Author's response to reviews

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

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Author's response to reviews: see over
Dear Prof. Murray,

Thank you very much for the time you and the reviewers have spent in reviewing our paper, and for the thoughtful, comprehensive and very helpful comments regarding our submission. In fact as a result of some of the comments received we identified some small errors in the analysis and have therefore re-run all the statistical analysis for the paper. This has subsequently been independently checked by two researchers and we are now confident that it is accurate and correct. We have tried to fully address each concern and hope that this revised manuscript is now acceptable. Each concern is discussed in detail below. The amendments made in light of the comments received outlined here are indicated in the text by tracked changes. Thank you for allowing us to resubmit our manuscript for your consideration.

We understand that you may wish to send the paper out for full review in light of these changes, but perhaps the original reviewers would be so kind as to review the revised numbers when they are reviewing the other amendments made.

Should you require any clarification on any aspect of the amendments made please do not hesitate to contact us.

Yours sincerely,

Dr. Karen Morgan
Response to Reviewers’ reports

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

Version: 2
Date: 8th August 2014

Reviewer 1: Pete Ellis

Reviewer’s report:

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.

Response: We thank the reviewer for this comment. The papers by Goodman and Le Grande have been considered and are now cited – page 5

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.

Response: We thank the reviewer for this comment. We have reduced the length of introduction considerably, page 3-5, and amended the discussion to be more focused, page 10-13. Additional details have been added to the methods and results as further discussed below.

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.

Response: We thank the reviewer for this comment. This study was completed in Ireland. We have added the country and confirmation of ethical approval on page 6.

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.
Response: Thank you for highlighting the typographical error, we have corrected the symbol in question and also clarified that these criteria were used simply for the purposes of this study – page 6.

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.

Response: Thank you for highlighting the typographical error; we have corrected the text to correctly reflect the scoring system of the Abbreviated Mental Test.

5. Table 2 reports the mean scores on the IPQ-R subscales, 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.

Response: We thank the reviewer for this comment. The identity subscale of the IPQ is calculated differently to the other subscales in the questionnaire, as noted in the methods section. The score for the ‘Identity’ sub-scale of the Illness Perception Questionnaire is the sum of answers for 14 questions related to symptoms, and so has a range of 0 – 14.

The single figure in the range column represented the difference between the lowest and highest scores achieved on each subscale. We have simplified the range column in Table 2 and now present the range in the more traditional way, minimum and maximum scores possible for each subscale, as suggested by the reviewer.

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.

Response: We thank the reviewer for this comment and have amended the text to achieve consistency of terminology.

- References to ‘mental health’ in relation to HADS have been amended to ‘anxiety and depression’ – page 7,8
- Also ‘relationship of illness perceptions to mental health in the context of HF’ changed to anxiety and depression - page12
- ‘Mental health’ changed to ‘mental well-being’ – page 13

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.

Response: We take on board the reviewer’s point and thank him for this comment and have replaced the % min and max scores in table 2 with information regarding the percentage of subjects...
scoring in the top 10% of each illness perception sub-scale in the text on page 8. We have noted that few interventions have been developed to change illness perceptions, none as yet in a HF population; this is an area of interest for future research (page 12).

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.

Response: We thank the reviewer for this comment. We have added information regarding the scores for caseness of anxiety and depression and a brief explanation of the NYHA classes (page 7) We have also detailed the percentage of subjects in the possible case and probable case categories of HADSA and HADSD and in each class of NYHA classification of HF. (Page 8)

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 understanding from Table 4 is that younger more educated subjects were more prone to depression?

Response: We thank the reviewer for this interesting comment. In this study we asked about completion of secondary school, not the specific age of leaving or completing school.

The study population are an older population of Irish adults. Among older adults in Ireland educational attainment varies significantly with age. It is not unusual for older adults not to have completed secondary level education. This is due to changes in the Irish education system in the late 1960’s, specifically the introduction of free second level schooling in 1968. In this older cohort education and socio-economic status are often not aligned– unlike in younger cohorts.

Table 4 shows a negative correlation between depression and age and education, as depression increased age decreased , and as depression increased education also decreased, showing more depression in the young less educated subjects.

This relationship between education and depression has been described in the literature (Ross and Mirowsky 2006; Song et al. 2012).

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.

Response: We thank the reviewer for this interesting comment.
Table 3 shows that emotional representation and personal control correlated significantly with anxiety, while only personal control correlated significantly with depression. Table 4 shows that none of these dimensions were independently significant in explaining either depression or anxiety. In the hierarchical regression analysis the addition of illness perceptions (Model III) explained 29.9% of the variance in depression scores, and 33.7% of the variance in anxiety.

Illness perceptions accounted for a large proportion of variance in both anxiety and depression, however it must be acknowledged that some play a stronger role than others; in this case emotional representations contributed the most of all the illness perception sub-scales. Interventions addressing emotional representations may be a worthwhile approach to treatment in this patient group.

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 (e.g. abbreviated in line 550 and in full in line 53/554), etc.

Referencing amended

12. Line 123 states “These components have recently” yet the supporting reference is 2002.

Removed ‘recently’ – page 4

13. Line 134 should be affect, not effect.

Corrected – page 5

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

Corrected – page 5

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

Changed to ‘a patient’s’ – page 9

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

Corrected – page 12

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

Corrected - page 13
Reviewer 2: Matthew Macfarlane

Reviewer's report:

Thank you for the opportunity to review this fascinating manuscript by Morgan et al, which used a validated questionnaire (the IPQ-R) to determine illness perceptions in a group of 95 patients with diagnosed heart failure, and correlated scores on different domains within the IPQ-R with scores on the Hospital Anxiety and Depression Scale. They found that heart failure patients did not apportion a high proportion of their symptoms to their illness, and that they felt well-informed about the illness. They had what seemed to be accurate perceptions of their illness’ chronicity and attendant impact on their lives, while feeling that it was controllable through their own actions and through treatment. The authors also found significant correlation between caseness for depression on the HADS and illness perceptions, particularly negative emotional representations. Similarly, they found a correlation between caseness for anxiety on the HADS and illness perception scores, but no significant correlation within the subscales.

I have a number of comments regarding methodology and interpretation of the results.

Major Compulsory Revisions

1. The main interpretation I feel needs to be discussed in the paper is that high scores on the ‘negative emotional representations’ section of the IPQ-R and high scores of depression and anxiety on the HADS may in fact be measuring the same thing, thus rendering the correlation trivial. For instance, on the IPQ-R, statements in the relevant section include ‘I get very depressed thinking about my illness’, ‘having this illness makes me feel anxious’, ‘my illness makes me feel afraid’, as well as other portions of the IPQ-R which reflect feelings of hopelessness common in clinical depression such as ‘there is nothing that can help my condition’. While these don’t correlate 1:1 with statements people are asked to endorse on the HADS, it would be unusual for a depression or anxiety disorder patient not to score highly on these aspects of the IPQ-R. In general experience, those people who have major medical illnesses of all types and comorbid depression have negative views about their illness, because they have generalised negative views about themselves, others and the world – Beck’s triad. Similarly, the correlation between anxiety and a feeling of lack of control/poor confidence in measures of coping/ameliorating the condition was described by Beck decades ago. I would appreciate more discussion on this point to justify what additional information this study adds to what is a fairly well-established set of CBT principles.

Response: We thank the reviewer for this valid comment and have discussed this potential limitation on page 11.

Essentially according to CSM anxiety and depression are outcomes –products of coping which depends on illness perceptions. Unfortunately in a cross-sectional study such as ours the relationship cannot be teased out, so we recommend a longitudinal study so pathways can be explored in detail. In additional illness perceptions are formed directly in response to an illness and influence coping skills. The HADS on the other hand is not specifically focused on a particular illness – it is a snapshot of previous 2 weeks. Further discussion of this area has been added to page 11.

2. Similarly, the suggested implications for further treatment seem unfocused. A suggestion of cognitive behavioural therapy is welcome, but I would like some clarity about whether the
suggestion is that CBT should be given whether only when diagnosable depression or anxiety is present (in which case it is already indicated regardless of illness perceptions, and illness perceptions will not doubt be addressed in the course of therapy if the therapist and client feel they are relevant), or to all patients with heart failure (a conclusion I’m not sure is borne out by the results here).

Response: We thank the reviewer for this comment and have further discussed the suggested treatment implications on page 13.

3. No mention is made of the male preponderance of the sample, which is not representative of heart failure in general: the latter has much closer to a 1:1 distribution (Ho et al. The epidemiology of heart failure: the Framingham Study. J Am Coll Cardiol. 1993; 22(4 Suppl A). This needs to be discussed and counted as a limitation. The rates of depression and anxiety seen in this sample, on the other hand, appear similar to other published work (e.g. Rutledge et al. Depression in heart failure a meta-analytic review of prevalence, intervention effects, and associations with clinical outcomes. J Am Coll Cardiol. 2006;48(8):1527).

Response: We thank the reviewer for this comment. The sample included in this study was a consecutive sample and was not nationally representative. This has been noted as a limitation of the study on page 13.

Minor Essential Revisions

4. Some more clarity on patient selection would be helpful, particularly in which country the three hospitals mentioned in the methods were based (I presume Ireland, but I note one author’s affiliation is from Malaysia, which may make the decision to limit the study to English language more relevant if one or more of the sites was in that country).

Response: We thank the reviewer for this comment. This study was completed in Ireland. We have added the country and confirmation of ethical approval on page 6.

5. When outpatient clinics are mentioned in methodology, I presume this means cardiac outpatient clinics – this should be specified, as many people with heart failure are managed in the general hospital and a cardiac clinic-only sample may be a little less generalizable (although still a worthwhile population to study in their own right).

Response: We thank the reviewer for this comment. The outpatient clinics were cardiac specific; this information has been added on page 6.

6. The inclusion criteria appear to be in error – heart failure is generally defined by EFs lower than 40%, rather than higher. Also, the Abbreviated Mental Test (not the Abbreviate Mental Test – a typo) is scored abnormal if the score is below 8, not above 8.

Response: Thank you for highlighting these typographical errors, we have corrected the symbol in question and also corrected the text to correctly reflect the scoring system of the Abbreviated Mental Test – page 6.
7. I note that the ‘treatment control’ subscale of the IPQ-R had poor internal reliability as per Table 2. Given that conclusions are drawn later in the paper about treatment recommendations around addressing feelings of control over treatment, this limitation should be mentioned.

Response: We thank the reviewer for this comment and have noted this limitation in the discussion – page 13

Generally, this paper covers an important topic, and the area of illness perceptions in general is an area which is exciting, based on the potential for short, focused interventions to improve physical and mental health outcomes in those with physical illness. However, I feel that this paper requires significant revision to accommodate the problems inherent in trying to measure a subset of negative beliefs in the context of people who, by definition, have a large and disabling set of negative beliefs leading to their diagnosis of an anxiety or mood disorder.

References
