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DSM-5 diagnosis of Internet Gaming Disorder: Some ways forward in overcoming issues and concerns in the gaming studies field

Response to the commentaries

DARIA J. KUSS*, MARK D. GRIFFITHS and HALLEY M. PONTES

International Gaming Research Unit, Psychology Department, Nottingham Trent University, Nottingham, UK

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Background and aims: The current DSM-5 diagnosis of Internet Gaming Disorder (IGD; American Psychiatric Association [APA], 2013) has led to a number of issues and concerns that we highlighted in our recent paper (Kuss, Griffiths, & Pontes, 2017). Experts in the field responded to our evaluation of these issues resulting in six commentaries. *Methods:* In this paper, we offer responses to the six commentaries to move the scientific field forward. All of the responses to our original paper highlighted many conceptual, theoretical, and/or methodological problems with the proposed IGD diagnosis as outlined in the DSM-5. We outline some ways forward in overcoming issues and concerns in the gaming studies field. *Results:* We argue that rather than stigmatizing gaming per se, the role of scientists and practitioners is to establish a clear-cut distinction between someone who may use games excessively but non-problematically and someone who is experiencing significant impairment in their daily lives as a consequence of their excessive gaming. This responsibility needs to be shared by popular media who are often quick to build a moral panic around gaming behaviors, often based on cherry-picking specific case studies and pieces of research which support their headlines. *Conclusion:* Researchers, practitioners, gaming developers, and the media need to work together and collaboratively to build a realistic and comprehensive understanding of gaming as a normal, enjoyable, and often beneficial sociocultural practice, which for a small minority of excessive users may be associated with the experience of addiction-related symptoms that may require professional support.

Keywords: Internet Gaming Disorder, gaming addiction, diagnosis, DSM

The current DSM-5 diagnosis of Internet Gaming Disorder (IGD; American Psychiatric Association [APA], 2013) has led to a number of issues and concerns that we highlighted in our recent paper (Kuss, Griffiths, & Pontes, 2017). Experts in the field have responded to our evaluation of these issues, and all of the responses to our original paper highlighted many conceptual, theoretical, and/or methodological problems with the proposed IGD diagnosis as outlined in the DSM-5. In what follows, we will respond to the commentaries, and hope the scientific dialogue concerning the outlined issues will help move the scientific field forward and to ultimately support those individuals who may require professional help to overcome problems associated with their excessive gaming use that may cause significant stress and impairment in their daily lives.

Most of the commentaries – particularly those by Starcevic (2017) and Van Rooij and Kardefelt-Winther (2017) – repeated the same arguments that they have outlined in previous papers. Starcevic (2017, p. 2) argues that basing IGD within an addiction framework is "*constraining because it interferes with the development and testing of the alternative conceptual frameworks for problematic gaming, such as those based on the idea that this behavior may be a consequence of maladaptive coping or a way of meeting particular needs*" (Kardefelt-Winther, 2014). However, as Griffiths (2017) has noted, many – if not most – addictions (whether substance-based or behavioral) are a manifestation of maladaptive coping and therefore this is not a case of "either/or" in this particular instance. Recent empirical research by Kuss, Dunn, et al. (2017) furthermore suggests dysfunctional coping significantly predicts excessive Internet and gaming use, providing support for a self-medication hypothesis of addictive disorders, including gaming. The self-medication hypothesis has also been established for substance use (see Khantzian, 1985, 1997) and this does not take the legitimacy or nosological importance of substance use disorders. For this reason, IGD as a maladaptive coping behavior fits perfectly well within an addiction framework and does not invalidate its status as a mental health disorder affecting a minority of individuals.

Starcevic (2017) also appears to claim that those working in the IGD field and who conceptualize IGD as an addiction assume that persistent gaming behavior is engaged in as a

* Corresponding author: Daria J. Kuss; International Gaming Research Unit, Psychology Department, Nottingham Trent University, 50 Shakespeare Street, Nottingham NG1 4FQ, UK; Phone: +44 115 848 4153; E-mail: daria.kuss@ntu.ac.uk

way of avoiding withdrawal symptoms. This is not our view and we simply believe that those genuinely addicted to gaming experience withdrawal symptoms if unable to engage in gaming but do not necessarily believe that addicted gamers play games to avoid withdrawal symptoms (although that does not rule out the possibility that some addicted gamers do this).

We agree with Starcevic that "*addictive disorders are generally chronic and progressive, if not treated*" (p. 2) and that the onset of excessive gaming can be episodic and transient. However, in cases such as this, the behavior should not be described as an addiction. Our own previous papers have specifically noted that some gamers can play very excessively without experiencing any major problems and that while all genuine gaming addictions are problematic, not all problematic gamers have an addiction (Griffiths, 2010b).

Starcevic (2017) also argues that if the gaming addiction is a consequence of other psychopathologies then it should not be viewed as a genuine addiction. This argument was recently put forward by Kardefelt-Winther et al. (2017) but in response to this, Griffiths (2017) noted that other genuine addictions (e.g., alcoholism and gambling disorder) are not discounted as addictions if there are other underlying comorbidities. Addictions are defined by the characteristics of the behavior itself and the consequences, not the underlying causes. In addition to this, clinical evidence suggests that if one mental disorder is present, the presence of other disorders is the norm, not the exception, and this holds both in the context of Internet and gaming addiction psychopathology (Kuss & Griffiths, 2015) as well as for other mental disorders (Starfield, 2006).

The issue of whether "tolerance" and "withdrawal" are core criteria of IGD (and addictions more generally) was also raised by Starcevic (2017). Just because some more recent operational definitions of substance-based addictions do not include tolerance and withdrawal does not mean they are not useful indicators of addictive behavior. For us, the major issue is how concepts such as "tolerance" and "withdrawal" are defined as recent research has shown that these criteria in the context of IGD are more nuanced (King, 2017; King & Delfabbro, 2016). For instance, almost two decades ago, Griffiths noted in his case studies that one type of tolerance that was unique to online addictions was the continual upgrading of computer hardware and software (Griffiths, 2000). Therefore, we would agree with Starcevic (2017) that the current conceptualization of tolerance in the DSM-5 is inadequate (because tolerance only relates to increasing amounts of time spent gaming rather than other actions that might equally be indicative of tolerance), and that it should be revised.

However, we agree with Starcevic that the DSM-5 criteria create "*high levels of heterogeneity*" (p. 2) given that only five of the nine DSM criteria need to be endorsed to diagnose IGD. More research and clinical insight into what the "core" (as opposed to peripheral) criteria of IGD are would be particularly helpful to all those working in the IGD field. Starcevic (2017) also asserts that those in the field should move away from a "checklist" approach to addiction diagnosis. However, it could be argued that any diagnosis of mental health disorder is ultimately checklist-based and that such an assertion is impractical. We believe it is more helpful to conceptualize addictions as syndrome-based (Shaffer et al., 2004) and fully acknowledge that what unites addictions is their similarities rather than their dissimilarities (Griffiths, 2017).

Van Rooij and Kardefelt-Winther (2017) rehearse many of the arguments they have performed before in their previous writings. They assert that the IGD field "*lacks basic theory, definitions, and properly validated and standardized assessment tools*" (p. 1). We would actually argue the opposite that the field has too much theory, too many definitions, and over 20 psychometrically validated instruments (King, Haagsma, Delfabbro, Gradisar, & Griffiths, 2013; Pontes, 2016). To exemplify this, Kardefelt-Winther focuses on writing commentaries and critiques of researchers collecting data on IGD rather than collecting his own data on the topic [e.g., his 12 most recent papers and communications about IGD and behavioral addiction on *Research Gate* (2014–2017) do not contain new primary data collected on IGD, but are commentaries of others' research: see https://www.researchgate.net/profile/Daniel_Kardefelt-Winther/publications].

Van Rooij and Kardefelt-Winther (2017) assert IGD as a "*new clinical disorder*" (p. 1). However, it is only new in terms of its suggested terminology and inclusion in the DSM. Given that IGD includes offline gaming disorders, detailed clinical case studies of the disorder and its treatment (typically using cognitive-behavioral therapy) have been in the psychological literature for three decades (e.g., Keepers, 1990; Kuczmierczyk, Walley, & Calhoun, 1987). While Van Rooij and Kardefelt-Winther (2017) are correct in stating that most validated assessment tools have not included patients with IGD, it does not mean that they do not include items that were based on previous cases and samples. For instance, instruments that we have ourselves developed (e.g., Demetrovics et al., 2012; Pontes, Király, Demetrovics, & Griffiths, 2014) have partly relied on data collected among those seeking treatment for their addiction to gaming (e.g., Beranuy, Carbonell, & Griffiths, 2013; Griffiths, 2010b).

Van Rooij and Kardefelt-Winther (2017) cite their own recent paper to assert "*we do not even have a clear idea of how to properly define overuse or problematic use of technology*" (Kardefelt-Winther et al., 2017, p. 2). We would argue that most authors do have a very clear idea of how they would define IGD. *What we do not have is any consensus* as argued in a paper that we were all co-authors on (i.e., Griffiths, Kuss, Lopez-Fernandez, & Pontes, in press). We certainly agree that self-reported survey scores are not sufficient to establish the presence of IGD, but all epidemiological papers are published on the proviso that the prevalence of IGD in such studies is only *indicative*, and that in-depth clinical interviews are the only way of establishing with any true validity that IGD is present in any specific individual. Our own view is that the IGD field is no different to the study of any other addictive behavior (e.g., alcoholism, cocaine addiction, and gambling disorder) and that the majority of the papers published are self-selected self-report surveys using convenience samples. However, there are an increasing number of papers on IGD using other methodologies (e.g., neuroimaging studies), which also suggest that IGD is akin to other more traditional addictions in terms of neurology and psychobiology (see Kuss & Griffiths, 2012a; Pontes, Kuss, & Griffiths, 2017).

Van Rooij and Kardefelt-Winther's commentary (2017) demonstrates that most studies carry out research on "*largely healthy populations*" (p. 3). We do not dispute this, but this is no different from the much larger and established literature on gambling disorder. What these epidemiological studies show (whether in IGD or other addictive behaviors) is that the vast majority of the population have no problems whatsoever, but that a small minority appear to have such problems. No survey can ever definitively show that any disorder definitely exists. Such surveys are only ever indicative of how widespread a disorder might be. The fundamental issue in this area is whether such a disorder exists or does not exist. As we argued in response to whether the World Health Organization should include gaming disorder in the latest edition of the International Classification of Diseases (Aarseth et al., 2016), as far as we are aware, there is no minimum number of cases needed to be identified for a disorder to be classed as such (Griffiths et al., in press). We take the view that there is ample empirical evidence that has been published from a clinical perspective suggesting IGD exists (e.g., Park, Lee, Sohn, & Han, 2016; Sakuma et al., 2017; Yao et al., 2017; Young, 2013). What is clear is that case-by-case details differ concerning the periphery (and that it is a syndrome, as argued above), but that the core consequence is the same across cases (i.e., that gaming has a significant negative psychosocial impact on core areas of the individuals' lives). Van Rooij and Kardefelt-Winther (2017) also assert the same argument used by Aarseth et al. (2016):

> "*Moreover, there are genuine risks involved in creating a new disorder. We believe that Kuss et al. (2016) do not fully consider the impact that recognizing a formal disorder would have on gamers everywhere. Gaming is different from substance abuse behaviors in that it is one of the most popular hobbies for children and adolescents worldwide, with many healthy and positive outcomes resulting from it… Therefore, whether we formalize extensive gaming as a disorder or a normal pastime activity is likely to impact the general population of gamers and the attitudes of their parents.*" (p. 3)

We have most definitely considered the wider impact and we have never confused "extensive gaming" with problematic and/or addictive gaming (that Van Rooij and Kardefelt-Winther appear to do in the paragraph quoted above). We have published many papers on the positives of gaming including both the educational and therapeutic values (e.g., De Freitas & Griffiths, 2007, 2008; Griffiths, 2002, 2005b, 2005c, 2010b; Griffiths, Kuss, & Ortiz de Gortari, 2013, 2017 – full list of papers available on request) as well as the importance of the context and culture of gaming for the individual gamer (Griffiths, 2010b; Kuss, 2013a, 2013b). Our research clearly makes a distinction between excessive/extensive gaming, problematic gaming, and addictive gaming. These all lie on a continuum of no pathological gaming to pathological gaming. Very few individuals by our own addiction criteria are pathological gamers.

In taking the field forward, one of the key actions suggested is based on another paper Kardefelt-Winther et al. (2017) recently published which claims it advances a definition of behavioral addiction. Kardefelt-Winther et al. (2017) provided four exclusion criteria and argued that behaviors should not be classed as a behavioral addiction if:

1. "*The behaviour is better explained by an underlying disorder (e.g., a depressive disorder or impulse-control disorder).*
2. *The functional impairment results from an activity that, although potentially harmful, is the consequence of a willful choice (e.g., high-level sports).*
3. *The behaviour 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.*
4. *The behaviour is the result of a coping strategy.*" (p. 2)

Griffiths (2017) criticized three of the four criteria by arguing that other behaviors classed as addictions (a) often have other comorbid pathologies, (b) engage in behaviors willfully (e.g., drug-taking and gambling), and (c) often use the behavior as a way of coping. If the exclusion criteria for non-substance use behaviors were applied to substance users, few individuals would be diagnosed as addicts. In short, the suggested criteria for behavioral addiction are not tenable.

Müller (2017) contends that research in the area of IGD has progressed substantially, particularly with regard to its quality, emphasizing its methodological soundness (relating to epidemiological and clinical data), and the use of different methods, which suggests that (a) IGD exists and (b) it "*causes severe negative consequences for those losing control of their gaming behavior and for their social environment*" (p. 1). We agree with this understanding of the problem. Systematic reviews of research (e.g., Kuss, Griffiths, Karila, & Billieux, 2014) have previously shown that there are a number of epidemiological studies assessing Internet and gaming addiction, including representative studies, and which map out the research field better than ever before. In addition to this, it has been found individuals seeking help for their IGD-related problems experience high levels of distress and negative consequences in their academic, professional, and personal lives, leading them to get in contact with specialized treatment providers (Kuss & Griffiths, 2015). Moreover, objective neuroimaging research has been used in the area of Internet and gaming addiction (Kuss & Griffiths, 2012a; Pontes et al., 2017), substantiating that IGD is similar to substance-related addictions on the molecular, neurobiological, and cognitive-behavioral levels.

Nonetheless, the methodological problems and the relative scarcity of contemporary research including prospective designs assessing etiopathology, and clinical research is worth pointing out, as stressed by Müller (2017). Recently, King et al. (2017) evaluated evidence-based treatments of IGD from an international perspective employing the widely used CONSORT criteria, highlighting problems with the research to date, namely (a) an inconsistent approach to definition, diagnosis, and measurement, problems regarding (b) randomization, (c) controls, and (d) sample descriptions, further stressing the need to develop valid and reliable research approaches to further a comprehensive understanding of IGD and how those who require professional support can be helped.

Müller (2017) also stresses the usefulness of diagnostic criteria for IGD in the research context as establishing valid and reliable criteria may solve some methodological problems and allow for comparisons across studies. We agree with this evaluation as we have previously outlined that the existence of an abundance of diagnostic tools for the potential disorder significantly hampers scientific progress in the area (Kuss et al., 2014), which was the case prior to the publication of the APA's preliminary IGD criteria in 2013, negatively impacting prevalence rate estimates. Only if the research community adopts the same criteria and cutoff points can the problem of IGD be understood comprehensively from a global mental health perspective, without relying on a multitude of heterogeneous tools being used in often incomparable ways. In addition to this, we agree with the postulation that diagnostic criteria require rigorous testing across different and diverse groups of individuals to increase diagnostic accuracy, paving the way for additionally required empirical research.

In addition to this, Müller (2017) emphasizes that the APA focuses on IGD and disregards other online activities which appear to have a high addictive potential, such as online gambling, online social networking, online pornography, and generalized Internet addiction. We also agree with this contention. Previous research has showed that online gambling may be a distinct problem for a small minority of gamblers (Kuss & Griffiths, 2012b) and should be viewed separately to IGD, as should other problematic online behaviors such as online sex addiction (Griffiths, 2012), online shopping addiction (Andreassen et al., 2015), and social networking addiction (Griffiths, Kuss, & Demetrovics, 2014). For instance, we recently developed a number of arguments highlighting how the excessive use of online social networking sites (SNSs) may lead to symptoms traditionally associated with substance-related addictions (Kuss & Griffiths, 2017). This understanding becomes particularly relevant when considering what an integral element of today's everyday culture and way of being SNS have become, with individuals feeling pressured to get involved due to a fear of missing out and the constant availability of connection via mobile technology, leading to unprecedented compulsive behaviors and sociocultural pressures, which for the small minority of excessive SNS users may result in seeking professional help (Kuss & Griffiths, 2015). Given SNSs contain gaming elements, and excessive gaming has been associated with possible mental health problems, we agree with Müller's (2017) assertion that consequences, phenomenological similarities, and differences of IGD and related problematic behaviors need to be addressed by research.

Furthermore, Müller (2017) calls for research involving different fields, such as media psychology to understand IGD better. Research has previously highlighted the relevance of the sociocultural context to the gaming experience (e.g., Kuss, 2013a, 2013b), stressing that interdisciplinary research including media, communication, human–computer interaction, and gaming studies is the way forward. In addition to this, anthropological (Snodgrass, Dengah, Lacy, & Fagan, 2013) and ethnographical perspectives (Karlsen, 2013; Kuss, 2013a) are also useful as they may shed light on gaming motivations, gaming structure and mechanics, perceived rewarding effects and the meaning of gaming for the individual and the gaming community, and how these may impact differentially on problematic gaming.

Quandt (2017) makes two specific points which we feel we should respond to. The first point denotes problems on the definitional level, with Quandt (2017) arguing that currently, research does not provide many insights as to *what* people may become addicted to, providing examples regarding platforms, channels, and game genres, each of which may have a different user base and associated gaming motivations, different game mechanics, including reward systems, narrative and graphics, and social aspects. Quandt (2017) points to the interplay between the narrative, mechanics, and the context as important elements needing consideration in the context of an IGD diagnosis. This is in line with the points raised above regarding the requirement of an interdisciplinary study of gaming and gaming addiction involving diverse disciplines, such as media, communication and gaming studies, anthropology, and ethnography (Karlsen, 2013; Kuss, 2013a; Snodgrass, et al., 2013). To understand a potential disorder comprehensively, the sociocultural context of gaming as a practice requires elucidation, and the meaning of the gaming for the individual and the gaming community deserves attention. This is particularly relevant when the aim is to disentangle causes and effects in IGD, as rightly noted by Quandt (2017), given gaming may fulfill a wide variety of functions in the individual's life, including, but not limited to, serving as a coping mechanism to escape real-life problems (Kuss, 2013a; Kuss, Dunn, et al., 2017).

The second point relates to the notion of "*defining a social behavior as a disease*" (Quandt, 2017, p. 2), a conception which has been picked up by other researchers in the context of possibly overpathologizing everyday life behaviors (Billieux, Schimmenti, Khazaal, Maurage, & Heeren, 2015). Quandt (2017) argues prematurely "*defining 'something' as an addiction may affect many people's lives by stigmatizing them and exposing them to potentially wrong treatment*" (p. 1), which may lead to opening "*the door for behavioral control along the lines of norms decided in academic (or other) circles*" (p. 2). One may claim there is a fine line between "behavioral control" and supporting the public making informed decisions about their and their children's behaviors. For instance, age restrictions for films and games are in place in many countries. The Pan European Game Information (PEGI) is a game ratings board covering most of Europe (PEGI, 2017), whereas the Entertainment Software Rating Board (ESRB) covers North America (ESRB, 2017). Both share similar guidelines, including content discriptors, which denote the suitability of playing particular games for different age groups. Rather than controlling possibly unwanted behaviors, such agencies support families making informed decisions by providing relevant information. Similarly, the understanding of excessive gaming being associated with addictive symptoms and causing detrimental health outcomes for a small minority of excessive users may then lead to the development of appropriate and effective approaches of treating the resulting problems, rather than functioning as a method of behavioral control. In addition to this, rather than stigmatizing individuals, a possible diagnosis may destigmatize

individuals as the source of the resulting problems can be viewed from the perspective of neurobiology, which is in line with the commonly adopted disease framework, taking away the blame from the individual (Kuss, 2013b). This may increase confidence, the willingness to change, and positively impact on treatment completion (Kuss & Griffiths, 2015).

Carbonell (2017) discusses the construct of IGD and its feasibility in light of functional impairments and the stability of the disorder. Other aspects related to the gaming experience were also considered in terms of their diagnostic implications [i.e., avatar identification, motives, video game genre, and game mode (online/offline)]. He pointed to potential issues related to the development and conceptualization of IGD that have been extensively debated in the literature (e.g., Griffiths et al., 2016; Pontes et al., 2017). It is worth noting that the development of the nine IGD criteria was a laborious and systematic process that involved regular meetings and expert discussions over a period of 5 years with 12 members and 20 advisers of the substance use disorder working group commissioned by the APA (Petry & O'Brien, 2013). To develop the IGD criteria, the APA analyzed well over 250 empirical reports on video game addiction (Petry & O'Brien, 2013; Petry et al., 2014). Although it is true that the nine IGD criteria "*were derived in large part from the report of Tao et al. (2010) that used an iterative process to identify diagnostic criteria*" (Petry et al., 2014, p. 2), the nine IGD criteria were developed and worded to parallel some substance use and gambling disorder criteria, while acknowledging that the clinical expression of IGD may differ from these disorders (Petry et al., 2014).

Carbonell (2017) argues that the IGD criteria are "*more appropriate for a developmental disorder than a diagnosis for adults*" (p. 1) and that "*IGD diagnosis is for adults and not for teenagers*" (p. 2). We disagree with this point given that a relatively large number of empirical and clinical studies have investigated the feasibility of the criteria in samples of different age groups (e.g., Ko et al., 2014; Pontes et al., 2014). Notwithstanding the clear need for refining the diagnostic criteria, most studies support the idea that IGD is a clinical and sociological phenomenon affecting a minority of individuals across different age groups. Furthermore, it is also important to acknowledge these findings to avoid generating further stigma and inaccurate stereotypes about video game addiction.

Carbonell (2017) also highlights that "*the criteria for behavioural addictions are ambiguous in general*" (p. 1). While many scholars (e.g., Sinclair, Lochner, & Stein, 2016) (including ourselves) agree with this assertion, we believe it only suggests that further research endeavors should be carried out to help clarify these conceptual conundrums. For this reason, it would be premature to disregard IGD as a relevant clinical construct on the basis of scholars' disagreement on how best to conceptualize it. To this end, a study by Pontes et al. (2014) was able to empirically test the nine IGD criteria against a well-established conceptual framework for behavioral addictions and the results of this study demonstrated that the IGD criteria can be empirically framed within the components model of addiction (Griffiths, 2005a), similar to many other behavioral addictions.

Another issue raised by Carbonell (2017) related to functional impairment and stability of IGD. Carbonell (2017) compared the functional impairments of IGD with those from substance use disorders and concluded that issues arise due to the fact that IGD does not cause impairments in a similar fashion. As aforementioned, the development of the IGD criteria acknowledges that its clinical expression may differ from other addictions (Petry et al., 2014). For this reason, it would be unreasonable to expect that IGD would cause functional impairments with similar intensity and detrimental impact as those caused by substance use disorders even though they share important neurobiological similarities with IGD. With regard to the stability of IGD, little longitudinal and clinical research has been conducted so far to allow any definite conclusion regarding this. For this reason, it is paramount that future research investigates the stability and clinical course of IGD as up to 50% of individuals with IGD may recover naturally and efficacious treatment protocols must be able to exceed unassisted recovery rates (Petry, Rehbein, Ko, & O'Brien, 2015).

Furthermore, Carbonell (2017) suggested that in-game experiences and processes such as avatar identification, high levels of immersion, structural characteristics of video games, and motivations could be relevant to understand problematic use. Although we agree with this idea, it is worth noting that these in-game experiences are not central to the diagnostic framework of IGD as they relate to non-pathological secondary processes inherent to the gaming experience. Carbonell (2017) alluded to the difficulties in distinguishing high engagement from addiction (especially in Asian cultures) and that when professional gamers start playing a game, they often required a period of time of training and exercise to master the game. Regardless of the cultural context, we would argue that salient behaviors in which individuals are highly engaged do not constitute behavioral addictions per se as in the case of professional gamers, playing video games for extended periods of time is not necessarily detrimental as gamers enjoy what they do and get paid for doing it, similar to academics working with computers and using the Internet for many hours and not being addicted to the Internet. It is important to acknowledge that while hobbies and professional/academic commitments generally add to life (even when engaging excessively), addictions take away from it given their clinical and sociological impairments that interfere with daily activities and overall functioning (Griffiths, 2010b).

Finally, Carbonell (2017) criticized the idea of gaming addiction given the confusion generated by the DSM-5 with regard to addiction in online and/or offline games and the terminology chosen (i.e., IGD) for the phenomenon. Carbonell (2017) mentioned that "online" and/or "offline" should be the key differentiating point between "gaming disorder" and "playing disorder." We would argue that at the theoretical level, any behavior engaged in excessively and causing significant clinical impairment could be classed as an addiction with this being the key differentiating point between healthy versus addictive play. However, in the case of gaming addiction, studies (e.g., Bakken, Wenzel, Götestam, Johansson, & Øren, 2009; Lemmens & Hendriks, 2016) have shown that although online gaming appears to

be more addictive than offline gaming, gaming addiction can occur regardless of how games are played (e.g., online or offline) or their structural characteristics (Griffiths, Kuss, & King, 2012).

The paper by Krossbakken, Pallesen, Molde, Mentzoni, and Finseråas (2017) discusses important conceptual and methodological aspects of IGD research at the broader (i.e., construct) and specific (i.e., criterion) levels. Methodological implications were also considered, and there are a few areas in which we disagree with the points being made despite some of the excellent ideas being put forth. Krossbakken et al. (2017) appear to concur with our view that the term "Internet" in the terminology of gaming addiction (i.e., IGD) is not accurate given that gaming addiction can occur both online and offline as we previously argued and discussed (e.g., Pontes & Griffiths, 2014). Krossbakken et al. (2017) also discussed the role of risk factors for IGD and noted that in their opinion cross-sectional studies investigating risk factors for IGD "*do not possess the necessary methodological rigour for drawing conclusions*" (p. 1). We disagree with this view as there are a number of advantages in cross-sectional studies even though they do not allow for testing causal hypotheses. Nevertheless, given the early stages of research on IGD, cross-sectional studies present with many advantages as they are the least expensive in terms of time and resources, and can be valuable in generating meaningful hypotheses about causes of a disease, providing foundations for future epidemiological research studies seeking to ascertain specific causal relationships with regard to a disease (Page, Cole, & Timmreck, 1995).

Krossbakken et al. (2017) further suggest that given the extant limitations in cross-sectional research, inferring causality with regard to risk factors for IGD "*a developmental psychopathological research frame seems warranted*" (p. 2). They further add that "*there is a need to consider both the time frame and context when evaluating short- and long-term consequences of a gaming disorder*" (p. 2). We agree with Krossbakken et al. (2017) with regard to the need of further longitudinal research in the field. However, we would like to highlight that recent developments in the psychometric assessment of IGD have taken into account the recommended 12-month time frame suggested by the APA in the evaluation of IGD (see Pontes, 2016, for a review on IGD assessment). For instance, both the Internet Gaming Disorder Test (IGD-20 Test) (Pontes et al., 2014) and the Internet Gaming Disorder Scale – Short-Form (IGDS9-SF; Pontes & Griffiths, 2015) evaluate IGD within the APA's suggested time frame of 12 months.

Krossbakken et al. (2017) further noted that the "*increasing convergence of gaming and gambling deserved further attention*" (p. 2). Although gambling and gaming may share some common structural features (e.g., money betting), it can be argued that these two activities are not the same as their key defining features differ in a number of ways. For instance, "chasing losses" has been long established as a robust criterion in the development of problem gambling, and research has shown this criterion accounts for a significant amount of variance in problem gambling (Fisher, 2000). Conversely, "chasing losses" is not a relevant/applicable criterion for understanding IGD as the main psychological motivations to play video games vary in a distinctive way, with escapism and time spent gaming being often associated with IGD (e.g., Hagström & Kaldo, 2014; Pontes & Griffiths, 2016).

We wholeheartedly agree with the view of Krossbakken et al. (2017) and other researchers that "*excessive gaming without adverse consequences should not be classified as a mental disorder*" (p. 2). We believe this is something the field has already acknowledged in the literature. For instance, the APA noted that IGD encompasses persistent and recurrent use of video games that leads to clinically significant impairment or distress (APA, 2013). At the specific (i.e., criterion) level, Krossbakken et al. (2017) noted that inconsistencies with many of the criteria defining IGD have been identified by research, especially with regard to withdrawal symptoms and tolerance. Indeed, several studies examining the construct of IGD at the specific level produced mixed findings. However, this only suggests that further research has to be conducted, especially among clinically diagnosed cases where the IGD criteria can be compared against a robust gold standard. Furthermore, we would argue that most of the inconsistencies found in research with regard to the IGD criteria partly result from the fact that the vast majority of these studies drew their findings from non-clinical/normative community samples where the endorsement and severity of these criteria is naturally low given that behavioral addictions are a relatively rare phenomenon affecting a very small proportion of individuals.

Finally, Krossbakken et al. (2017) suggested that IGD should be assessed as a formative construct in measurement models as it "*offers an opportunity to develop the research*" (p. 3). While we agree that new methodological advancements to assessing IGD should be in place, we disagree with the idea that IGD should be assessed as a formative construct in measurement models for a number of reasons including statistical and theoretical caveats. At the statistical level, Kline (2013) explained that formative models assume that cause indicators have perfect score reliabilities (i.e., $r_{XX} = 1.00$), which is unrealistic for most observed variables, leading to increased disturbance variance of the corresponding latent composite. Furthermore, unlike a reflective measurement model, a formative measurement model does not explain the variances and covariances of the indicators (Kline, 2013). At the theoretical level, the shortcomings of the formative model of IGD can be also explained by the criterion of "relapse" (IGD criterion 4). If we assume IGD is a formative construct, then it implies that "relapse" causes IGD. This assumption is theoretically problematic as "relapse" occurs due to the development of an addiction and not vice versa. In short, individuals will not "relapse" if they do not present with an addiction. Other psychometricians (i.e., Howell, Breivik, & Wilcox, 2007) concluded that "*formative measurement is not an equally attractive alternative to reflective measurement and that whenever possible, in developing new measures or choosing among alternative existing measures, researchers should opt for reflective measurement*" (p. 205). In the same vein, we would argue that conceptualizing IGD within a feedback loop measurement model would be more feasible at the

statistical and theoretical level in comparison with both reflective and formative models (see Kline, 2013).

Taken together, we hope that the scientific dialogue that has emerged as a consequence of our collaborative work in this area will continue to carry the field forward. Rather than stigmatizing gaming per se, the role of scientists and practitioners is to establish a clear-cut distinction between someone who may use games excessively but non-problematically and someone who is experiencing significant impairment in their daily lives as a consequence of their excessive gaming. This responsibility needs to be shared by popular media who are often quick to build a moral panic around gaming behaviors, often based on cherry-picking specific case studies and pieces of research which support their headlines. In sum, researchers, practitioners, gaming developers, and the media need to work together and collaboratively to build a realistic and comprehensive understanding of gaming as a normal, enjoyable, and often beneficial sociocultural practice, which for a small minority of excessive users may be associated with the experience of addiction-related symptoms that may require professional support.

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