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Both sides of the story: Addiction is not a pastime activity

Commentary on: Scholars' open debate paper on the World Health Organization ICD-11 Gaming Disorder proposal (Aarseth et al.)

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The proposed inclusion of Internet gaming disorder (IGD) into the upcoming ICD-11 has caused mixed reactions. Having a sound diagnostic framework for defining this new phenomenon has been applauded but concerns have risen regarding overpathologizing a mere pastime activity. The review by Aarseth et al. (2016) provides a fine but one-sided impression on IGD. What has been totally left out in the argumentation is the clinical perspective. Although the concerns depicted must not be ignored, the conclusion provided by the authors is reflecting quite subjective speculations while objectivity would rather be needful.

Keywords: Diagnostics, Internet gaming disorder, phenomenology

SEX, DRUGS, AND JUMP 'N' RUN

Certain behaviors that are usually meant to be a simple or even enjoyable part of our lives can make life become difficult. Looking back in history reveals that more (e.g., sex, sports, and gambling) or less (e.g., work) enjoyable activities under certain circumstances can get out of control, enacting a negative impact on an individual's life. While – in contrast to former times – nowadays, no doubt is left that the consumption of psychoactive substances can lead to physiological and psychological symptoms of addiction, the concept of behavioral addictions is still a matter of debate.

When the DSM-5 was released (American Psychiatric Association [APA], 2013), it was decided to stick to a broader concept of addiction. As the first non-substance-related addiction disorder, gambling disorder entered the chapter of "Substance-Related and Addictive Disorders" and Internet gaming disorder (IGD) was included as a preliminary diagnosis in Section 3. Especially, the inclusion of IGD has caused heated discussions among experts from different fields – a discussion similar to the one following the release of the DSM-III and ICD-10 in 1980, when pathological gambling was first defined as a new mental disease (e.g., McGarry, 1983; National Research Council, 1999; for details of the historical development, see Wilson, 1993).

The contribution of the group around Aarseth et al. (2016) is a good example for the 2017-version of the discussion from the 80s. It is also a good example for the dilemma researchers, clinicians, parents, enthusiastic gamers, and even patients suffering from the symptoms of IGD are experiencing these days. Not for the first time ever, it raises the question where to draw the line to appropriately distinguish between normal behaviors that are part of a modern lifestyle and harmful usage patterns that can lead to psychopathological symptoms and suffering.

On one hand, Aarseth et al. (2016) invoke some good arguments and justified concerns on the nature and diagnostic complexity of IGD. On the other hand, some of the aspects depicted have to be critically seen and suffer from considerably flawed interpretations of the issue. The most important weakness regards a rigorous oblivion of the situation of people suffering from IGD. In that context, the contribution of Aarseth et al. (2016) takes an academic perspective that is far away from clinical reality. Thus, it reminds of the metaphoric ivory tower science can be trapped in.

RESEARCH QUALITY IS IN THE EYE OF THE BEHOLDER

Roughly estimated, serious research on IGD and Internet addiction in general has begun just about 10 years ago. Thus, Aarseth et al. (2016) are right when they refer to several missing links in our understanding of IGD. Indeed,

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different experts have called for a more systematic and a more specific research on that matter (e.g., Griffiths et al., 2016). While we have plenty of data from epidemiological surveys based on questionnaires, clinical research is still underrepresented. While we have numerous data from cross-sectional studies, prospective investigations are either missing or suffering from methodological problems. Thus, the need for enhancing our knowledge becomes clear. Yet, Aarseth et al. (2016) have a quite unique position here. Despite saying that the quality of research on IGD requires to be further enhanced, they argue that the inclusion of a formal diagnosis would lead to a "waste of resources in research, health, and the public domain." Following this recommendation would lead to a stagnation of our knowledge on IGD. Apart from the term "wasting resources," which is totally misplaced when talking about research meant to enhance health, it is hard to find the point in that argument.

Furthermore, the authors are referring to the mismatch between prevalence studies and patients entering the health care system ["reported patient numbers do not always correspond to clinical reality, where patients can be hard to find (Van Rooij, Schoenmakers, & van de Mheen, 2017)"]. Again, one has to ask, is this mismatch a specific feature of IGD? Again one has to say, no, it is not! Looking at prevalence studies on addictive behaviors, such as alcohol dependence or gambling disorder, teaches that prevalence rates found within the community exceed by far the number of patients seeking treatment (Bischof et al., 2012; Slutske, 2016). The reasons for that gap are quite different and encompass both specific motivational correlates of the disorders and structural features of the health care system (see Rockloff & Schofield, 2004; Suurvali, Cordingley, Hodgins, & Cunningham, 2009). Does this circumstance mean that we have to reconsider the clinical relevance of alcohol dependence or gambling disorder or even removing them from the ICD?

As we all know, IGD has not yet been recognized as a mental disorder. With few exemptions in some Asian countries, European inpatient and outpatient clinics are not offering specific intervention programs for IGD patients on a regular basis. Indeed, still many clinicians are not aware that IGD exists and consequently are not assessing diagnostic criteria for IGD among patients. If there are only few places where patients with IGD can appropriately be treated, it is no surprise that these patients might not easily be found.

EXCESSIVE? COMPULSIVE? ADDICTIVE? THE DIAGNOSTIC DEBATE CONTINUOUS

The variety of empirical results from all over the world are impressively demonstrating that we have not yet reached the stage, where exploratory research is being dismissed by more theory-driven approaches. We definitely have too many empirical findings that are standing on their own and endeavors focusing on replicating these findings are scarce.

By referring to the current debate on diagnostic criteria on IGD, the authors reveal with right that a broad consensus has not been reached yet (see also Griffiths et al., 2016; Kuss, Griffiths, & Pontes, 2016; Müller, 2017). But again, this only stresses the need for intensifying research on this field. It is neither indicative for beginning to ignore the phenomenon of IGD nor for refraining from defining it as a mental disorder.

By the way, we should not forget that in the paper by Griffiths et al. (2016), the authors are referring to is mainly related to the diagnostic criteria proposed for IGD. It does not contain serious doubts regarding the fact that IGD is a health issue but rather puts into question the fact that an "international consensus" has been reached by the mere proposal of nine diagnostic criteria.

To conclude, by referring to diagnostic uncertainties among researchers and – perhaps even more important – clinicians, Aarseth et al. (2016) hit an important point. And that is exactly why we desperately require reliable criteria for assessing IGD, for providing clear definitions of those criteria to enable (clinical) experts in the field to put a reliable diagnosis. And – rhetorical question – where is the right place for such diagnostic criteria? The due place might be the ICD-11.

SYMPTOM OR DISEASE? A RECURRING DEBATE

Among their third argument, the authors are referring to high rates of comorbid disorders among IGD patients. There is little doubt that IGD is frequently accompanied by other mental disorders. However, while these associations have repeatedly been documented, we are far from knowing the causality of these associations. Clinical psychology and psychiatry has taught us that one mental disorder enhances the risk of developing further psychiatric symptoms and even a second mental disorder. Even more important is that the high rates of comorbid disorders are also present in other addiction disorders, for example, alcohol dependence and gambling disorder (e.g., Petry, Stinson, & Grant, 2005; Regier et al., 1990). This does not mean that the mere existence of comorbid disorders is automatically a better explanation for the health condition under examination. However, it stresses the fact that we have to apply sound diagnostic measures, when assessing IGD in a clinical context.

MORAL PANIC AND STIGMA?

Some of the arguments provided in the first part of the contribution can be shared to a certain extent. However, the conclusions presented by the authors in the second part of their review are a serious matter of concern.

Calling for research on the "exploration of the boundaries of normal versus pathological" is a crucial point that undoubtedly deserves our full attention. We shall be aware that there are still many question marks left in the research on IGD and these must not be forgotten. Alternative hypotheses require to be tested – this is an essential aspect of good scientific practice. However, arguing that having a clear diagnostic framework for IGD – as it is the case in the DSM-5 – would tempt the scientific community to "stop conducting necessary validity research" has to be called a

presumptuous position. The notion is implied that the authors perceive themselves as the only saviors of good scientific practice. Apart from the probability that there are further skilled researchers out there, the authors should take another look into the DSM-5. As can be seen there, IGD has been included in Section 3 and explicitly defined as a "condition for further study" (APA, 2013)!

Unfortunately, the weakest argument is given at the end of the paper. By stating that "The healthy majority of gamers will be affected by stigma and perhaps even changes in policy," it becomes more than obvious that the authors are forgetting about those the DSM-5 and the ICD-11 are meant for the patients. Fortunately, there are way more individuals with a healthy use of computer games than patients suffering from IGD. However, those in need of help should not be impaired from getting help – hopefully, this is a point the authors would agree with. One prerequisite for being in the position of receiving therapeutic help is having a clear diagnosis a therapist can rely on – and here we are finally, leaving the scientific ivory tower behind and understanding that clinical reality demands having an ICD diagnosis of IGD. Thus, to conclude, instead of being afraid of "moral panic," we have to be aware of the opportunities for treatment an ICD diagnosis can offer.

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