with ambiguous locations in circumplex space, including *proud*, and *guilty*. It is important to reiterate a point made earlier, namely that the circumplex is most appropriate for the study of basic affect, not the study of emotions. The states *proud* and *guilty*, although they might have been considered relevant to the circumplex by some researchers, are emotions (Lazarus, 1991). As others have also emphasized, the circumplex 'represents and is limited to the core affect involved' (Russell & Feldman Barrett, 1999, p. 807). It should be clear that the inclusion of terms that are not relevant to the circumplex model in a CIRCUM analysis would likely lower the fit of the model. To illustrate, two respondents may indicate that they both 'feel good'. However, one of them feels good as a part of feeling 'proud' (a complex emotional state, implying a positive cognitive evaluation of oneself), whereas the other just feels good (implying only the presence of core or basic affect, not a positive evaluation of himself or herself or any other cognitive appraisal). Including the emotion item 'proud' in an analysis of the affect circumplex creates problems because, although one might expect *feeling good* and *proud* to be close, the ratings on these two items need not show a strong correlation.

Sixth, as has been demonstrated by several authors (e.g., Carroll, Yik, Russell, & Feldman Barrett, 1999; Feldman Barrett & Russell, 1998; Green, Goldman, & Salovey, 1993; Green & Salovey, 1999; Green, Salovey, & Truax, 1999; Russell & Carroll, 1999), random and systematic measurement error can substantially influence the outcomes of structural analyses that pertain to the circumplex model (see Ekkekakis & Petruzzello, 2001b, for a detailed review on this issue). In many analyses, including those conducted by Watson *et al.* (1999) and Remington *et al.* (2000), the impact of measurement error, particularly systematic error, was not taken into consideration.

In sum, the argument that the circumplex model fails to meet generally accepted standards for goodness of fit does not seem to provide a balanced account of the evidence. In the context of exercise, Ekkekakis *et al.* (2005b) used CIRCUM to test the circumplex structure of the items of the Activation Deactivation Adjective Check List (AD ACL; Thayer, 1989) before and after a short walk. The RMSEA was .041 before the walk (indicating a 'close' fit) and .076 after the walk (indicating a 'reasonable' fit). These goodness of fit indices were found despite the fact that, as Ekkekakis *et al.* (2005b) noted, 'the AD ACL was not developed with the circumplex in mind and, consequently, the location of the items on the circle was not used as an item selection criterion in the course of its development' (p. 96). As it must have become apparent by the foregoing discussion, fitting the circumplex model to data is a complex undertaking. Therefore, researchers are urged to take the caveats that were presented here into account in evaluating the empirical evidence.

On separable neural substrates of pleasure and displeasure and implications for bipolarity

Authors both in general and in exercise psychology have questioned the validity of the notion of bipolarity between pleasure and displeasure, which is central to the circumplex model. For example, Gauvin and Rejeski (2001) wrote that 'even Russell and Carroll (1999) have indicated that a two-dimensional representation of the semantics of affect is oversimplified ... These same authors point to data suggesting that the neurological processes underlying positive and negative affect are distinct' (p. 76). Russell and Carroll (1999), in turn, were referring to an earlier opinion by Cacioppo and Berntson (1994) that positive and negative affect may be subserved by separable neural systems. Parenthetically, it should be noted that, although Cacioppo and Berntson refer to 'positive affect' and 'negative affect,' what they mean is pleasure and displeasure, not 'Positive Affect' and 'Negative Affect' in the sense that the terms have been used by Watson and Tellegen (for the differences between pleasure-displeasure and Positive Affect-Negative Affect, see Ekkekakis and Petruzzello, 2001b). Russell and Carroll interpreted this view as suggesting that 'even if [positive affect] and [negative affect] are typically bipolar, they might be separable in specific circumstances' (p. 26). Although the existence of separable neural substrates could entail the independent and, therefore, non-reciprocal activation of positive and negative affect (pleasure and displeasure), this possibility must be considered in the context of several caveats. Some of these refer to the conceptual aspects of the interpretation by Cacioppo and his co-workers and some refer to the neuroscientific evidence itself.

First, the claim that the presence of separable neural substrates entails the independence of positive and negative affect is not straightforward and must be examined cautiously, taking into account a number of assumptions. As Russell and Carroll (1999) noted, findings pointing to separable neural substrates suggest that positive and negative affect *could* be separable in certain circumstances, not that these findings entail that they always and necessarily are. This is in line with the position taken by Cacioppo and his co-workers. Cacioppo and Gardner (1999), for example, wrote that, if there are separable neural networks that correspond to two independent evaluative channels, then we should expect three modes of activation:

> (a) reciprocal activation occurs when a stimulus has opposing effects on the activation of positivity and negativity; (b) uncoupled activation occurs when a stimulus affects only positive or only negative evaluative activation; and (c) nonreciprocal activation occurs when a stimulus increases (or decreases) the activation of both positivity and negativity. This model thus does not reject the reciprocal activation that is assumed in subjective reports of affect, and demanded in behavioral manifestations of affect, but rather subsumes it as one of the possible modes of activation (p. 201).

As a first observation, briefly setting aside the issue of extrapolating between neural substrates and affective responses, it is important to have a realistic understanding of just how frequently each of these alternative modes of affective responses is likely to occur. Cacioppo and his co-workers have offered some evidence to this effect. In a study of the relationship between positive and negative responses to affective slides, the vast majority of participants (n = 239) 'consistently showed a reciprocal relation in their positivity and negativity scores', whereas very few (n = 8) 'showed a consistently uncoupled relation between the valent systems' (Ito, Cacioppo, & Lang, 1998, p. 876). As Larsen, McGraw, and Cacioppo (2001) have noted, 'we do agree with Russell and Carroll (1999) that individuals typically feel either happy or sad a great deal of the time' and, thus, they only posed the question whether it is possible for individuals to feel both happy and sad at the same time under 'more emotionally complex situations' (p. 687). Examples of such complex situations that were investigated by Larsen *et al.* and shown to induce both happiness and sadness in percentages of individuals that ranged from 44% to 54% include watching a 'bittersweet' film (*Life is Beautiful*, which included both dramatic and comedic elements), moving out of a college dormitory at the end of freshman year (at which time, students are likely to experience nostalgia for leaving behind the friendships that were formed during the year but also look forward to the summer break), and graduating from college (at which time, the new graduates may experience both hopes and fears about the future). Other examples of 'mixed' or 'conflicting' emotions that have been used by

Cacioppo and his collaborators include love and bitterness toward one's parents, conflicting attitudes, such as approval and attraction or disapproval and resentment, toward different aspects of a political candidate's platform, and getting a raise when an even larger raise was expected (Larsen, McGraw, Mellers, & Cacioppo, 2004).

The second and more crucial problem becomes evident when one compares situations such as those listed above, which are offered as examples of the independence of positive and negative affect by Cacioppo and his collaborators, to the affective states that occur in the context of a bout of exercise. An introspective exercise should reveal that these are substantively distinct entities. Russell and Feldman Barrett (1999) noted that 'this dimensional structure [i.e., the circumplex] represents and is limited to the core affect involved' (p. 807) and, consequently, emphasized that the circumplex is an inadequate template for studying constructs at more complex levels of the affective hierarchy, particularly those that depend on cognitive antecedents (Ekkekakis & Petruzzello, 2002). Russell and Feldman Barrett (1999) have also pointed out that Cacioppo and his collaborators, in arguing for a bivariate evaluative space, 'discuss not core affect, but evaluative reactions to a stimulus' (p. 813), which is a fundamentally different concept.

Although Cacioppo and his collaborators often use the term 'affect', the types of states and situations they describe make it clear that they mostly refer to such evaluative concepts as appraisals and attitudes, which extend well beyond the domain of core affect, as described by Russell and Feldman Barrett. Larsen *et al.* (2001) acknowledged that, although they agree that evaluative reactions and affect are distinct, they 'do not draw such sharp divisions between classes of affect' (p. 687). To illustrate the problems that could result from failing to distinguish between the different levels of the affective hierarchy, consider the following. Larsen *et al.* (2001) asked people to recall situations in which they felt both happy and sad at the same time. One woman wrote 'My best friend and I were up for a part in a play (the same part). She got the part so I was happy for her but sad also that I didn't get it' (p. 694). Exercise psychology researchers should consider the qualitative differences between 'affect' of the kind referred to in this anecdote and the affective responses typically elicited by exercise (e.g., feeling energetic, calm, tense, tired, etc). Arguably, feeling happy for the success of a friend or sad for one's lost opportunity to play a theatrical role (presumably following complex cognitive appraisals) are qualitatively distinct from the typical affective responses that accompany exercise. The difference is that the former states rely, at least to some extent, on symbolic cognitive appraisal, whereas the latter can and, in most cases, do arise directly from the body (although they may be influenced by cognition, they do not necessarily originate from it).

Exercise is a multifaceted stimulus and, as such, 'it has the capacity to induce affective responses emerging from any level of affective processing, from basic affect to specific emotions' (Ekkekakis & Petruzzello, 2000, p. 78). Although it is conceivable that a person may experience conflicting responses to exercise, it seems reasonable to suggest that, in such cases, at least one of these responses will originate from a cognitive appraisal. For example, a colleague, who is also a renowned ultra-endurance athlete, once asked how the circumplex deals with conflicting experiences, as in anecdotal accounts that exercise 'hurts so good' (see Acevedo, Kraemer, Haltom, & Tryniecki, 2003). Something similar has been reported by Pronk, Crouse, and Rohack (1995) in a study that involved middle-aged women who participated in a maximal treadmill test. These researchers found that this exercise protocol resulted in parallel increases in both fatigue and self-esteem. A reasonable interpretation is that the 'hurt' and the fatigue probably reflect the immediate effects of the bodily sensations that accompany strenuous physical effort (i.e., basic affect). On the other hand, feeling 'good' and reporting a boost in self-esteem probably stem from the thought (i.e., the cognitive appraisal) that one has responded to a challenge or has done something beneficial for his or her health or fitness. In sum, although one cannot dismiss the possibility of a non-reciprocal relationship between pleasure and displeasure under certain complex and rare conditions, bipolarity appears to be the norm when responses reflect basic or core momentary affective experiences. Consequently, in evaluating the validity of the notion of bipolarity, it is crucial to first be clear about the level of the affective hierarchy that is being considered.

To return to the issue of the neural substrates, the argument put forth by Cacioppo and co-workers regarding the separable neural substrates of positive and negative affect is based on the belief that the mesolimbic dopaminergic pathway projecting from the ventral tegmental area of the midbrain to the nucleus accumbens is selectively implicated in positive affect, whereas the amygdala is selectively implicated in negative affect (Cacioppo & Gardner, 1999; Cacioppo, Gardner, & Berntson, 1999). Regarding this premise, an important third caveat worth considering is that the interconnectedness of brain systems makes any characterizations of distinct anatomical areas as 'pleasure centers' or 'displeasure centers' somewhat precarious. Gray (1991), one of the first to propose separable brain networks responsible for approach, withdrawal, and fight or flight, was keenly aware of this problem when he cautioned that the idea of separable subsystems of the brain mediating different kinds of affective responses is likely to raise objections 'on the grounds that any part of the brain put into one subsystem is in fact also connected to a myriad others' (p. 274). Certainly, the nucleus accumbens and the amygdala, the supposed substrates of pleasure and displeasure, respectively, are known to share an extensive network of connections (e.g., Friedman, Aggleton, & Saunders, 2002).

Fourth, labeling certain brain areas as 'pleasure centers' or 'displeasure centers' appears to be an oversimplification contradicted by a substantial body of evidence. With respect to the claim that the nucleus accumbens is selectively involved in positive affect or 'reward', the conflicting evidence is voluminous and has been the subject of several reviews (e.g., Ikemoto & Panksepp, 1999; Salamone, 1994; Salamone & Correa, 2002; Salamone, Cousins, & Snyder, 1997). In one example, Becerra, Breiter, Wise, Gonzalez, and Borsook (2001) found an initial increase and a subsequent decrease in the activation of the nucleus accumbens in response to a noxious thermal stimulus. Likewise, there is accumulating evidence that the amygdala is activated in response to both pleasant and unpleasant stimuli (see the meta-analyses of Sergerie, Chochol, & Armony, 2008; Costafreda, Brammer, David, & Fu, 2008; and the narrative reviews of Baxter & Murray, 2002; Murray, 2007). For example, Taylor, Liberzon, and Koeppe (2000) found amygdala activation in response to non-aversive, complex visual stimuli. Zalla *et al.* (2000) found increases in left amygdala activation in response to winning on a simulated computer game. Hudry, Ryvlin, Royet, and Mauguière (2001) found amygdala activation in response to both pleasant and unpleasant odorants. Hamann, Ely, Grafton, and Kilts (1999) showed that amygdala activation during memory encoding is correlated with the subsequent recall of both pleasant and unpleasant visual stimuli. Hamann and Mao (2002) reported significant left amygdala activation in response to both positive and negative emotional words. Hamann, Ely, Hoffman, and Kilts (2002) found left amygdala activation while viewing highly arousing pleasant images. Sander and Scheich (2001) showed that listening to both laughing and crying, as well as self-inducing the corresponding emotions while listening to laughing and crying, led to activation of the amygdala. In rats, Schoenbaum, Chiba, and Gallagher (1998) found activation of amygdala neurons in anticipation of both rewarding and aversive outcomes. In monkeys, Paton, Belova, Morrison, and Salzman (2006) found that amygdala neurons responded to stimuli with both positive and negative value (acquired through conditioning). Collectively, these findings seriously challenge the assertion that there are clearly distinct and independently functioning areas responsible for pleasure and displeasure in the brain.

The fifth problem stems from the principle that, although consciousness may be spatially distributed in the brain (i.e., subserved by multiple assemblies of neurons), it is experientially unitary at any given point in time (Greenfield, 1995). In other words, the existence of separable neural systems for positive and negative affect does not necessarily entail that an individual would be able to *experience* both kinds of affect simultaneously. In the words of Russell and Feldman Barrett (1999): 'Core affect might involve multiple and functionally independent neural mechanisms that need not themselves be bipolar. Nevertheless, bipolarity may emerge in forming conscious affective feelings' (p. 813). Cacioppo and Gardner (1999) have acknowledged this, at least in reference to the unity of behavioral expressions. They noted that, although there may be 'at least two specialized evaluative channels that process information in parallel – one in which threat-related (i.e., negative) information is derived from the flow of sensation and a second in which safety and appetitive (i.e., positive) information is derived', behavioral expressions vary on a bipolar continuum (i.e., either approach of withdrawal) because 'the outputs of all the evaluative processors comprising the affect system are combined in order to compute preference and organize action' (p. 201). A similar idea is also at the core of a conceptualization that should be familiar to researchers in exercise psychology, namely Solomon's (1980, 1991) opponent process model of affect. Solomon theorized that affective valence (or 'hedonic tone') is jointly determined by two processes (the *a*-process and the *b*-process) having opposite valence signs. In the case of vigorous exercise, the *a*-process is charged with negative affective valence (possibly reflecting the function of a system responsible for visceral or interoceptive nociception) whereas the *b*-process is charged with positive affective valence (possibly reflecting the function of the endogenous opiate system). The affect arising at any point during and following the exposure to a stimulus is theorized to reflect the algebraic sum of the two processes, a function that Solomon attributed to a hypothetical 'affect summator'. A mechanism of this sort is necessary because, although two separate processes are needed to account for the pattern of affective responses to a stimulus, the experience of affect at any given point is unitary.

Finally, it is again important to keep in mind that the relationship between pleasure and displeasure at the level of self-report (i.e., whether the relationship will sway toward bipolarity or independence) can be influenced by a host of factors, such as the location of the analyzed items along the dimensions of valence and activation, the time frame of the responses, the (affectively charged versus mundane) context of the assessment, the unreliability of measurement, and the format of the response scales (see Ekkekakis & Petruzzello, 2001b, for a review). In sum, the physiological argument commonly cited by Cacioppo and his co-workers (e.g., Cacioppo & Berntson, 1994; Cacioppo & Gardner, 1999; Cacioppo *et al.*, 1999; J.T. Larsen *et al.*, 2001, 2004) and echoed by Gauvin and Rejeski (2001) in support of the notion of the independence of pleasure and displeasure does not accurately represent the available evidence.

On activation as a cognitive, behavioral, or physiological construct

Of the two dimensions of the circumplex, affective valence and perceived activation, there is little doubt that activation presents more challenges for researchers. For example, Parfitt *et al.* (2006) have noted that perhaps additional training and familiarization with self-report measures of perceived activation might be necessary since different participants may exhibit different levels of comprehension or ability in using such instruments.

Gauvin and Rejeski (2001) also argued that controversy surrounds the dimension of activation, as it is described as ranging from low to high arousal by some (e.g., Russell, 1980) but from disengagement to strong engagement by others (e.g., Watson & Tellegen, 1985). Gauvin and Rejeski's interpretation was that the former description has physiological connotations whereas the latter has cognitive and behavioral ones. Based on this assertion, they claimed that, if activation is not 'physiological' but rather 'cognitive or behavioral' (as in their interpretation of *engagement*), then measuring perceived activation by a single-item rating scale during exercise could possibly tap only the *potential* for high arousal but not *actual* high arousal. If that were the case, then 'investigators would [not] be able to identify immediate evidence of high arousal' (p. 76). Gauvin and Rejeski's conclusion, based on this syllogism, was that the use of ratings of perceived activation in applied exercise research (e.g., Van Landuyt, Ekkekakis, Hall, & Petruzzello, 2000) is 'flawed' (Gauvin & Rejeski, 2001, p. 76).

First, the claim that the term 'arousal' has physiological and not cognitive or behavioral connotations seems inconsistent with extant theoretical views. Even early theorists, such as Duffy (1957) and Malmo (1959), consistently referred to physiological 'indicants' of arousal, suggesting that the concept of arousal itself is broader. Heilman and R.T. Watson (1989) clarified that the distinction between physiological and behavioral arousal only refers to the different perspectives from which the subject has been approached and studied.

The distinction between arousal as a physiological construct and engagement as a cognitive or behavioral one is also absent from the writings of the authors that have used these terms in conjunction with the circumplex. Russell (1989) has explicitly opposed the idea that arousal is only a physiological concept. Commenting on Schachter's (e.g., 1964) model of emotion, he noted that there is a problem in 'speaking of arousal in strictly physiological terms' (p. 105) and that 'there is no more reason to speak of arousal as strictly physiological and pleasure-displeasure as strictly mental than there is to express it the other way around' (p. 106).

Watson and Tellegen (1985), discussing the second principal component that emerged from their structural analyses of affective words, stated that this 'is clearly the Arousal factor (we have called it Strong Engagement-Disengagement) that has been consistently reported' (p. 222). Although this excerpt suggests that arousal and engagement were seen as interchangeable labels referring to the same concept, Tellegen (1985) noted that 'the "engagement" terminology is used here in some preference over "arousal" language to convey that these descriptors refer not only to activation but to characteristic cognitive modes and relationships with the environment' (p. 690). Tellegen explained this by stating that 'affectively engaged states are often cognitively unsettled and future oriented' whereas 'affectively disengaged states are cognitively more settled and past-centered' (p. 704). This line of reasoning perhaps makes more sense in light of the controversial argument first put forth by Zevon and Tellegen (1982) and since supported by both Tellegen and Watson that 'emotion or affect' necessarily entails 'a state of arousal or engagement' (p. 112). Consequently, they proposed that only high-arousal or 'engaged' states qualify as affects, whereas low-arousal or 'disengaged' states denote the absence of affect. This view is seen as highly controversial and several theorists have expressed their opposition (e.g., Carver, 2001).

The important point for the present discussion is that the distinction between arousal as a physiological construct and engagement as a cognitive or behavioral one seems arbitrary and different from the view held by Tellegen and Watson. What Tellegen proposed was a more specific interpretation of the dimension of activation, but there is no indication that the term *engagement* was used to describe a substantively different dimension than that of *arousal* or *activation*. After all, it should be kept in mind that Watson and co-workers (Watson & Tellegen, 1999; D. Watson *et al.*, 1999) have renamed the dimensions of Positive Affect and Negative Affect as Positive *Activation* and Negative *Activation*, respectively. In particular, the basis for Gauvin and Rejeski's claim that *engagement* could be interpreted as only the potential for arousal and not actual arousal is unclear. Given the lack of support for this premise, Gauvin and Rejeski's conclusion that the use of ratings of perceived activation is 'flawed' (p. 76) seems questionable. Using ratings of perceived activation has been the standard methodological approach for over two decades (see review of measures in Ekkekakis & Petruzzello, 2002). Besides, in the case of exercise, an activity that clearly does not involve the potential for arousal but rather actual and perceived arousal, this criticism does not appear relevant.

Nevertheless, researchers should be aware that the assessment of the dimension of activation does present some challenges (see Ekkekakis & Petruzzello, 2002, for more on this). As Watson *et al.* (1999) noted, there is a relative shortage of items that are high or low in terms of activation and neutral in terms of valence. It should be kept in mind that valence has consistently been the strongest component to emerge from structural analyses, since most affect words connote positive or negative valence. Thus, despite the fact that arousal also consistently emerges as the second underlying dimension, finding pure markers of activation has been difficult. Although this is not an inherent weakness of the circumplex as a conceptual model (i.e., the presence of such pure markers is not required for the model to be valid), it is a fact that poses a challenge to those wishing to assess the activation dimension through self-report, particularly through multi-item instruments. Furthermore, it is important to remember that, as reported by Feldman (1995), there are individual differences in the ability to make discriminations along the dimension of valence and the dimension of activation, with most people assigning more weight to the former than to the latter.

Conclusion

The circumplex model of affect has a history that extends to the beginning of the previous century. Despite differences in terminology, analytic strategies, and conceptual interpretations or preferences, the basic postulates of this structure (i.e., the two orthogonal dimensions of pleasure-displeasure and activation, the bipolarity of pleasure-displeasure, etc) are reliably supported by empirical evidence and form the basis of an increasingly strong consensus in affective psychology (Feldman Barrett & Russell, 1999; Posner, Russell, & Peterson, 2005; Yik, Russell, & Feldman Barrett, 1999). What is facilitating the emergence and fortification of this consensus is the acknowledgment of some important distinctions within the affective domain (see Ekkekakis & Petruzzello, 2002; Russell, 2003, 2005; Russell & Feldman Barrett, 1999, for reviews). Although from time to time the circumplex has been promoted as a conceptual model of all the major constructs under the affective umbrella, including emotion, mood, and affect, it is becoming clear that this lack of specificity has not served the field well. Dimensional models in general and the circumplex in particular are not the most fruitful conceptual approach for the study of each and every one of these constructs. As emphasized by Russell and Feldman Barrett (1999), 'this dimensional structure [i.e., the circumplex] represents and is limited to the core affect involved' (p. 807) and, because the 'structure of core affect is much simpler than the structure of prototypical emotional episodes' (p. 809), 'assessment devices based on the dimensional-circumplex approach capture core affect but miss ... other components' (p. 807). Such 'other components' include, among others, attention toward the eliciting stimulus, a cognitive appraisal of the meaning of that stimulus for the survival and well-being of the individual, behavioral expressions or action tendencies, coping responses, and a multitude of physiological changes.

The distinction between *core* or *basic affect* as a relatively simple construct and *emotions* or *prototypical emotional episodes* as relatively more complex constructs, which, in addition to affect, include other important components essentially entails an hierarchical structure. The basic dimensions of pleasure-displeasure and activation can offer a parsimonious account of the differences at the level of *core* or *basic affect* and traditional categorical models can account for the subtle differences at the level of *emotions* or *prototypical emotional episodes* on the other end (Ekkekakis & Petruzzello, 2002; Russell & Feldman Barrett, 1999).

The debate in affective psychology about the structure of affect, which was revived in recent years, in addition to bringing some points of agreement in sharper focus, has underscored one important consideration: that sweeping generalizations and characterizations of one or another conceptualization as 'correct' or 'incorrect' underestimate the true complexity of the issues and, as such, are misguided. The circumplex model of affect has gone past the point at which a single study could render it 'incorrect'. Therefore, approaching this issue with the expectation of finding answers that conform to a simple 'right or wrong' dichotomy is unlikely to prove fruitful. Instead, before reaching a conclusion, one should carefully contemplate the evidence that has been uncovered about the properties of this dimensional structure, including both its considerable strengths and its important limitations.

Finally, a note seems warranted regarding the extent to which the debate within exercise psychology on the measurement of affect has or has not kept pace with developments in 'mainstream' affective psychology and related fields. The view of exercise psychology as insular territory and the ensuant isolationist attitude are alarmingly counter-productive tendencies, particularly with respect to a topic with such a rich tradition in psychology as affect. Arguably, delving into the technically and conceptually complex matters surrounding the study of affect could make this line of research within exercise psychology more challenging and, therefore, perhaps less accessible. However, it is important to recognize that there is a critical difference between (challenging but meaningful) complexity and trivial minutia. Summarily dismissing decades of psychological research and theorizing as unimportant, irrelevant, or too esoteric to be worth our time may create a comforting illusion of simplicity but it does not necessarily offer the best solution for the field. One has to wonder, for example, why the study of the affective changes associated with exercise, amounting to several hundred publications, has received very little recognition in and has had virtually no impact on other branches of psychology. For the study of the affective responses to exercise to continue to evolve, taking on the complexity is obligatory. It could be argued that, after more than four decades of research, rising to the challenge has already taken long enough.

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