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GUEST EDITORIAL

The evolution of the 'components model of addiction' and the need for a confirmatory approach in conceptualizing behavioral addictions

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ABSTRACT

Many psychometric instruments assessing the risk of developing various behavioral addictions have been developed over the past decade based upon Griffiths' (2005) 'components model of addiction'. This paper briefly examines the evolution of the components model and argues that some of the components are not "peripheral" to addiction (as some have argued) but that the problem lies in the operationalization of some of these components in many psychometric instruments (because items in such instruments do not necessarily include negative wordings in operationalizing the components). The paper also argues that the confirmatory approach for identifying those at risk of behavioral addictions (i.e., classifying behaviors on the basis of criteria of substance use disorders or behavioral addictions such as gambling disorder) is the best way to unify to the behavioral addiction field rather than bespoke idiosyncratic criteria.

Over the past decade, the 'components model of addiction' (1) has become highly cited in the field of behavioral addiction and has been used in the development of many psychometric instruments assessing the risk of developing various behavioral addictions including exercise addiction (2), gaming addiction (3), work addiction (4), social media addiction (5), Facebook addiction (6), YouTube addiction (7), Tinder addiction (8), shopping addiction (9), pornography consumption (10), sex addiction (11), love addiction (12), dance addiction (13), tanning addiction (14), and problematic television series watching (15).

The components model of addiction

The evolution of the components model of addiction has often been misattributed to the psychologist Iain Brown. For instance, a recent paper by Billieux et al. (16) made reference to the "[addiction] components in Brown's model." However, Brown never put forward a model relating to components of addiction although he did influence the model I later developed. Almost everyone who ever cites Brown's work, cites his 1993 book chapter (17) which was also cited by Billieux et al. (16) in their paper.

In Brown's 1993 chapter, there is a table entitled 'Common Components of Addictions' (17). The table lists seven components (salience, conflict, loss of control, relief, tolerance, withdrawals, and relapse/reinstatement). In a later book chapter (18), he again features a table entitled 'Common psychological components of addictions' and lists six components (salience, conflict, apparent loss of control, relief, low self-esteem, relapse/reinstatement).

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Over the years, I adapted some of these and changed some of the definitions of these components and then put forward the assertion that a behavior should not be classed as an addiction unless there is endorsement and empirical and/or clinical verification of six specific components (i.e., salience, mood modification, tolerance, withdrawal, conflict, and relapse). These six components of addiction are different from those listed in both of the book chapters by Brown (17,18). However, Billieux et al. (16) state that:

"[A] large number of these 'new' behavioral addictions have been conceptualized on the basis of DSM [substance abuse disorder] criteria and/or the updated Brown's components model provided by Griffiths…where the euphoria component was replaced with a mood modification one."

However, this is incorrect. As shown above, Brown was never consistent in his writings as to what he believed to be the core components, and 'euphoria' was not even mentioned in the lists of either his 1993 or his 1997 book chapter. It was actually in one of his earliest (unpublished) papers that Brown (19) included the component of 'euphoria' as a component of addiction. I initially incorporated euphoria into the first iterations of my own components model in the mid-1990s (20,21). In 1996, I dropped the euphoria component and replaced it with 'mood modification' in a short communication published in the journal Nature (22).

Although many of my papers from 1995 onwards referenced the seminal work by Brown, it was my paper in the Journal of Substance Use where I specifically explicated the 'components model of addiction' (1). This paper has become one of my most cited papers and contains the six core components on which many psychometric instruments in the literature are based. As far as I am aware, not a single psychometric scale assessing the risk of different behavioral addictions has ever been based on the components listed by Brown – either in his 1988 paper (19), or his 1993 and 1997 book chapters (17,18). In terms of operationalizing the six components:

"[I] argued that all these components need to be present for a behaviour to be operationally defined as addictive. It is clear that some individuals engage in behaviours that have addictive elements without it necessarily being a full-blown addiction. For instance, if someone has no negative withdrawal effects after stopping their excessive behaviour, are they really addicted? If the excessive behaviour does not conflict with anything else in that person's life, can it be said to be an addiction? The difference between an excessive healthy enthusiasm and an addiction is that healthy enthusiasms add to life whereas addictions take away from it" (1).

Based on this latter sentence, this simple 'rule of thumb' clearly indicates that excessive enthusiasms are not addictions, and that if there are no negative consequences as a result of engaging in the behavior it cannot be defined as an addiction. In their latest paper on gaming disorder, Billieux et al. (16) made the case that there is a distinction between excessive involvement in gaming and addiction to it. I cannot think of a single researcher working in the behavioral addiction field who would not agree that there is a clear and differentiated distinction between high involvement and pathological involvement. I myself have pointed this out constantly in my research on various behavioral addictions including gaming addiction (23), work addiction (24,25), gambling addiction (26), and exercise addiction (27,28).

Are some components of addiction "peripheral"?

Perhaps the key message in the paper by Billieux et al. (16) is their assertion that some of the criteria in the components model of addiction are not necessarily negative. Particular reference was made to the "peripheral" components of "salience, tolerance, and euphoria". However, a simple re-reading of my own definitions of these components shows this not to be the case. For instance, salience, based on Brown's (17) original definition, quite clearly concentrates on the negative aspects of both cognitive and behavioral salience:

"[Salience] refers to when the particular activity becomes the most important activity in the person's life and dominates their thinking (preoccupations and cognitive distortions), feelings (cravings) and behaviour (deterioration of socialized behaviour)… even if the person is not actually engaged in the behaviour they will be thinking about the next time they will be" (1).

This operational definition clearly focuses on the negative aspects of an individual's psychological behavior (i.e., the experiencing of cognitive distortions, and total cognitive preoccupation to the neglect of everything else) and actual behavior (i.e., deterioration in an individual's socialized behavior), as well as 'classic' addiction consequences (i.e., cravings). I would argue that the cognitive and behavioral salience defined here is almost wholly negative.

Where the problem really lies is that the operationalization of salience in many psychometric instruments based on my model does not necessarily

include a negative wording and it is this aspect that Billieux et al. (16) concentrated on in their paper, not the operational definition of salience in the actual components model of addiction. Criticism was also made of Brown's 'euphoria' component. I too had issues with this component ,which is why I replaced it with 'mood modification' in my model (22) and saw such behavior as relieving a dysphoric mood state. As I noted:

"[Mood modification] refers to the subjective experience that people report as a consequence of engaging in the particular activity (i.e. they experience an arousing 'buzz' or a 'high' or paradoxically a tranquillizing and/or destressing feel of 'escape' or 'numbing')…In essence, many addicts use substances and behaviours as a way of producing a reliable and consistent shift in their mood state as a coping strategy to 'self-medicate' and make themselves feel better in the process" (1).

Again, this operational definition (while maybe not as clear-cut as the definition of salience in terms of detrimental consequences) is still more focused on the negative aspects of mood modification in which the mood-modifying experiences are used as a coping mechanism to help individuals self-medicate. Again, the problem really lies in how some psychometric instruments have operationalized this component when using my component model.

Finally, Billieux et al. (16) made observations about the problems in operationalizing tolerance (in relation to gaming rather than addiction more generally); but again, the components model of addiction defines tolerance in relation to mood modifying experiences which as explained above are rooted in more negative than positive experiences:

"[Tolerance] refers to the process whereby increasing amounts of the particular activity are required to achieve the former effects. The classic example of tolerance is a heroin addict's need to increase the size of their 'fix' to get the type of feeling (e.g. an intense 'rush') they once got from much smaller doses. In gambling, tolerance may involve the gambler gradually having to increase the size of the bet to experience a mood-modifying effect that was initially obtained by a much smaller bet. It may also involve spending longer and longer periods gambling" (1).

I agree with many of the issues concerning the problems of defining tolerance within a gaming addiction context and published a paper with many other colleagues expressing these concerns (29). The main point I would emphasize is that Billieux et al. (16) asserted that some 'peripheral' criteria in the components model of addiction are not negative. However, a neutral and unbiased reading of the operational definitions of the six core addiction components in my components model paper (1) shows them to be quite clearly negative rather than positive (although admittedly some more so than others).

The confirmatory approach to conceptualizing behavioral addiction

Another key criticism made by Billieux et al. (16) is that many researchers in the behavioral addiction field (and I would argue it is the overwhelming majority including myself) adhere to the confirmatory approach where behavioral addictions are classified on the basis of criteria of substance use disorders or behavioral addictions such as gambling disorder. The reason why the confirmatory approach is popular in the behavioral addiction field is that researchers like myself believe that addictions should be conceptualized based on similarities rather than differences (see [30] for a more detailed outline) – otherwise there is little point in calling such behaviors 'addictions.' This may actually be the primary reason why Billieux et al. (16) argue against the confirmatory approach, because it provides a conceptual mechanism of classifying almost any behavior as a non-addiction. Everybody in the behavioral addiction field is aware that every addiction has idiosyncrasies (for instance, one of the key diagnostic criteria for gambling disorder is 'chasing losses,' a criterion that is unique to gambling disorder). However, it is the similarities that will ultimately unify the study of addictive behaviors, not the differences. The anti-confirmatory approach is (arguably in my view) an 'anti-behavioral addiction' stance. If we are going to class any problematic behavior as a genuine addiction, we have to have a core set of criteria, and that is what the components model of addiction tries to provide.

Should there be exclusion criteria for behavioral addictions?

Recently, a group of scholars attempted to conceptualize behavioral addiction without pathologizing common behaviors (31). Again, instead of searching for unifying inclusion criteria, the authors proposed four exclusion criteria and argued that behaviors should not be classed as a behavioral addiction if:

- the behavior is better explained by an underlying disorder (e.g. a depressive disorder or impulse-control disorder).

- the functional impairment results from an activity that, although potentially harmful, is the consequence of a willful choice (e.g., high-level sports).
- the behavior can be characterized as a period of prolonged intensive involvement that detracts time and focus from other aspects of life, but does not lead to significant functional impairment or distress for the individual.
- the behavior is the result of a coping strategy

I argued in response to this paper that:

"[If] these criteria were applied to substance abuse, very few substance users would be classed as addicted. For instance, it is proposed that any behaviour in which functional impairment results from an activity that is a consequence of wilful choice should not be considered an addiction. I cannot think of a single addictive behaviour that when the person first started engaging in the behaviour (e.g. drinking alcohol, illicit drug-taking, gambling) was not engaged in wilfully. The key issue…is sustained harm, distress and functional impairment in the behaviour…Also, not being classed as an addiction if the behaviour is secondary to another comorbid behaviour (e.g. a depressive disorder) or is used as a coping strategy again means that some other substance addictions (e.g. alcoholism) would not be classed as genuine addictive behaviours using such exclusion criteria, because many substance-based addictions are used as coping strategies and/or are symptomatic of other underlying pathologies" (30).

In short, three of the four exclusion criteria for behavioral addiction are simply untenable – unless of course we apply the anti-confirmatory approach and argue that these criteria should only be applied to potential behavioral addictions and not substance addictions.

"Moot" behavioral addictions

Finally, I could not help but notice that almost all the examples of "moot behavioural addictions" that Billieux et al. (16) cited – as did the paper by Kardefelt-Winther et al. (31) – are papers that I co-authored. I do not have the space to give a detailed account of how each of these studies came about and the rationale for conceptualizing these behaviors within a behavioral addiction framework. However, I thought I would briefly outline some contextual information in relation to just one of the behavioral addictions that was challenged by Billieux et al. (16) – namely, 'tanning addiction'.

Over the past 15 years, there have been dozens of studies published on tanning addiction – often referred to as 'tanorexia' and 'tanning dependence' in early papers but now commonly referred to as an addiction in more contemporary papers (32-50).

Papers on this topic have been published in journals such as Addiction Biology, Addiction Research & Theory, American Journal of Drug & Alcohol Abuse, and Current Pharmaceutical Design. Tanning is far from a trivial behavior given the large number of skin cancer cases worldwide caused by the behavior (particularly in those who use sun beds excessively). There are at least ten psychometric instruments that have been developed to assess the risk of problematic tanning and/or tanning addiction. However, most of these instruments have poor psychometric properties and were developed using small sample sizes. Of all the papers that have been published on the topic, the paper I co-authored on tanning addiction (14) was singled out by Billieux et al. (16), yet our study had a very large sample size (over 23,500 participants) and outlined the development of a robust psychometric instrument to assess the risk of tanning addiction (i.e., the Bergen Tanning Addiction Scale) using core addiction criteria unlike many of the other instruments. Kardefelt-Winther et al. (31) recommended that research into behavioral addictions should begin with in-depth case studies and qualitative studies. This is exactly how the field of tanning addiction started, and our 2018 study was the latest in a relatively long line of previous qualitative, survey, and psychometric studies (14). Maybe the addiction studies field will never be unified in terms of bringing substance and behavioral addictions under the same definitional, conceptual, and diagnostic umbrella. However, that will never stop me trying.

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