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COMMENTARY

Apples and oranges in the basket of a clinical model for exercise addiction: Rebuttal to Brevers et al. (2022)

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ABSTRACT

This note is a reply to Brevers et al.'s (2022) the commentary. We first explain that the commentary's title is in discord with the theoretical implications of the Expanded Interactional Model of Exercise Addiction (EIMEA; Dinardi et al., 2021). Subsequently, we argue that in contrast to Brevers et al.'s arguments, exercise volume or intensive physical exercise is not even mentioned in the revised EIMEA. Most importantly, we point out that the commentary's reference to assessment scales of exercise addiction is irrelevant, because the EIMEA is intended for *idiographic* clinical cases rather than nomothetic research. Furthermore, we discuss how the ELMEA cannot account for secondary exercise addiction and motivational incentives due to its individual-specific orientation. Finally, we conclude our reply by highlighting that Brevers et al.'s commentary seems to revolve around nomothetic research assessing a certain level of 'risk' of exercise addiction, while the EIMEA accounts for specific clinically dysfunctional cases presented in the limited number of case studies published in the literature.

KEYWORDS

addiction, dependence, exercise, physical activity, sport

INTRODUCTION

Title and abstract

We thank Brevers et al.'s (2022) attention to our recent article by commenting on it. Yet, the commentary addresses many issues not or only remotely pertinent to the Expanded Interactional Model of Exercise Addiction (EIMEA). Regrettably, its title implying that we see "… *excessive physical exercise as an addictive disorder*:…" is already imprecise since we do not conceptualize "*excessive physical exercise*" in any way. Such words do not occur in our report. Moreover, the commentary's Abstract connects the EIMEA to "*excessive physical exercise*" and hints that we use the Self-Determination Theory (SDT) "*to model exercise addiction,*" both being inaccurate theses. Then the commentary is organized into four subsections to which we respond below.

Inaccurate use of the Self-Determination Theory risks pathologizing intensive physical exercise. Brevers et al. state that "*In our opinion, the existing evidence challenges the proposal made by Dinardi et al. (2021), namely conceptualizing key SDT dimensions as*

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promoting physical exercise addiction." First, we make a valid statement in that "*The SDT is a valuable framework for understanding motivation in exercise, sports, and training.*" The SDT, thus, in our paper refers to the part of the EIMEA concerning the '*Incentives for exercise*' block (p. 628), which comprises healthy exercise behavior (Juwono & Szabo, 2020a; White et al., 2021). In our view, SDT plays a complementary role in identifying healthy exercise patterns, not a defining role in characterizing exercise addiction. We cited several studies that adopted the SDT to examine motivation in the exercise addiction context to illustrate its application in the field. We agree with Brevers et al. that further research is needed to understand the role of motivation in exercise addiction fully. However, we also conjecture that the SDT might be helpful in the idiosyncratic scrutiny of the disorder.

Further, Brevers et al. discuss overtraining and burnout in their commentary, which is not an issue addressed in our paper. We make no connection between athletic burnout and exercise addiction. Furthermore, they highlight that "*… incidence and prevalence rates of exercise addiction are likely inflated by misclassifying committed sportspeople who use exercise to attain their personal needs as "addicted.*"" While we agree with them, the EIMEA does not account for *prevalence rates* but only for unique clinical cases (i.e., Juwono & Szabo, 2020b). As such, the EIMEA accounts for a *shift between healthy to morbid exercise* (Egorov & Szabo, 2013) in a person at risk (i.e., Szabo, 2018), possibly showing dysfunctional exercise behavior, and *not* for an 'overall risk' measured in nomothetic research.

Finally, Brevers et al. (2022) implicate our report in "*… pathologizing intensive physical exercise,*" which is inaccurate because we do not discuss *exercise volume or intensive physical exercise* at all. Indeed, the EIMEA *does not* link exercise volume to exercise addiction because, apparently, there is no relationship between them (Szabo & Kovacsik, 2019).

Validity problems in exercise addiction measurement. Brevers et al.'s potential misunderstanding of the EIMEA also surfaces in its nomothetic view, implied by the claim that the "*…model is derived from evidences obtained through questionable but widely used exercise addiction scales.*" By making this assertion, the authors overlook the model's *idiographic* clinical orientation accounting for unique *cases* characterized by numerous possible interactions. As Egorov and Szabo (2013) stated "*The possible number of interactions between personal and situational factors is so large that each case is idiographic in a mindset resembling a secret "black-box." The box could only be opened after diagnosis with the help of mental health professionals.*" Thus, Brevers et al.'s criticism of the assessment instruments - unrelated to the EIMEA and not even mentioned in Dinardi et al. (2021), - is irrelevant here.

The primary versus secondary disorder fallacy. Brevers et al. (2022) discuss a "primary versus secondary disorder fallacy" in the context of our paper, while we state that "*This model describes only primary exercise addiction…*" p. 627). We explain how secondary exercise addiction has been conceptualized and that our model does not account for interactions between eating disorders, for example, and exercise addiction. In this context, we are puzzled by their reference to research using 'cut-off points' in controlling for secondary exercise addiction and similar comments because our model is *person-specific* and accounts for *clinical* cases. In contrast, cut-off points are used in nomothetic research that can only assess a level of *risk* of exercise addiction, which may never turn into morbidity.

Links between specific motives and distinct forms of physical activity. In this section, Brevers et al. (2022) imply that we "*claim that fitness is driven by a desire to enhance one's physical appearance or health, whereas sport participation is driven by performance,*" when in fact, instead of categorization, we only mean to provide examples of the two: "*For example, the desire to enhance one's physical appearance or health are common motivations in exercise, while seeking improved sports performance, either for competitive reasons or to achieve personal goals, is associated with training.*" Then they say: "*Perhaps even more problematic is the fact that the proposed model itself does not inform on how distinct motivational patterns differentially lead to maladaptive physical exercise.*" This view reveals a misinterpretation of the EIMEA via inference to (an expected) generalized nomothetic context, despite Egorov and Szabo's (2013) posit that: "*… nomothetic research could yield results about proneness or risk while actual clinical cases can only be examined through idiographic research.*" Indeed, the EIMEA cannot account for the *general* connections of motivational constructs to exercise addiction because it focuses on individual-specific interactions contained within the 'black box' of the person that only mental health professionals can open.

CONCLUSION

Considering that Brevers et al. focus most of their comments around nomothetic research that investigates a *level of risk of exercise addiction* that might never turn into dysfunction and that our model accounts for *clinical cases*, we feel that we owe no rebuttal to their concluding thoughts, which would amount to comparing apples to oranges. Nevertheless, it was suggested that nomothetic research should meet clinical cases through active and focused collaboration of academics with health professionals. Szabo and Rendi (2008) proposed a pyramid approach for this collaboration. Both the original and expanded Interactional Model of Exercise Addiction could serve such initiatives.

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