Author's response to reviews

Title: A Descriptive Study of Older Adults with Persistent Pain: Use and Perceived Effectiveness of Pain Management Strategies [ISRCTN-CCT-NAPN-11230]

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To the Editors:

We appreciate the reviewers' comments on the manuscript, A Descriptive Study of Older Adults with Persistent Pain: Use and Perceived Effectiveness of Pain Management Strategies [ISRCTN-CCT-NAPN-11230]. Our point-by-point response follows. The revised manuscript and the Appendix are attached.

Reviewer: Robert D Helme

Major Compulsory Revisions

This study reports the use and perceived efficacy of pain management strategies in a sample of older people using a non-validated measure called by them a Pain Management Strategies Survey. They would have been wise to cast their literature review net wider so as to incorporate a validated measure already in the literature (Kung et al The Pain Clinic 12, 299, 2000). They could also have cited further papers by Kung, including perhaps Kung et al 2000 (The Pain Clinic 12,193, and The Journal of Pain, 1, 293, 2000).

The authors appreciate the suggested additional references. The Pain Clinic articles were not identified through the bibliographical databases that we typically use (e.g., PubMed, CINAHL) nor were they referenced in other articles that we reviewed in the preparation of this manuscript. Therefore, we are grateful that you have directed us to these articles. We now summarize findings from the Kung et al, 2000 article (The Pain Clinic, 12, 299-315) in the Background section (page 5) and also refer to it in the Discussion (page 16).

The instrument that we did use was based on information from the National Center for Complementary and Alternative Medicine http://nccam.nih.gov/health/whatiscam/, accessed September, 2003) and was adapted from the work of Warms, Turner, Marshall, and Cardenas (2002). This information is now added to the manuscript on p. 11. As indicated, we were not aware of the Kung et al measure, but wanted to capture use of many complementary therapies that were not included in that instrument.

The two other references are appreciated. However, after careful consideration, we decided not to cite them because we viewed them as not directly related to the aim of our paper, which focuses on use and perceived effectiveness of pain management strategies. However, the Kung et al. Journal of Pain article will be an important reference to cite in our planned subsequent papers that describe the intervention portion of our study.

Two other thoughts arise from reading the study: firstly, the most effective treatment of pain from degenerative disease in older people is joint replacement. This is not included by the authors yet is so effective it stands as a marker for comparing the effectiveness of other treatments.

The study inclusion criteria required participants to have pain intensity rated as greater than 2 on a 0-10 scale and pain that interfered with daily activities. Individuals who had effective relief of all pain from joint replacement would thus not have been eligible for the study. Furthermore, the exclusion criteria regarding surgery in the past 6 months or coming 6 months prevented the enrollment of older adults recovering from
or anticipating joint replacement surgery. We should also emphasize that our sample was heterogeneous with regard to causes of chronic pain; many participants had more than one problem and many had problems other than pain in joints that might have been amenable to joint replacement. The focus of our study was on how older adults with chronic pain self-manage their chronic pain; thus, we did not study surgical interventions such as joint replacement or other types of surgery (e.g., decompressive laminectomy for lumbar and radicular pain due to spinal stenosis, which is also effective in relieving pain).

Secondly, the first reference to religious thoughts and pain to my knowledge was by Corran in 1994 Corran et al. Proceedings of the 7th World Congress on Pain p895).

We carefully reviewed the Corran et al. (1994) article. The coping measure used was the Coping Strategies Questionnaire, which contains a scale labeled "Praying and Hoping." The article reported that adults aged 60 and over had higher scores on this scale than did adults aged 18-39. The items on this scale are: 'I know someday someone will be here to help me and it will go away for awhile,' 'I pray to God it won't last long,' 'I try to think years ahead, what everything will be like after I've gotten rid of the pain,' 'I have faith in doctors that someday there will be a cure for my pain,' 'I pray for the pain to stop,' and 'I rely on my faith in God.' Thus, we do not view this scale as a "pure" measure of use of prayer or other religious practices. The articles by Barry et al. and Dunn and Horgas were cited because they examined the use and perceived effectiveness of prayer and spiritual practice to manage pain in samples of older adults.

Reviewer: Ingalill Rahm R Hallberg

Major Compulsory Revisions:

In the aim it is said that age and gender should be investigated which is important in a sample of this kind. However, sample demographics (table 1) and pain locations (table 2) does not present data distribution in relation to gender and age. This is important since differences in this respect may confound the analyses of pain management strategies.

We have reorganized Table 1 and Table 2 to show the data for the three age categories. However, because so few men (n=37) participated in the study, presenting demographic and body pain location data by gender would create subgroups too small to be meaningful.

Also, in table 3, some errors in the presentation need to be corrected - for instance it is not clear what is presented in column 5, is it percent and if so percent of what - percent is said to be within brackets but figures within brackets is not presented?

We have revised Table 3 to more clearly indicate what is presented in the last column.

Also, in the result section, non significant data is presented, either as a trend or as significant, p-value 0.05. It should be accepted that these statistics do show non significant data - ie not twist the interpretation in the direction of thinking it is significant.

We have revised the manuscript to make it clear that P-values of 0.05 and above are not statistically significant (page 8 second paragraph and page 14 last paragraph).

Minor Essential Revisions:

Throughout in the result section terms like: most, a majority, some is used. This type of valuation of the data is not acceptable in a result section. It is more appropriate to use the exact figures.

We have revised the manuscript to include exact figures rather than these types of terms (page 12 last paragraph and page 13 first paragraph).

Some language errors; page 5 last paragraph first sentence, page 11, fifth row from the bottom of the last paragraph - something is missing

We have corrected these errors. Please see page 6, paragraph 1, and page 12 of the revised manuscript.

Discretionary Revisions

perhaps it would be interesting to know or address if the participants was aware of all the pain management strategies available?

We did not assess in this study whether participants were familiar with/aware of each pain management
strategy on the questionnaire. We agree that this would be of interest to assess in future research.

Also, perhaps it would be interesting to know if the 71 individuals admitted after the baseline assessment might impose any kind of bias to the results.

We agree that this is an important question and thus conducted additional analyses to assess this. We describe the analysis on page 12 in the statistical analysis section, report the results on page 13, and comment on the findings in the discussion on page 17.

Reviewer: C. Reid

Major compulsory revisions:

Page 11. I was not able to retrieve the Appendix, but I think it is important to let readers know how many of the reported strategies came from the other category. Providing evidence that your closed-ended questions captured the vast majority of responses (as opposed to the other category) is important. On a related note, what method(s) was used to categorize the responses you got from the 'other category? Were these responses folded into Table 3?

We have included the Appendix showing the PMSS items in the resubmission. In the text of the manuscript on page 13, we describe the responses from the other category and explain what responses were folded into Table 3.

Page 12 and Table 1. You do not include univariate statistics for two of your primary psychological variables (depression, self-efficacy). Please add information to the text (or Table 1) so that readers can discern the mean scores and distributions for these two variables.

We have added mean (SD) scores on the depression and self-efficacy measures in the results section (top of page 13).

Page 13. You identified bivariate associations between depressive symptoms and treatment helpfulness and for self-efficacy score and treatment helpfulness. Given that both depressive symptoms and self-efficacy are likely related to one another (and both psychological factors may also be related to pain intensity), why were multivariable analyses not performed?

We agree that depressive symptoms and self-efficacy are likely to be related to each other. We also agree that in building a model to predict treatment helpfulness, it would be important to carefully examine the associations of pain intensity, depression, and self-efficacy scores with each other and with treatment helpfulness, and to conduct multivariable analyses that consider these associations. However, we view our study as descriptive and exploratory. Therefore, we think it most appropriate to present in this manuscript only bivariate results, which could help guide future studies. However, we have added a sentence in the discussion to acknowledge this issue and suggest the use of multivariable analyses in future, hypothesis-testing studies (page 18).

Page 17. Your findings do NOT point to the need for further research to develop SAFE medications (given that you did not assess risks associated with analgesic medication use. I would simply delete the word 'safe' from the sentence. As a group the medications did not appear to be more effective than the other strategies listed in Table 3. Your results suggest the need for the development of more effective drug AND non-drug therapies.

We agree with the reviewer and have made the recommended revisions (page 16, paragraph 2).

Minor Essential Revisions

Page 2. Please include age ranges for young-old and mid-old and old-old in the abstract

The revised abstract includes this information (page 2).

Page 11. It is not clear why you selected reporting strategies for 25 or more participants only. Please provide a rationale in the methods section.

We now provide a rationale for this on page 11, last paragraph.
Table 3. I was not able to locate an Appendix to review. I would be interested in how you worded the question regarding exercise. It would be important to know that participants were asked whether they used exercise specifically as a means of managing their pain (as opposed to using exercise for various other health benefits) Presumably the Appendix makes this clear.

We have included the Appendix with this resubmission. It includes the exact wording of the question regarding exercise.

We appreciate the thoughtful critiques by the reviewers. We hope the revisions and responses satisfactorily address their concerns.

Sincerely,

Carol A. Kemp
Research Nurse Coordinator
Swedish Medical Center