Reviewer’s report

Title: Treatment outcome of patients with comorbid type 1 diabetes and eating disorders

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Reviewer: Giovanni Abbate-Daga

Reviewer’s report:

The manuscript considers a well-defined and relevant topic, namely features and outcome of patients with eating disorders (EDs) and comorbid type 1 diabetes (T1DM). Although this issue has been long known, in literature there is a lack of studies on this matter, particularly as regards treatment response and dropouts. Therefore, the article, in spite of the small sample size considered, seems novel and thought-provoking.

However, it can be improved as follows:

Major Compulsory Revisions

Introduction

Saying that T1DM individuals are at “high risk” of developing ED seems too strong also given the sample (1.06% of total ED cases). I would suggest to remove “high”. Moreover, individuals at risk are characterized by purging rather than restricting symptomatology (see the meta-analysis conducted by Mannucci et al. 2005) and such datum should be discussed in the introduction and discussion also referring to the results since a mixed-diagnosis sample has been considered.

Statistical analysis

If the reviewer is not mistaken, the authors stated to assess the correlation between personality profiles and dropout, but such data are not shown either in the results or in the discussion.

Results

The features of the sample should be better described. What was the age of diabetes onset? We know indeed that age at onset is associated with the risk of EDs (Yasuko et al. 2011).

ED diagnostic distribution should be presented not only as a total, but also divided into ED + T1DM and ED without T1DM groups.

Treatment outcome: patients with ED + T1DM showed worse outcome and were more likely to dropout. It would be interesting to show the same table divided according to diagnostic groups. I know some cells may be very small but dropout rates vary across different diagnoses.

As a trivial note: the percentage of 94.74% for 18 patients out of 20 is wrong.
Discussion

The discussion is clear but somehow too succinct. For the sake of the diffusion of scientific knowledge, the authors should discuss the difference of ED patients with T1DM in comparison with the dia-bulimia phenomenon, highly common among adolescents with diabetes (ranging between 10 and 40%).

Moreover, the temperament trait of low persistence in those with both ED and T1DM should be discussed more: this finding could explain an instable rather than lower motivation and it can represent a relevant difference with respect to psychopathology (e.g., restricting vs purging).

As a limitation it should be highlighted that future studies need to investigate as to whether the presence of comorbid diabetes impacts motivation to treatment per se.

Finally, mentioning dialectical behavioral therapy seems not very pertinent since patients with ED and T1DM showed less self-harm behaviors; the authors could more properly comment instead on the need to tailor diabetes-specific treatments in EDs as well as to inform diabetologists more.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: I declare that I have no competing interests