Author’s response to reviews

Title: Associations of personality traits with Internet addiction in Chinese medical students: the mediating role of attention-deficit/hyperactivity disorder symptoms

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Author’s response to reviews:

Dear editor,

We appreciate your letter and the two reviewers’ comments concerning our paper entitled “Associations of personality traits with Internet addiction in Chinese medical students: the mediating role of attention-deficit/hyperactivity disorder symptoms” (BPSY-D-19-00133). We have revised our manuscript after carefully reading the comments, and thoroughly proofread the manuscript before turning it in. All changes were highlighted in red color. Below is point-by-point description of our revision according to the comments.

Reviewer 1 (Ursula Eleonore Oberst)

1. Why medical students? The authors dwell on the cultural differences in IA, but there are already many studies on IA addiction in Chinese populations, and also with respect to its relationship to personality and ADHD. Can the authors justify more clearly why it is relevant to use a sample of medical students, specifically? What makes them different as a population? It could have also been interesting to see if there are differences between medical students and other student groups or med students and doctors, etc.

Response: Though preliminary evidence has shown that the prevalence of IA in medical students may not be significantly different from that of other student groups [1-3], it is also revealed that the prevalence may not drop when medical students become junior physicians [3,4]. As IA is associated with cognitive impairment [5], quality of care and safety of patients could be negatively affected if no effective intervention strategies are undertaken to deal with the issue of IA among physicians-in-training (p.4, line 22; p.5, line 1-7).

Given the adverse consequences of IA, there are many studies on IA in Chinese populations. However, there is a paucity of relevant research conducted among medical students published on peer-reviewed international journals, particularly large multi-center studies [6]. While some
studies deal with the relations between personality traits and IA in Chinese populations, most of them use Big Three model, like Eysenck Personality Questionnaire [7], to measure personality traits instead of Big Five, which is the most widely used personality model worldwide. Five-Factor Model transcends languages and cultures, but its relation with IA and the size effect may vary significantly from culture to culture, and this relation has yet been examined in Chinese university students (p.6, line 7-8).

In addition, though the association between IA and ADHD is robust, the majority of the studies are carried out in Taiwan and South Korea [8], and the association has neither been examined in college students in mainland of China nor in medical students worldwide, despite the fact that a recent large survey has demonstrated that ADHD is the most common self-disclosed disability for medical students to receive accommodations out of all types of disabilities [9] (p.6, line 20-22; p.7, line 1-3).

Reference:


2. In my opinion, studying Internet addiction as such is not very interesting anymore; more recent literature investigates subtypes of internet addiction, or what people actually do on the internet (studying, gaming, communicating, etc.), and this can be quite different in medical students and in other students. In this context it would have also been interesting to assess how much time people spend on the internet. Can the authors justify why they used such a general approach?

Response: We appreciate the reviewer’s suggestion. Besides literature review, part of the idea of the current study came from our daily observations of medical students in traditional classrooms, where some of them seemed to use smartphones excessively and lack adequate concentration. The phenomenon we observed was in line with the findings of a recent study which showed that 95% Brazilian medical students reported using their smartphones inside the classroom for non-medical related activities, and nearly a third of the students used them “always” or “almost always” [1] (p.13, line 11-14). Meanwhile, the latest meta-analysis regarding IA in medical students revealed limited information with a small study sample in 6 nations, and no data regarding IA in Chinese medical students were available [2]. Thus, though such a general approach of IA may not be very interesting, the results of the study could still have significant implications for all the parties involved in medical education, given the possible negative effects and the prevalence of IA among future physicians.

We are very interested to investigate subtypes of smartphone usage (e.g., for academic purpose, for social interaction, for entertainment) and their respective associations with other psychological variables among the students in our future research.

Reference:


3. I also have a methodological suggestion: the authors describe the paths for a mediation model with direct (Personality on IA and on ADHD, ADHD on IA) and indirect effects (Personality - ADHD - IA), but they do not use goodness of fit indices to check the goodness of fit of their model. Is this an intentional omission or were the results insufficient to warrant adequate model fits? Obtaining adequate indices would add value to this manuscript and not having them is a limitation that should be mentioned in the manuscript.
Response: According to the reviewer’s advice, a path analysis was performed using Amos 23.0 to further test the mediation model. Examination of the goodness of fit indicated that the model was fairly adequate (chi-square/df = 1.227, CFI = 1.000, GFI = 0.999, AGFI = 0.992, RMSEA = 0.013). The results of the path analysis were shown in Fig 1, which were consistent with those of the regression analysis as well as the asymptotic and resampling strategies (p.10, line 16-17; p.12, line 12-16; p.33).

4. Language in the manuscript needs some language corrections before being published.

Response: According to the reviewer’s advice, we thoroughly proofread the whole manuscript and tried our best to correct language errors.

Reviewer 2 (Katalin Felvinczi)

1. The methods are mostly well chosen though some further argumentation would have been needed regarding the appropriateness of the measurement techniques especially in case of measuring internet addiction. Recent publications reveal some concerns and formulate recommendations regarding the usability of the 1998 version of IAT (Faraci et al 2013, Poli, 2017). At least some of these doubts should have been mentioned and counterarguments could have been shown to disperse these possible doubts related to the measurement tool.

Response: We appreciate the references provided by the reviewer, and thoroughly read them through and some more. More information was added concerning the measure of IAT in methods and discussion. The reason why IAT was chosen in the study was because it is the most widely used measure for Internet addiction worldwide and in China [1-3] (p.8, line 18), so that better comparison could be made in terms of prevalence of IA. Despite this, a few concerns existed regarding IAT, as pointed out by the reviewer. Firstly, out of dozens of instruments evaluating IA, none has emerged as the diagnostic gold standard [1,2] (p.13, line 1-3). Secondly, different cutoffs of IAT had been used in research [1]. The cutoff of IAT ≥ 50, adopted in the current study, was based on the IAT-Manual, which is available on the website of its developer: http://netaddiction.com/assessments/. This cutoff has been broadly used globally and in Chinese populations [3]. Thirdly, because IAT was originally developed as a unidimensional instrument, most studies using this scale have utilized the total scale score and its corresponding cutoffs [4]. However, studies examining the psychometric properties have revealed different factor structure between one- and six-factor solutions, and a consensus on the overall structure has failed to emerge [2,4-8]. Researchers have not been able to reproduce the dimensions of IAT consistently or differentiate them reliably [4]. When similar numbers of factors were extracted, differences were found in items distribution on the factors, and different names were assigned to the same factors [5,6]. Moreover, Different factorial structures and items distribution were observed in the same language version of IAT [5,6]. A systematic review of IAT construct validity supports 1 or 2 factors [6], with some research supporting both single- and two-factor models [7]. In the present study, the IAT was considered a one factor model (p.9, line 1-4).
Reference:


2. In the limitations chapter an even stronger argument could have been made regarding the cross-sectional nature of data collection; the causality of the involved constructs cannot be determined based on the available data and their analysis.

Response: A stronger argument regarding the cross-sectional nature of data was made as suggested by the reviewer (p.16, line 21-22).

Best regards,

Meng Shi