

NURSING RESEARCH REVIEW FORM

1st Review

Manuscript # 2005/201
Manuscript title: Family caregiving in heart failure
Manuscript type: Regular
Number of text pgs.: 13
Number of figures: 1
Number of tables: 5
Reviewer: Mark Parshall
Stat reviewer assigned?: No -- do you recommend a stat reviewer? No

Please evaluate the following with these choices: (1) adequate, (2) inadequate (describe in written review) or (3) not appropriate (describe in written review)

Problem statement: 1
Attention to relevant literature: 1
Theoretical framework: 1 (with minor reservations covered in the review)
Research design: 1 (albeit with a small convenience sample that makes this more of a preliminary study than anything definitive)
Data analysis: 1 (basically adequate, but the way results are presented could be clearer and more concise—covered in the review)
Discussion of results: 1 (basically adequate, with a few weaknesses noted in the review)
Organization: 1 (basically adequate, with a few weaknesses noted in the review)
Writing style: 1

Please rate the following topics 1-5 (with 5 being the highest rating):

Value of topic: 4
Probable reader interest in topic: 3
Importance of present contribution to nursing: 3
Priority of topic for publication: 3
Rank this manuscript for its value: 3

Reviewer's Recommendation (please type "X" after your choice):

Accept without revisions
Accept with revisions X
Maybe accept with revisions
Do not accept

**Please provide a comprehensive and integrated review of this manuscript.
Be sure to present a balanced view of the manuscript's strengths and weaknesses.**

2005/201: Family caregiving in heart failure

This is a mostly well-written MS that deals with an important topic. There are some issues in the organization of the Methods section, with one of the measures, and in how some of the results section reads. I also have a few suggestions about the Discussion.

Problem, Background, & Conceptual Model. A clear statement that the study was exploratory (or not hypothesis-driven) would clarify that the use of the model was heuristic (i.e., as a guide to what constructs ought to be measured). The model seems overly complex (i.e., pretty much everything is related to everything else), an issue that might be addressed in the Discussion.

Design & Methods. The exploratory descriptive-correlational design is appropriate to the limited state of knowledge with respect to this population. In my opinion, the basic information on research design (currently under Procedure, p. 6, line 15) should precede the subsection on sample. What I think should be under Methods/Sample is information on how the sample was identified / recruited (currently under Procedure, p. 6, lines 16-20), and then just the size of the sample and basic demographics (age, sex, race). The more detailed characterization of the sample could be placed in a table or made the first subsection under Results. What that would leave in the Procedure section would be the statement on IRB approval (p. 6, line 16) and the last 3 sentences of that paragraph (lines 20-23, beginning with, "Patients were asked...."). It would be helpful under Procedure to state how many patients were approached, how many provided contact information, how many caregivers were invited to participate, and how many declined.

Measures. With respect to measures, on the CAS, Cronbach's α was only .75 which might be satisfactory for a 4-item scale, but this is much lower than reported in the 2 cited studies by Dracup and Moser (1995 [patients with MI or CABG, .89], 2000 [spouses of patients with acute cardiac events, .88]). That discrepancy deserves some comment in the discussion. I suspect that it may have something to do with the setting of caregiving for a chronic disease, rather than an acute event.

Results. I have three general concerns with the Results section: 1. lack of subheadings; 2. inadequate information on scale distributions combined with a tendency to distract from actual results with interpretive statements; and 3. wordiness and redundancy in describing in text what is presented in tables.

1. It would be very helpful if the authors included subheadings (e.g., *Descriptive statistics for study variables; Aim 1: relationships among study variables; Aim 2: caregiver burden and outcomes; Aim 3: perceived control.*)
2. With respect to the descriptive statistics for the scale scores (p. 9, lines 9-22) that paragraph is cluttered with statements that are vague and difficult for a reader to judge as *results* (e.g., "several of the caregivers did experience a moderate degree of difficulty" [line 12]; CAS scores "averaged a bit higher than the natural

midpoint of the scale” [lines 13-14], whereas BCOS scores averaged “lower than the natural midpoint of the total scale” [line 15]; score ranges were wide and “some” scores were “as low as” some rather arbitrary low score [2 instances, lines 18-22]). Meanwhile, what is really needed in the Results section is some clear indication about distributional characteristics of the scale scores in Table 1. One sample Kolmogorov-Smirnov tests could be used to test whether or not any given scale was normally distributed; alternatively (or in addition) reporting medians and interquartile ranges in addition to the means and SDs in the table would permit a reader to judge whether distributions were symmetrical or skewed (and the direction of any skew).

3. For the item-level statistics pertaining to Aims 2 and 3 (p. 10, line 9 through p. 11, line 12) there is a lot of text devoted to reiterating what is fairly clearly shown in Tables 3-5.

Discussion. Two arguments at the beginning of the discussion are either convoluted and miss the point.

1. The connection, if any, between the study by Dracup et al. (2004) (p. 11, 19-22) and the Schulz and Beach (1999) study (p. 11, lines 22-3 through p. 12, lines 1-4) is highly speculative. In the first place, what is stated as a kind of connecting premise (i.e., that the relationship between age and task difficulty needs further study because of what Schulz and Beach found) is tenuous at best. The fact that Schulz and Beach controlled for age is irrelevant. No association with mortality *could have* been found in the present study because it wasn't one of the aims and the design was cross-sectional. The fact that Schulz and Beach studied another population of caregivers altogether may or may not be relevant, but it is moot unless and until a longitudinal study is designed with caregivers of patients with HF with a specific aim of investigating the connection, if any between task difficulty and mortality in this population.
2. The authors miss what seems to be going on with the pattern of correlations in Table 2 between the 2 SF-36 scales, age, and the other study measures (p. 12, lines 5-12). First, the overall pattern is SF-36 General Health (GH) and Mental Health (MH) are modestly and positively correlated (they're supposed to be correlated; they are both important broad dimensions of HRQOL and come from the same instrument). Secondly, as is shown in Table 1, and as the authors note, MH was well-preserved in this sample (indeed consistent with norms for the general population as found in numerous SF-36 studies); also shown in Table 1, but unremarked on, the mean GH score was shows relatively minor impairment. The key point from Table 2 is that, aside from the expected correlation between GH and MH, the former correlates with nothing, whereas MH correlates with everything to a moderate-to-strong degree. What this means is that SF-36 MH is a far more responsive scale than GH in relation to age and the other constructs in the model in this sample. This may have some implications for the model itself (see below under Miscellaneous Comments).

Miscellaneous comments:

Regarding Tables 3 through 5:

- Those tables could be made clearer by placing the response categories and item scoring in a footnote to the tables (instead of “possible ranges”). That would save the reader the bother of having to refer back to the methods section and would also make the item-scores and table headings in the rightmost column for each of those tables easier to interpret at a glance.
- If those minor changes were made to the tables, then the text could be considerably simplified and less duplicative. For example, the last 3 paragraphs could be condensed to two, one for Aim 2, and one for Aim 3:

Under Methods/Instruments (p. 7, lines 7-10), it would be helpful to give the total number of items (17) in the modified OCBS here, rather than in the very last sentence of that paragraph (e.g., “...revised from one to three items [...], for a total of 17 items, to determine which types of behaviors...”).

Possible issue to consider in the Discussion with respect to the conceptual model:

One possible implication of the pattern of correlations shown in Table 2, if those findings were to be replicated in a larger and more heterogeneous sample of caregivers of patients with HF, is that the conceptual model could be considerably simplified (e.g., perhaps a double-headed arrow between GH and MH, but no other variables with direct paths to GH). Something along those lines could be a possible hypothesis for future study. Given the strong correlation between the BCOS and MH scores in Table 2, another issue for future study might be whether MH is a mediator or moderator of the relationship between task difficulty and caregiver outcomes (i.e., not a correlated outcome as modeled in the figure). If those suggestions are too speculative or specific, it could still be said that a possible implication of the correlational findings is that a simpler conceptual model might suffice, a possibility that could be tested in future studies.

There are several minor APA oversights in text (ordering of multiple citations on p. 3, use et al. on first citation in text if = 6 authors, p. 4 and p. 11) and on the reference list (e.g., comma before ampersand in two-author references, space after periods separating first and middle initials).