PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Study Protocol of the Internet user Cohort for Unbiased Recognition of gaming disorder in Early Adolescence (iCURE), Korea, 2015–2019
AUTHORS	Jeong, Hyunsuk; Yim, Hyun Woo; Jo, Sun Jin; Lee, Seung-Yup; Kim, Eunjin; Son, Hye Jung; Han, Hyun-ho; Lee, Hae Kook; Kweon, Yong-Sil; Bhang, Soo-young; Choi, Jung-Seok; Kim, Bung-Nyun; Gentile, Douglas A.; Potenza, Marc

VERSION 1 – REVIEW

REVIEWER	Daniel Kardefelt-Winther Department of Clinical Neuroscience, Karolinska Institutet. Sweden
REVIEW RETURNED	08-Jul-2017

GENERAL COMMENTS	This protocol describes a longitudinal study of gaming disorder in early adolescence in Korea. The aim is to clarify the natural and clinical courses of IGD proposed in DSM-5 and to evaluate its risk and protective factors. Generally, this protocol details a comprehensive and ambitious project. It responds adequately to several criticisms that have been voiced in the field with regards to how studies of IGD are typically conducted. There are four very strong points to this study: 1. Its longitudinal design will enable the researchers to determine if there are any permanent, long-term negative outcomes from the proposed 'gaming disorder', which so far has rarely been demonstrated in research. Rather, the behaviour seems to be transient for many individuals. It will be critical that the research team does not only consider 'meeting criteria for IGD' as a harmful outcome, because many of these criteria do not truly indicate life-impairing harm. Rather, the team should look at actual instances of self-reported life-impairment and distress as a direct consequence of gaming through the clinical interviews. 2. Using repeated clinical interviews for a sub-sample is key to get a clear picture of the course of the disorder as well as the utility of the clinical interview itself, which is arguably more important. Given the many criticisms voiced against DSM-5 criteria and their possible unsuitability for clinical interviews, this is important to determine whether the first interview yielded accurate results and provided a useful picture. I would strongly suggest that the research team draws primarily on data from these interviews when describing and

characterizing IGD, above and beyond survey data, as the former is more likely to yield unique insights. This is particularly true given that survey-based assessments of IGD relying on DSM-5 criteria are controversial and unlikely to accurately capture truly disordered behavior. However, it would be beneficial to also assess whether the IGD criteria were useful in the clinical interview, or if the participants felt that they were inaccurate or emphasized wrong aspects of their behaviours.

3. The authors explicitly address the replication crisis in many sciences (e.g.,
Munafo M. R., Nosek B. A., Bishop D. V. M., Button K. S., Chambers C. D., Percie du Sert N. et al. A manifesto for reproducible science. Nat Hum Behav2017; 1: 0021), aiming with this protocol to document a priori the research methods, goals and approach. Such transparent Open Science approaches are extremely valuable and the authors should be commended for this initiative; it is critical in order to produce scientific results that are unbiased and reliable.

I would encourage the research team to not only register this first study protocol, but to continue with the Open Science approach and also pre-register every research paper plan, including hypotheses, as well as providing the raw data for public scrutiny (e.g., at http://www.osf.io). This would ensure that the results of this project are transparent, replicable and reliable. As a project of this magnitude is likely to yield an uncountable number of 'significant effects', an Open Science approach will help researchers decide a priori which effects are likely to be most crucial for theory/treatment and then test these in a transparent fashion, rather than flooding the field with a great number of 'significant effects' that lack meaning.

4. The study involves a large number of environmental/social questions for the baseline survey, in addition to only looking at psychological/mental health variables. This is important, because more robust and recent research tends to find that contextual factors are important in determining why children's use of digital technology results in positive or negative outcomes for their well-being. I encourage the research team to make great use of these background variables, more so than factors relating to the actual gaming, because they are likely to explain a great deal about why eventual problem behaviours occur. It also helps to avoid overly deterministic explanations, for example that time spent gaming would somehow 'cause' gaming addiction, which many people still believe and repeatedly find through correlational analysis or incomplete regression models that discount environmental factors.

Regrettably, there are also two critical concerns with the study design which potentially undermines a sound interpretation of the data and interferes with data collection. As this is a key element of the evaluation of a study protocol as per BMJ Open's standards, this requires some elaboration. Due to the ongoing nature of the study some of these issues will be difficult to rectify, but it is worth highlighting here for the public record as well as to give the authors the opportunity to address these points in their interpretation of data and in any future publications or policy statements.

1. I wonder about the justification for selecting a high-risk IGD sub-cohort based on participants meeting only 3 or more criteria for IGD. I believe this requires considerable elaboration in the study protocol.

My concern is that because many IGD criteria in DSM-5 arguably indicate normal, healthy gaming behavior (preoccupation, tolerance, withdrawal, to mention a few, see: Griffiths, M., van Rooij, A., Kardefelt-Winther, D., et al. (2016). Working towards an international consensus on criteria for assessing Internet Gaming Disorder: A critical commentary on Petry et al (2014). Addiction, 111, p. 167-175.), this sub-cohort may not actually represent gamers at risk but just regular gamers. It seems a somewhat low cut-off, given existing problems with IGD criteria.

Furthermore, the notion of a high-risk group when it comes to everyday behaviours and hobbies is deeply problematic, as it risks stigmatizing young people who enjoy engaging in particular activities that are ultimately healthy. Would we consider semi-professional adolescent soccer players as a 'high-risk' sub-cohort, if they keep thinking about soccer when not playing, wanting to play more and more soccer, or feeling irritated or depressed when they can't play soccer? I very much doubt it. In this respect I am also curious about Korea's e-sports stars – are they considered as having a gaming disorder since they practice all day every day? Or are they exempt because of their financial/societal success? I would be interested to see how many of the IGD criteria they meet – at least, I think it's likely that they meet criteria for pre-occupation, tolerance and withdrawal and could thus be included in the high-risk sub-cohort for this study, but is that truly what we are looking to capture? My point is that it would have been useful when selecting this sub-cohort to do so based on the clinical interviews instead, to ensure that actual problem gamers were included. Currently, I worry that highly engaged gamers who experience very few problems because of their gaming will be included in this cohort.

Havig said that, there is an opportunity here if these individuals are recognized as unproblematic gamers and compared to those who experience self-reported, severe, life-impairing problems as a direct result of their gaming, but any such inquiry must be done outside of the restrictive framework of the IGD diagnosis in DSM-5 and instead be based on the clinicians' judgment.

2. The study protocol aims for an 'unbiased' recognition of gaming disorder in early adolescence, as defined in its name. However, the study design is clearly not unbiased because A) it has already been assumed (and stated several times in the protocol) that gaming disorder is real problem in Korea, and B) it draws on the proposed DSM-5 diagnosis for IGD which pre-supposes a disorder-lens (specifically, an addiction-lens) to the behavior.

A truly unbiased study would not pre-suppose that gaming disorder exists, especially given the unresolved controversies in this field. Rather, it would conduct qualitative and clinical interviews with highly engaged gamers to explore whether their involvement in gaming can be described as disordered, given the thoughts, experiences and feeling of the gamers and the outcomes from their gaming behavior. This should then be compared to people who are highly engaged in other activities to verify whether this is unique to gaming or a broader phenomenon. This way, the research team could avoid falling into a confirmatory trap where the team only finds something because they went looking for it.

Second, an unbiased study would not be using pre-defined IGD criteria, because this pre-supposes that these adequately define

gaming disorder. As stated in the protocol (p. 10), for this study "interviews assessed participants based on the nine DSM-5 IGD Criteria". My concern is once more that in the interviews, the research team will find what they are looking for by virtue of simply looking for it. That is to say, they are not conducting an unbiased investigation into the gaming behavior of the participants – rather, they are evaluating a behaviour against an pre-defined set of criteria that have not been properly evaluated or consensually agreed upon. While this is of course typical for a clinical interview, it requires a properly validated instrument in order to be effective. It is not an unbiased inquiry, as the DSM-5 IGD criteria bring with it plenty of (disputed) assumptions about what the criteria actually indicate. I find that in general, the study protocol does not consider the large body of critical literature on this topic, which makes it somewhat unbalanced. In particular, criteria for preoccupation, withdrawal and tolerance can arguably indicate perfectly normal interest or passion in relation to everyday behaviours or hobbies. See for example:

Starcevic V. Tolerance and withdrawal symptoms may not be helpful to enhance understanding of behavioural addictions. Addiction 2016;111(7):1307-08.

King D, Delfabbro P. Is preoccupation an oversimplication? A call to examine cognitive factors underlying internet gaming disorder. Addiction 2014;109:1566–70.;

Kardefelt-Winther, D., Heeren, A., Schimmenti, A., Van Rooij, A. J., Maurage, P., Colder Carras, M., … Billieux, J. (2017). How can we conceptualize behavioral addiction without pathologizing common behaviors? Addiction, 1–7. https://doi.org/10.1111/add.13763.

If there is an opportunity, I would strongly suggest that the research team considers conducting a truly unbiased investigation into highly engaged gamers and whether the gaming has truly, measurable and life impairing problematic outcomes (i.e., not 'problematic' as defined by IGD criteria in DSM-5, as about half of these criteria do not indicate a truly harmful outcome).

This is particularly important given that recent qualitative studies with potential IGD candidates reveal that criteria are not always useful or accurate: "The interviews showed that the respondents often misunderstood the intention of the questions, misjudged the severity of the negative effects that the questions probed and often interpreted the questions very differently. Only one of the respondents believed pathological gaming to be a primary disorder, but he also believed it to promote more positive than negative effects. The rest of the respondent either did not believe in the disorder at all or believed it to be secondary to other problems, such as anxiety or depression." (from Nielsen, 2015: http://ieeexplore.ieee.org/document/7399493/?reload=true).

Or another recent qualitative study with gamers in China: "When we investigate the social existence of online gamers labeled Internet addicts in China, and then subject their social existence to the DSM's own definition of a mental disorder, we discover not a clearly understood mental disorder called Internet gaming disorder but more so an issue of social deviance." (excerpt from Bax, 2015: http://journals.sagepub.com/doi/abs/10.1177/1555412014568188).

It seems to me that a very useful outcome of this project could be to

try to characterize the gaming behaviours and thoughts, feelings and outcomes of those considered as potentially having IGD, rather than only exploring our pre-conceived notion of what IGD should look like and see if we can find these behaviours in gamers. This would be a truly unbiased inquiry that could yield important insights into why, for some, gaming might lead to problematic consequences above and beyond what we currently capture in DSM-5. Such an approach would have 3 concrete positive outcomes:

- It would help us shift from an adult-centric view of gaming disorder to instead account for the voices and experiences of adolescent gamers when trying to understand problems with gaming, because it is only by listening to them that we can understand why they engage in these behaviours.

- It would open up for an improvement of IGD criteria, as several important dimensions could be currently missing due to a broad lack of understanding of online gaming by many researchers and adults.

- It could also reveal misunderstandings to current conceptualizations of IGD, which would be important to avoid misclassifying healthy individuals as having a disorder. No one stands to gain from such misclassification – not parents, not children, not researchers.

Given recent revelations with regards to internet addiction treatment camps in certain countries and reports of physical and psychological abuse, a truly unbiased inquiry of gaming behaviours (not disorder), seems more important than ever. I urge the authors to consider these points for the next waves of data collection and clinical interviews and wish them the best of luck in their endeavours.

REVIEWER	Cheng-Fang Yen Kaohsiung Medical University, Taiwan
REVIEW RETURNED	14-Jul-2017

GENERAL COMMENTS	This study is well-designed and comprehensive. The results of longitudinal study can provide new knowledge to the filed of internet gaming disorder. As a study protocol, I don't think that the current manuscript needs any revision. However, I would like to provide some suggestions to the authors for their further analysis. Firstly, there are many measures in this study. As the description in the methods section, students will complete the self-reported measures during their class time. It means no one-by-one interview based on the research questionnaires. The authors need to ascertain that the adolescent students can tolerate, as well as whether the self-reported results from a group of adolescents who are experiencing high stress from academic achievement are reliable. Secondly, the authors said that they will send the parents or guardians the results of diagnostic interview and adolescents' self-reported mental indicators. Although the students are not adults and are guarded by their parents or guardians, they may want to keep their privacy from being known by their parents or guardians. Whether the study design may violate the adolescents' will need to be kept in mind.

	Thirdly, in the current study design, the authors may hypothesize the nine criteria in the DSM-5 internet gaming disorder (IGD) as the same validity to the diagnosis. However, a previous study (Journal of Psychiatric Research 2014; 53:103-110) did not support the hypothesis. Further analysis is needed to examine the validity of these nine criteria.
	Fourthly, although this study makes the diagnosis of IGD by using the diagnostic interview, the functional impairment caused by IGD needs further attention. Impairments of self-care, physical health, interpersonal relationship, and academic performance caused by IGD should be formally evaluated.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Daniel Kardefelt-Winther
Institution and Country: Department of Clinical Neuroscience, Karolinska Institutet. Sweden
Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below
This protocol describes a longitudinal study of gaming disorder in early adolescence in Korea. The aim is to clarify the natural and clinical courses of IGD proposed in DSM-5 and to evaluate its risk and protective factors.

Generally, this protocol details a comprehensive and ambitious project. It responds adequately to several criticisms that have been voiced in the field with regards to how studies of IGD are typically conducted. There are four very strong points to this study:

1. Its longitudinal design will enable the researchers to determine if there are any permanent, long-term negative outcomes from the proposed 'gaming disorder', which so far has rarely been demonstrated in research. Rather, the behaviour seems to be transient for many individuals. It will be critical that the research team does not only consider 'meeting criteria for IGD' as a harmful outcome, because many of these criteria do not truly indicate life-impairing harm. Rather, the team should look at actual instances of self-reported life-impairment and distress as a direct consequence of gaming through the clinical interviews.

⌐ Response: Thank you for the comment. We agree to your opinion. Gaming behaviors in adolescents might be transient. We thought that it is very important to verify the consequences of the gaming behaviors through clinical interviews. We will evaluate the relationship between criteria for IGD and clinical outcome through the further follow-up assessments.

2. Using repeated clinical interviews for a sub-sample is key to get a clear picture of the course of the disorder as well as the utility of the clinical interview itself, which is arguably more important.

Given the many criticisms voiced against DSM-5 criteria and their possible unsuitability for clinical interviews, this is important to determine whether the first interview yielded accurate results and provided a useful picture. I would strongly suggest that the research team draws primarily on data from these interviews when describing and characterizing IGD, above and beyond survey data, as the former is more likely to yield unique insights. This is particularly true given that survey-based assessments of IGD relying on DSM-5 criteria are controversial and unlikely to accurately capture

truly disordered behavior. However, it would be beneficial to also assess whether the IGD criteria were useful in the clinical interview, or if the participants felt that they were inaccurate or emphasized wrong aspects of their behaviours.

⌐ Response: We think the reviewer's comment is very important. We focused on the clinical interview. We are also planning to compare clinical interview and survey to evaluate the gaps through the initial interview. It might be helpful to characterize IGD criteria by comparing between the two methods.

3. The authors explicitly address the replication crisis in many sciences (e.g., Munafo M. R., Nosek B. A., Bishop D. V. M., Button K. S., Chambers C. D., Percie du Sert N. et al. A manifesto for reproducible science. Nat Hum Behav2017; 1: 0021), aiming with this protocol to document a priori the research methods, goals and approach. Such transparent Open Science approaches are extremely valuable and the authors should be commended for this initiative; it is critical in order to produce scientific results that are unbiased and reliable.
I would encourage the research team to not only register this first study protocol, but to continue with the Open Science approach and also pre-register every research paper plan, including hypotheses, as well as providing the raw data for public scrutiny (e.g., at http://www.osf.io). This would ensure that the results of this project are transparent, replicable and reliable. As a project of this magnitude is likely to yield an uncountable number of 'significant effects', an Open Science approach will help researchers decide a priori which effects are likely to be most crucial for theory/treatment and then test these in a transparent fashion, rather than flooding the field with a great number of 'significant effects' that lack meaning.

⌐ Response: We agree to the reviewer's comment. We have already registered this protocol including the pre-specified primary endpoint at clinicaltrials.gov before starting the study. To ensure the transparency of the study, we have to upload the raw data periodically in the iCREAT, web-based research management system of the Korean Control Disease Center (KCDC) because this project was supported by a Korean government fund. Government funded researches are required to upload raw data in Korea, and the iCREAT system is operated by strict audit trail principles and is not allowed to edit or correct the data arbitrarily without the authority's permission.

4. The study involves a large number of environmental/social questions for the baseline survey, in addition to only looking at psychological/mental health variables. This is important, because more robust and recent research tends to find that contextual factors are important in determining why children's use of digital technology results in positive or negative outcomes for their well-being. I encourage the research team to make great use of these background variables, more so than factors relating to the actual gaming, because they are likely to explain a great deal about why eventual problem behaviours occur. It also helps to avoid overly deterministic explanations, for example that time spent gaming would somehow 'cause' gaming addiction, which many people still believe and repeatedly find through correlational analysis or incomplete regression models that discount environmental factors.

⌐ Response: Thank you for the reviewer's recommendation. We will reveal the causes of gaming addiction through considering using various environmental and background variables as well as game exposure variables. Especially, we plan to analyze the mediation factors to prevent gaming addiction.

Regrettably, there are also two critical concerns with the study design which potentially undermines a sound interpretation of the data and interferes with data collection. As this is a key element of the evaluation of a study protocol as per BMJ Open's standards, this requires some elaboration. Due to the ongoing nature of the study some of these issues will be difficult to rectify, but it is worth highlighting here for the public record as well as to give the authors the opportunity to address these points in their interpretation of data and in any future publications or policy statements.

1. I wonder about the justification for selecting a high-risk IGD sub-cohort based on participants meeting only 3 or more criteria for IGD. I believe this requires considerable elaboration in the study protocol. My concern is that because many IGD criteria in DSM-5 arguably indicate normal, healthy gaming behavior (preoccupation, tolerance, withdrawal, to mention a few, see: Griffiths, M., van Rooij, A., Kardefelt-Winther, D., et al. (2016). Working towards an international consensus on criteria for assessing Internet Gaming Disorder: A critical commentary on Petry et al (2014). Addiction, 111, p. 167-175.), this sub-cohort may not actually represent gamers at risk but just regular gamers. It seems a somewhat low cut-off, given existing problems with IGD criteria.

Furthermore, the notion of a high-risk group when it comes to everyday behaviours and hobbies is deeply problematic, as it risks stigmatizing young people who enjoy engaging in particular activities that are ultimately healthy. Would we consider semi-professional adolescent soccer players as a 'high-risk' sub-cohort, if they keep thinking about soccer when not playing, wanting to play more and more soccer, or feeling irritated or depressed when they can't play soccer? I very much doubt it. In this respect I am also curious about Korea's e-sports stars – are they considered as having a gaming disorder since they practice all day every day? Or are they exempt because of their financial/societal success? I would be interested to see how many of the IGD criteria they meet – at least, I think it's likely that they meet criteria for pre-occupation, tolerance and withdrawal and could thus be included in the high-risk sub-cohort for this study, but is that truly what we are looking to capture? My point is that it would have been useful when selecting this sub-cohort to do so based on the clinical interviews instead, to ensure that actual problem gamers were included. Currently, I worry that highly engaged gamers who experience very few problems because of their gaming will be included in this cohort. Having said that, there is an opportunity here if these individuals are recognized as unproblematic gamers and compared to those who experience self-reported, severe, life-impairing problems as a direct result of their gaming, but any such inquiry must be done outside of the restrictive framework of the IGD diagnosis in DSM-5 and instead be based on the clinicians' judgment.

∖ Response: Thank you for the comment. We understand your concern. We designated 3 or more criteria for IGD as "high-risk" sub-cohort, and we very much agree with you that they are probably a healthy gamer. We also agree with the idea that professional gamers are not gaming addicted. According to our experience so far, there is little chance that professional gamers are included in the high risk IGD group, because most of the professional gamers did not meet the 3 or more IGD criteria. The term "high risk group" could lead to bias, so we replaced "high-risk sub-cohort" by "clinical evaluation sub-cohort" in the manuscript. Thank you for helping us get a good idea for achieving unbiased results.

∖ The reason why we selected as a "clinical evaluation sub-cohort" was to observe the natural history or clinical course of them even though they did not reach the DSM-5 IGD diagnostic criteria. We believed following up the "clinical evaluation sub-group" would reveal whether they actually turned out to be IGD or normal behavior. We included the above contents in the manuscript. Please see page 10 marked in blue.

∖ Although some of the professional gamers may be included in the clinical evaluation sub-cohort, we will confirm if they are not gaming-addicted through follow-up observations. Our protocol contains follow-up assessments including quality of life and incidences of psychiatric comorbidities. If professional gamers are not gaming addicted, their quality of life will not decline and psychological comorbidities such as depression and anxiety will not occur because of gaming.

We believe that follow up observations will resolve this issue. Unfortunately, we did not select the clinical evaluation sub-cohort from the participants who were evaluated by clinical interviews. As a matter of fact, this problem might be very hard to be tackled, because this study has already been underway.

2. The study protocol aims for an 'unbiased' recognition of gaming disorder in early adolescence, as

defined in its name. However, the study design is clearly not unbiased because A) it has already been assumed (and stated several times in the protocol) that gaming disorder is real problem in Korea, and B) it draws on the proposed DSM-5 diagnosis for IGD which pre-supposes a disorder-lens (specifically, an addiction-lens) to the behavior.

A truly unbiased study would not pre-suppose that gaming disorder exists, especially given the unresolved controversies in this field. Rather, it would conduct qualitative and clinical interviews with highly engaged gamers to explore whether their involvement in gaming can be described as disordered, given the thoughts, experiences and feeling of the gamers and the outcomes from their gaming behavior. This should then be compared to people who are highly engaged in other activities to verify whether this is unique to gaming or a broader phenomenon. This way, the research team could avoid falling into a confirmatory trap where the team only finds something because they went looking for it.

⌞ Response: Thank you for your comment. We added the following sentences in the manuscript: "However, the classification and rationale for DSM-5 IGD as a mental health disorder have been controversial (Griffiths et al, 2016). Previous researchers proposed the possibilities that gaming behavior can be common behavior or leisure activity. Excessive involvement on gaming, per se, might reflect engagement, passion, or coping (Kardefelt-Winther, 2017). Several IGD criteria such as withdrawal and tolerance are often inappropriate to be put into the conceptualization of behavioral addiction (Starcevic, 2016) and difficult to apply convincingly and measure in relation to behaviors. Excessive behaviors on gaming seem to be transient for many adolescents and we will look into whether excessive behavior on gaming is episodic or steady through the longitudinal observation." Please see page 5 marked with blue color in the introduction section.

Second, an unbiased study would not be using pre-defined IGD criteria, because this pre-supposes that these adequately define gaming disorder. As stated in the protocol (p. 10), for this study "interviews assessed participants based on the nine DSM-5 IGD Criteria". My concern is once more that in the interviews, the research team will find what they are looking for by virtue of simply looking for it. That is to say, they are not conducting an unbiased investigation into the gaming behavior of the participants – rather, they are evaluating a behaviour against an pre-defined set of criteria that have not been properly evaluated or consensually agreed upon. While this is of course typical for a clinical interview, it requires a properly validated instrument in order to be effective. It is not an unbiased inquiry, as the DSM-5 IGD criteria bring with it plenty of (disputed) assumptions about what the criteria actually indicate. I find that in general, the study protocol does not consider the large body of critical literature on this topic, which makes it somewhat unbalanced. In particular, criteria for preoccupation, withdrawal and tolerance can arguably indicate perfectly normal interest or passion in relation to everyday behaviours or hobbies. See for example:

Starcevic V. Tolerance and withdrawal symptoms may not be helpful to enhance understanding of behavioural addictions. Addiction 2016;111(7):1307-08.

King D, Delfabbro P. Is preoccupation an oversimplication? A call to examine cognitive factors underlying internet gaming disorder. Addiction 2014;109:1566–70.;

Kardefelt-Winther, D., Heeren, A., Schimmenti, A., Van Rooij, A. J., Maurage, P., Colder Carras, M., … Billieux, J. (2017). How can we conceptualize behavioral addiction without pathologizing common behaviors? Addiction, 1–7. https://doi.org/10.1111/add.13763.

If there is an opportunity, I would strongly suggest that the research team considers conducting a truly unbiased investigation into highly engaged gamers and whether the gaming has truly, measurable and life impairing problematic outcomes (i.e., not 'problematic' as defined by IGD criteria in DSM-5, as about half of these criteria do not indicate a truly harmful outcome).

This is particularly important given that recent qualitative studies with potential IGD candidates reveal that criteria are not always useful or accurate: "The interviews showed that the respondents often misunderstood the intention of the questions, misjudged the severity of the negative effects that the

questions probed and often interpreted the questions very differently. Only one of the respondents believed pathological gaming to be a primary disorder, but he also believed it to promote more positive than negative effects. The rest of the respondent either did not believe in the disorder at all or believed it to be secondary to other problems, such as anxiety or depression." (from Nielsen, 2015: http://ieeexplore.ieee.org/document/7399493/?reload=true).

Or another recent qualitative study with gamers in China: "When we investigate the social existence of online gamers labeled Internet addicts in China, and then subject their social existence to the DSM's own definition of a mental disorder, we discover not a clearly understood mental disorder called Internet gaming disorder but more so an issue of social deviance." (excerpt from Bax, 2015: http://journals.sagepub.com/doi/abs/10.1177/1555412014568188).

It seems to me that a very useful outcome of this project could be to try to characterize the gaming behaviours and thoughts, feelings and outcomes of those considered as potentially having IGD, rather than only exploring our pre-conceived notion of what IGD should look like and see if we can find these behaviours in gamers. This would be a truly unbiased inquiry that could yield important insights into why, for some, gaming might lead to problematic consequences above and beyond what we currently capture in DSM-5. Such an approach would have 3 concrete positive outcomes:

- It would help us shift from an adult-centric view of gaming disorder to instead account for the voices and experiences of adolescent gamers when trying to understand problems with gaming, because it is only by listening to them that we can understand why they engage in these behaviours.
- It would open up for an improvement of IGD criteria, as several important dimensions could be currently missing due to a broad lack of understanding of online gaming by many researchers and adults.
- It could also reveal misunderstandings to current conceptualizations of IGD, which would be important to avoid misclassifying healthy individuals as having a disorder. No one stands to gain from such misclassification – not parents, not children, not researchers.

Given recent revelations with regards to internet addiction treatment camps in certain countries and reports of physical and psychological abuse, a truly unbiased inquiry of gaming behaviours (not disorder), seems more important than ever. I urge the authors to consider these points for the next waves of data collection and clinical interviews and wish them the best of luck in their endeavours.

⌊ Response: Thank you for your thoughtful review. Because the DSM-5 IGD criteria were proposed for the further study, we have devoted ourselves to diagnosing IGD through clinical interviews. The clinical interviews for IGD were considered the dependence syndrome criteria in ICD-10. Interviewers diagnosed IGD through evaluating clinically significant functional impairments which were substantially contributed by the gaming problem per se and were not derived from other existing psychiatric illnesses. In addition, the nine DSM-5 IGD criteria were assessed in accordance with an international consensus for IGD using the DSM-5 approach which was suggested by Petry et al. (2014). Psychiatric comorbidities were also thoroughly evaluated by the psychiatrists using the semi-structured tool of the Kiddie-Schedule for Affective Disorders and Schizophrenia-Present and Lifetime Version-Korean version (K-SADS-PL). Global function was assessed by the children's global assessment scale (GAS) based on the child's worst level of emotional and behavioral functioning in the past three months by selecting the lowest level which described his/her functioning on a hypothetical continuum of health-illness.

⌊ We think this study is a starting point for finding out true criteria for diagnosing IGD based on the results and the readers will be able to make judgments on the suggested DSM-5 IGD criteria.

Long-term follow-up observations will confirm whether adolescent game behavior ultimately results in clinical impairment or transient behavior. DSM-5 criteria must be verified to ensure that the criteria are correct. We will look at other outcome measures to see whether IGD is pathological or not.

Reviewer: 2
Reviewer Name: Cheng-Fang Yen

Institution and Country: Kaohsiung Medical University, Taiwan
Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below
This study is well-designed and comprehensive. The results of longitudinal study can provide new knowledge to the filed of internet gaming disorder. As a study protocol, I don't think that the current manuscript needs any revision. However, I would like to provide some suggestions to the authors for their further analysis.
⌊ Response: Thank you for your comment; we really appreciate you.

Firstly, there are many measures in this study. As the description in the methods section, students will complete the self-reported measures during their class time. It means no one-by-one interview based on the research questionnaires. The authors need to ascertain that the adolescent students can tolerate, as well as whether the self-reported results from a group of adolescents who are experiencing high stress from academic achievement are reliable.

⌊ Response: Thank you for the valuable comment. A web-based survey was administered to middle school students, and one by one interview was used for an elementary school. A feasibility test of the whole questionnaires was conducted to see if it could be performed for 3rd grade students before conducting the survey. Most of the children complete the survey. In some cases, research assistants had to give them one-by-one help to complete it. Also We will consider what you suggested when we analyze the data.

Secondly, the authors said that they will send the parents or guardians the results of diagnostic interview and adolescents' self-reported mental indicators. Although the students are not adults and are guarded by their parents or guardians, they may want to keep their privacy from being known by their parents or guardians. Whether the study design may violate the adolescents' will need to be kept in mind.

⌊ Response: This point was the most important issue when we designed this study. We also have some worries about this point. Because it may pose an ethical problem if not giving any comments to the caregivers after the clinical examination, we plan to send out the result expressed only as the signal lights (i.e., red, yellow, green) representing the level of danger. However, we do not report to the parents about detailed game time or game activities. We will try our best to protect the rights of the youth participating in the long term research.

Thirdly, in the current study design, the authors may hypothesize the nine criteria in the DSM-5 internet gaming disorder (IGD) as the same validity to the diagnosis. However, a previous study (Journal of Psychiatric Research 2014; 53:103-110) did not support the hypothesis. Further analysis is needed to examine the validity of these nine criteria.

⌊ Response: Thank you for the important point. We have already reviewed your article when we prepared our research plan. It was valuable results from the clinical interviews. Longitudinal follow-ups are likely to confirm or deny the DSM-5 criteria. We included this issue in the manuscript. Please see page 6, introduction section.

We will also try to analyze whether or not the nine criteria in the DSM-5 IGD have the same validity for the diagnosis. We are deeply grateful for your advice.

Fourthly, although this study makes the diagnosis of IGD by using the diagnostic interview, the functional impairment caused by IGD needs further attention. Impairments of self-care, physical health, interpersonal relationship, and academic performance caused by IGD should be formally

evaluated.

⌐ Response: Thank you for your comment. We strongly agreed with you. We consider diagnosing IGD based on clinically significant functional impairments that are substantially derived from gaming and not from any other psychiatric illnesses. Also, we plan to measure follow up assessments including, impairments of self-care, physical health, interpersonal relationship, and academic performance caused by IGD etc. In addition, the results will be analyzed to reveal the true causes of the functional impairments.

VERSION 2 – REVIEW

REVIEWER	Daniel Kardefelt-Winther
	Department of Clinical Neuroscience, Karolinska Institutet, Sweden.
REVIEW RETURNED	14-Aug-2017

GENERAL COMMENTS	The major points of the peer-review have been addressed in this revised version, though briefly. In particular, the added sentence on follow-up with individuals in the clinical evaluation sub-cohort to determine whether these children experienced long-term problems from their gaming behavior or not is crucial. This protocol will be useful for the authors when analyzing and interpreting data. It provides a good starting point for pre-registering additional study protocols before hypothesis testing, to ensure transparency in analysis and reporting.