Title: Atypical depression is more common than melancholic in fibromyalgia

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Reviewer: Kate Harkness

Reviewer's report:

The present study reports on the descriptive characteristics of patients with fibromyalgia who also suffer from melancholic or atypical major depression. Findings indicated high prevalence rates of MDE and ADE in this FM sample, and indicated that FM with either of these depression subtypes was associated with greater severity of FM symptoms are poorer quality of life indicators. This is an important topic. Nevertheless, there are a number of limitations with the authors’ design and analyses that should be addressed in order for this paper to make a substantial contribution to this literature. They are presented in the order in which they appeared in the text.

Major Compulsory Revisions

1. P. 5, top para: The authors stated that FM and ADE have “shared biological underpinnings.” I suggest that the authors briefly review what these are in order to motivate their hypothesis regarding a preponderance of ADE versus MDE in FM.

2. P. 5, top para: The hypothesis of the paper, as currently stated is that “atypical depression and melancholic depression will occur in a FM sample.” Given that 74% of FM patients have major depression and 60% of major depression can be characterized as atypical or melancholic it seems obvious that one will see both atypical and melancholic depression in FM. The hypothesis implied by the title is that one subtype (atypical) will be more prevalent in FM than another (melancholic). I suggest that the hypothesis be rephrased as such. Related to the above point, the authors state that they will “describe the demographic, clinical, and diagnostic characteristics.” Of what, specifically? This should be tied directly to the hypothesis and research question.

3. P. 6, bottom para: I am unclear as to why the SIGH-SAD-SR was administered. What relation does SAD have to ADE or MDE? How could this measure be used to “confirm the diagnoses of MDD subtypes” if (a) it is a self-report instrument, the purpose of which is not to derive a DSM-IV diagnosis, and (b) it does not include all of the specific symptoms required for an ADE diagnosis, nor does it include the majority of symptoms required for a MDE diagnosis? I suggest that this be clarified.

4. P. 6-7: I suggest that the authors clarify more explicitly how the diagnosis of MDE was derived and who performed these diagnoses. Was this just according to the DSM-IV-TR criteria? It would also be helpful to have inter-rater reliability
data on the diagnoses and SIGH-ADS interviews.

5. P. 7, 2nd para: What was the motivation for excluding the non-ADE/non-MDE patients? It seems as though it would be important to know what proportion of patients with FM and MDD meet criteria for MDE versus ADE, and whether these proportions are similar to what one sees in those with major depression outside the context of FM. This is particularly confusing because the authors included FM patients without MDD, so the only group they excluded were those who had non-MDE and non-ADE MDD. If the authors want to say that “atypical depression is more common than melancholic depression”, then it seems that reporting the proportion of each in the sample of those with depression would be necessary. The authors do report that the prevalence rate of MDE was 35.6% and of ADE was 52.6% (is this in the sample of non-MDD FM, MDE, and ADE?). Is this a statistically significant difference?

6. P. 10, 2nd para: It is unclear to what the p-values refer in Table 1. The relevant chi-square would examine the differential distribution of the demographic variable (e.g., sex) across the 3 study groups (no MDD vs. ADE vs. MDE). However, this is not how it appears to have been done in the text. For example, the authors state “With the exception of the expected predominance of females versus males (96% vs. 4%; p = 0.01), no significant differences existed between groups on demographic variables”, but this 96% vs. 4% is the sex distribution in the full sample. Was this the contingency that was tested? Clarification of exactly which groups were compared in the contingencies is necessary.

7. The large overlap in symptoms between the MDE and ADE groups, particularly in the symptoms that are required for the differential diagnosis, calls into question the reliability of the diagnoses of the groups in this study. This deserves some comment from the authors in the Discussion, and reliability data provided in the Method section would also be useful (see #4 above). How do these rates of overlap compare to what are seen between MDE and ADE in the literature on major depression outside of the context of FM?

Minor Essential Revisions
8. P. 6, 2nd full para: ADE and MDE should be spelled out in full the first time.

9. Most of the measures described in the Method section are not “outcome” measures (e.g., demographic data form). Outcome refers to one’s dependent variable, which in this case is depression subtype diagnosis.

10. Throughout the paper, there is a typo in the word ‘symptomology,’ which should be ‘symptomatology’.

Discretionary Revisions
11. P. 6, top para: Given that biological variables were not studied in the present design I was unclear why menstruating females were scheduled for a second visit during their next luteal phase. Why should menstrual cycle matter to the authors’ present research question? I suggest that the authors explicate this
Level of interest: An article of limited interest

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.