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GRIFFITHS ET AL.'S COMMENTS ON THE INTERNATIONAL CONSENSUS STATEMENT OF INTERNET GAMING DISORDER: FURTHERING CONSENSUS OR HINDERING PROGRESS?

Our recent paper [1] outlined the DSM-5 criteria for internet gaming disorder (IGD), and Griffiths *et al.* [2] commented upon it. As they note [2], their collective opinions vary considerably, but they contend that our report

does not constitute an international consensus. They also critique our interpretations of the criteria for IGD in the *Diagnostic and Statistical Manual of Mental Disorders, Revision 5 (DSM-5)* [3].

With regard to their first point, Griffiths *et al.* [2] appear to start from incorrect assumptions. We did not claim there was a consensus throughout the world (is there on anything?). We also made no assertion that our group was representative of all countries or researchers. 'International', according to the *Merriam-Webster Dictionary*, is defined as 'involving two or more countries'. Clearly, our group is international. Our group also achieved a consensus, 'a general agreement about something'. Although including more experts from a greater number of countries may have been desirable by having more members who are diverse in their information sources, large groups tend to be too complex for decision-making, e.g. [4], as appears to be the case among Griffiths *et al.* [2], who have not agreed upon any aspect of the criteria. Our goal was to take the DSM-5 as a starting point and suggest ways in which researchers and clinicians around the globe could begin to assess these criteria similarly given that framework. Griffiths *et al.* [2] support our original intent by continuing the types of discussion that our paper was promoting.

Although we are encouraged that our report stimulated consideration of the DSM-5 criteria, we found Griffiths *et al.* [2] to be dismissive of what we accomplished, given other positive reactions to our consensus [5–8]. Our paper provides a guideline for future research to consider more carefully and consistently what is being measured in the context of assessing IGD. We did not debate the appropriateness of the DSM-5 criteria, the proposed threshold for diagnosis, or whether IGD is a behavioral addiction or even a mental disorder. The jury is still out on those issues. It will be indefinitely if researchers and clinicians do not begin to assess the condition in some consistent manner.

In terms of their second point critiquing our interpretation of the criteria, we believe that some authors of Griffiths *et al.* [2] may be more in agreement than disagreement with our consensus based on review of their own publications. As depicted in Table , a 2015 publication by two of their authors [9] included many items with similar content to the meanings we recommended [1]. That study [9] states explicitly that the items are 'valid, reliable, and proved to be highly suitable for measuring IGD', so it is unclear why they now [2] contest the meanings we concluded represented the DSM-5 IGD criteria.

Furthermore, Griffiths *et al.* [10] have called for an aim common to ours: 'the gaming addiction field must unite and start using the same assessment measures'. Nevertheless, Dr Griffiths continues to apply disparate tools and items, some of which appear not to overlap even with respect to meaning. For example, one recent study [11] used the items: 'I have tried to control, cut back or stop playing,

or I usually play with the video games over a longer period than I intended', and 'When I lose in a game or I have not obtained the desired results, I need to play again to achieve my target', to reflect the reduce/stop criterion (therein termed loss of control). In another recent paper co-authored by Griffiths [9], this criterion is represented by: 'Do you systematically fail when trying to control or cease your gaming activity?' and 'Have you ever in the past 12 month [sic] unsuccessfully tried to reduce the time spent on gaming?' in a third [12]. The critiques that Griffiths *et al.* [2] raise against the intended meaning that we suggested for this criterion should apply equally to the items he has been using. Some of the items above, we feel, should draw even greater concerns.

Relatedly, a recent study by his group [13] included items such as: 'I feel sad if I am not able to play games', 'When I am not gaming I feel more irritable' and 'I tend to get anxious if I can't play games for any reason'. On the surface, and as stated in that report [13], such items may be interpreted to reflect the construct of withdrawal. However, we do not feel these types of items accurately tap the intention of the withdrawal criterion for reasons we outline in our report [1], including (but not limited to) that 'for any reason' can relate to parental interference. Apparently, authors of that study [13] also now agree that these items do not capture adequately the construct of withdrawal, as all of them are also authors on the Griffiths *et al.* [2] paper. Presumably, after considering our paper, their own opinions about the very items they used to address withdrawal may have changed, although it is impossible to determine who contributed which aspects or views to a paper with 28 authors. In the case of defining a time-frame for withdrawal symptoms, we also considered during our deliberations that such a frame may be useful, but 'for up to two days' appears determined arbitrarily, as little is known about the time-course of withdrawal symptoms. Thus, in our consensus [1], we elected not to impose a time-frame.

Adding to problems in this field is a loose interpretation of what constitutes a psychometrically sound instrument. There are regular claims throughout the literature to having assessed IGD in a multitude of 'reliable and valid' ways. An example relates to one of the above-referenced papers [13] that asserts yet another new instrument is a 'psychometrically robust tool for assessing internet gaming disorder' and one that 'proved to be valid and reliable'. As with most research in this field, there is no assessment of test-re-test reliability or objective index of validity. Further, strong claims about psychometric properties are made after evaluating instruments in a single cross-sectional study with a convenience sample, in this case of online gamers. We hope researchers will consider terminology more cautiously and appropriately in the future.

Although Griffiths *et al.* [2], along with others, e.g. [7,14], have raised some potentially valid points about meanings behind the DSM-5 criteria and possibly different methods to assess them, they [2] fail to achieve any agreement. In contrast, we discussed and voted collectively upon potential meanings behind each DSM-5 criterion. We acknowledged there may be additional, or better, methods to ascertain these criteria. We also noted that our interpretative statements should not be administered literally, as they are complex, often incorporating several different meanings into a single statement. As our paper states explicitly, empirical data need to determine if the definitions we provided are adequate or inadequate. Healthy debate regarding these meanings is useful, which is what our paper was designed to begin. We have, therefore, succeeded in stimulating potentially important discussions. We believe it is useful to have suggested wording so that others can begin to think more seriously upon what they are trying to measure.

It is also useful to consider more broadly how researchers in more established fields have responded to similar concerns. None of the issues raised by Griffiths *et al.* [2] related to diagnosis are unique to gaming, and they apply to substance use and gambling disorders as well as many other mental disorders. The substance use field, for example, has long debated methods for assessing tolerance and preoccupation. We adapted wording from parallel DSM criteria from psychometrically established questionnaires for these other disorders and applied them toward gaming when conceptually, clinically and logically appropriate. In terms of their critiques of withdrawal [2], psychological withdrawal symptoms from substances (e.g. cocaine) exist, and this has been an important construct for assessing gambling disorder, even though it does not involve ingestion of a substance. Whether or not these criteria apply to IGD is important to determine empirically.

The premise throughout the Griffiths *et al.* [2] paper that people with a disorder must endorse all the criteria to render them useful goes against established methods of psychiatric diagnosis. For conditions with which gaming disorder co-occurs (e.g. mood disorders, attention deficit hyperactivity disorder, substance use and gambling disorders), one needs to endorse a specific number of the criteria for diagnosis. This practice allows diagnosis of people whose age, living conditions, social situations or current state of the condition itself do not lend to meeting a specific criterion. As with most mental disorders, the symptoms vary to some degree across individuals. Requiring an exact set of criteria to manifest for diagnosis would be harmful to patients, prevent understanding of this condition and reduce help-seeking behavior.

Similarly, differentiating 'diagnosis' or criteria based upon the type of game played or applying too much weight to idiosyncratic cases is unlikely to advance recognition of this condition. Just as alcohol use disorder and its criteria are not distinguished based upon the beverage consumed,

it is unlikely that psychiatry as a field will distinguish or allow for multiple formats of IGD or its criteria based on preferred game genres, which will change over time. Although people with alcohol use disorder who drink primarily wine differ from those who drink beer or whiskey, the criteria relate to all with the same underlying condition. The same argument is relevant to the various preferred forms of gambling and diagnosis of gambling disorder. The criteria and items used to assess them must be broad enough to encompass the many types of players and harms derived from play, while at the same time specific enough to prevent over-diagnosis or endorsement of criteria by the vast majority of players who have not experienced clinically significant harms. With these understandings in mind, many of the critiques of our consensually agreed-upon meanings become superfluous.

Interestingly, the recommendations of Griffiths *et al.* [2] are more or less identical to many steps we as a group have already undertaken [1]. We invited researchers and clinicians from around the world to study more deeply these criteria and this condition. We urged others, both experienced as well as new researchers and clinicians working in this and related areas, to use the definitions we provided as a starting point. We encouraged others to add to them as needed, while recognizing that assessment burden can diminish accuracy and utility.

In contrast to Griffiths *et al.* [2], however, we do not think it will be beneficial or appropriate to involve the gaming industry in improving classification of IGD. Similarly to the substance use and gambling fields, involvement of alcohol, tobacco or gambling industries in efforts related to diagnosis, treatment or prevention for these conditions is probably counterproductive.

We do believe there are researchers and clinicians, including many of those of the Griffiths *et al.* [2] paper, who truly are committed to better understanding, preventing and treating people who have developed problems with gaming. However, if this condition is to be recognized as a legitimate mental disorder across the globe, the field needs to converge to more clearly and uniformly assess it. Consistent interpretation of the criteria is a necessary first step for both screening tools (used in epidemiological surveys) and semi-structured interviews (used most often in clinical settings). Although multiple instruments may ultimately be developed that can assess IGD reliably and validly, balance must be achieved between tapping the constructs concisely and accurately and overburdening the patients or respondents. This balance will vary depending on the context, but the ultimate goal is to classify people with a similar condition. We also caution that over-emphasizing inclusion of treatment-seeking samples in development of these tools will lead to biased results, as typically only people with the most severe forms of mental disorders seek professional care, and even then only when treatment is available.

Research by many of the authors of the Griffiths *et al.* [2] report and our report [1], and many others, spurred these discussions and led to the inclusion of Internet Gaming Disorder in Section 3 of the DSM-5. The DSM-5 criteria [3], our initial international consensus [1] and the discussions that are beginning represent a critical next step in the advancement of this field.

Declaration of interests

Preparation of this report was supported in part by NIH grants P50-DA09241, P60-AA03510, R01-AA021446, and R01-AA023502. No authors report any financial conflicts of interest associated with the paper. Drs. Petry, Auriacombe, Borges, and O'Brien served on the American Psychiatric Association Workgroup for Substance Use and Related Disorders for the DSM-5. The views and opinions expressed in this paper are those of the authors and should not be construed to represent the views of any sponsoring organizations, agencies, or governments.

Keywords Behavioral addiction, diagnosis, DSM-5, gaming, internet gaming disorder, video games.

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