Reviewer's report

Title: The contribution of illness perception to psychological distress in heart failure patients.

Version: 2 Date: 7 July 2014

Reviewer: Pete Ellis

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This paper argues that there is a significant relationship between illness perception and depression and anxiety in heart failure patients, and states that this is a poorly explored topic.

I have no dispute with their aim. I note that the matter has been examined by others, including Goodman et al 2013 and also Le Grande 2012, who examined a broader group but report heart failure patients separately. The authors may wish to consider the relevance of these papers to their conclusions.

Major compulsory revisions

1. Overall, I found the introduction and the discussion lengthy and suggest they might be reduced considerably. The introduction went well beyond briefly justifying the study, and the discussion could focus more tightly on the significance of their findings, comparison with directly relevant other investigations and potential weaknesses of the study.

In contrast, there were aspects of the methods and results that require clarification.

2. The setting of the study (i.e. country) is not stated and there is no reference to ethics approval. I have presumed that the study was completed in Ireland.

3. The inclusion criteria were stated to be a primary diagnosis of heart failure as determined by the hospital clinical team, but this was followed by a parenthetic criterion of an ejection fraction >40% (line 177), as if this was perhaps the inclusion criterion. There is a typographical error in this, as I presume an ejection fraction of <40% was intended. However, it is possible to have heart failure as clinically defined with a greater ejection fraction (e.g. diastolic heart failure), so there is potential conflict between a clinical diagnosis and an ejection fraction defined diagnosis.

4. There is a further typographical error in defining cognitive impairment as scoring 8 or more on the Abbreviate (sic) Mental Test (line 178/9). I presume it should have been less than 8, on the Abbreviated Mental Test.

5. Table 2 reports the mean scores on the IPQ-R, which are scored in the range 0-5. However, the mean score for ‘Identity’ is given as 6.18. The range in this table is also given as a single number, rather than the more conventional upper
and lower figures. Again, the range for ‘Identity’ is given as 13. It would be important to provide assurance that this is an error only in the table, not in the study data analysis.

Discretionary revisions

6. The paper title refers to ‘psychological distress’, yet line 194 et seq ref to ‘mental health’. I acknowledge our professions’ tendency to Orwellian newspeak in referring to mental health services etc, but given the choice of a more appropriate term in the title of the paper, consistency would be desirable. The HADS is not a measure of mental well-being.

7. Lines 222 – 234 provide a description of the mean scores for subjects. I did not find this helpful, and would be more interested in the proportions with ‘high’ scores on these scales – i.e. those where intervention might be indicated.

8. Lines 238 – 242 state that 25% of the sample scored in the borderline or higher range of depression scores. Given the earlier statements about high levels of depression in this group, it would be desirable to state how many scored in the ‘significant case’ range. This would be helpful in evaluating the generalizability of the findings. Similarly some more detail of the NYHA scores would allow a better understanding of the spread of this functional measure of heart failure in this sample.

9. Line 260-262 notes that age and education were significant explanatory elements in the model. Table 1 indicates that only 28.7% of the sample completed second level education. I am surprised at this if the study was completed in Ireland and wonder if current standards of schooling were being applied to an older population when school leaving ages were lower. Depending on when school leaving ages were increased, this may differentially affect older participants compared to younger ones, confounding the age/education findings. I wonder if the direction of the correlation is worth discussion – my understand from Table 4 is that younger more educated subjects were more prone to depression?

10. In the results (tables 3 and 4) I note the substantial contribution of some elements of the IPQ-R scale to the models and the minimal contribution of other elements – this is rather discounted in the discussion, which argues for a holistic approach to IPQ. If one argues that illness perception is a broader, or at least different, construct to depression and anxiety, then an alternative approach to this might be considered.

Minor essential revisions

11. The referencing needs attention. “Keith J Petrie et al” appears in the body text (line 134, 136, 137); the reference at line 492 is incomplete; citations of journals are given inconsistently (eg abbreviated in line 550 and in full in line 53/554), etc.

12. Line 123 states “These components have recently” yet the supporting reference is 2002.
13. Line 134 should be affect, not effect.

14. Line 151 should be “and psychological symptom severity”, rather than “and symptom severity”.

15. Line 294 should include an apostrophe after “patients”.

16. Line 406 should be “principal” not “principle”

17. Line 410 should be “development of a” not “development a”

References


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.