

Reviewer Comments:

Reviewer #1: Manuscript Review: NRES-D-07-00281

Overall, this is a solid manuscript with many strengths. Key among them are an important but understudied topic, a well-supported rationale for the study, solid use of a theoretical framework, and generally sound research methods. A few areas need clarification as noted below. What would make the manuscript really outstanding is a more in-depth discussion of the theoretical implications of the findings.

Problem Statement

The research problem is clearly stated and important. Relevant literature is presented in support of the research problem. However, the purpose statement on p. 4 does not fulfill the promise of either the manuscript title or the presentation of the theoretical framework, i.e. that this is an hypothesis-driven test of a theoretical model. The purpose statement sounds exploratory and understates the strength of the study. Simple clarification of the stated purpose could easily resolve this problem. (Note: The purpose is stated differently in the Abstract. The purpose statement in the Abstract is better.)

Literature Review

The literature review is brief, but adequately addresses the key accomplishments and gaps in prior research in this area.

A relatively minor issue is that the link between perceived self-efficacy (a cognitive construct) and performance of self-management strategies (a behavioral construct) is not explicated very clearly. Self-management or self-directed action is mentioned several times throughout the manuscript, but it is not included in the conceptual model or measured as a variable in this study. Most of the time this leads to a minor lack of clarity, but in one place it is misleading in that the results of the study are overstated: In the first paragraph of the Discussion, the authors note "This is one of the first studies of the cancer population to demonstrate the beneficial effect that PSE has on symptom management...." This is incorrect, as symptom management was not assessed. A brief statement that the link between self-efficacy and self-management behaviors is an untested assumption of the model would provide a needed distinction between cognition and behavior.

Theoretical Framework

The theoretical framework is an important strength of the study. The authors have integrated two well-known theories in a way that capitalizes on the strengths of both. This indeed accomplishes their aim of moving the science of symptom management forward. The hypotheses are clearly stated and Figure 1 is helpful. My only quibble with the model is (as noted above) that self-management behavior is absent without any

explanation. Even simply stating that assessing self-management behaviors was beyond the scope of this study would tie up this loose end.

Research Design

The authors have taken good advantage of two existing data sets, which provides a relatively large sample for model-testing. However, the adequacy of the sample size for testing this large model is not addressed. The measures are generally described well. The categories developed for the demographic and disease variables seem reasonable for the most part, although the rationale for the categories is not always clear. For example, it is not clear why type of cancer was coded as lung cancer and other cancer diagnoses. Likewise, why number of comorbidities (a continuous variable) was dichotomized is not clear. However, these are relatively minor points.

Clarification is needed regarding the "surgery during" variable. It appears that data on "surgery during" were collected later in the study, not at baseline. If this is the case, "surgery during" should not be conceptualized as a predictor of baseline CRF in the theoretical model.

Data Analysis

The authors have used a method for testing the model as a whole, rather than testing each individual hypothesis. This is fine, but one wonders why they didn't simply hypothesize that the data would fit the model as a whole. The updated approach to the Baron & Kenny method of assessing mediation is a strength of the data analysis. Readers steeped in classic Baron & Kenny (i.e. 3 regression equations) might appreciate a bit more information about the comparison of two nested models, as this is an innovative feature of the study. The procedures used to evaluate model fit seem appropriate and the explanations of what each means are excellent. Such "mini-tutorials" about complex analytic methods are helpful for seasoned researchers and students alike.

Despite the strengths of the data analysis, there are a few areas that need clarification. For example, the procedures for handling missing data are entirely unclear. Also the narrative about the study hypothesis appears to be inconsistent with what was stated earlier in the paper. It now appears that the model as a whole was hypothesized (p. 10, lines 17-22). Finally, it is not clear how Hypothesis 2 (a reciprocal relationship between CRF and other symptoms) was tested.

Discussion

The discussion section is the weakest of the manuscript in that it doesn't focus in any depth on the theoretical implications of study results. This is where some elaboration could make the manuscript really outstanding. What does the mediating function of self-efficacy mean in a conceptual sense? Baron & Kenny provide a good discussion of the conceptual meaning of mediation, but the present manuscript does not make use of it. The Discussion treats the hypotheses lightly and dwells on relationships found that were not

hypothesized. Especially with Hypothesis 3, a more in-depth discussion of the theoretical implications is needed.

One conclusion that can be drawn is that the data suggest a model that is more complex than the one hypothesized. Discussing this and the implications for nursing knowledge development would enhance the Discussion section.

Finally, the first sentence of the Conclusion section is misleading: it is PSE, not CRF that provides the mediating mechanism.

Organization & Style of Presentation: Polished.

Reviewer #2: The author begins by claiming a dearth of literature related to self-efficacy and cancer symptom management, and I am not convinced that is the case. Whereas there may be few studies that specifically considered the mediating role of self-efficacy between fatigue and physical functioning, there is clearly a very large theoretical and empirical literature on self-efficacy in related contexts that could be used to support the validity of the proposed theoretical model; that literature is even referenced to support the main hypothesis (e.g., p. 5, lines 20-23) and in the explanation of findings (p. 16, lines 2-3)

The very limited information provided about the two parent theories is not sufficient to support the proposed theoretical links in the model, particularly the rationale for how/why the various disease-related and demographic variables would influence cancer-related fatigue and how/why they would NOT influence the primary variable (perceived self-efficacy). In the absence of supporting literature, the author(s) nonetheless claim that "past research and theory" were used to evaluate the merit of demographic variables for inclusion in the statistical model (p. 13, line 8) and "theoretical considerations" guided adjustment of the model for fit (line 17).

As a potential target for intervention, PSE within the proposed model is only predicted by the severity of fatigue - which would suggest that the only way to increase PSE and therefore improve physical functioning is to decrease fatigue. In short, the theoretical support for the study is inadequate.

There needs to be extreme care taken in distinguishing between paths of indirect influence (e.g., CRF to PSE to PFS) and true mediation. I am not familiar with the constraints of the type of modeling performed here so it is not clear to me the extent to which mediation can be claimed.

The discussion of the findings goes beyond the relationships that are supported by the analyses. Overall, I do not feel that the links between the substantial literature on self-

management of cancer symptoms, the theoretical model, the analyses, and the discussion of findings were adequately presented. Making these links clearly and more convincingly would be necessary to supporting the validity of the findings.

Reviewer #3: This manuscript reports the results of testing a path model of the mediation of the effect of cancer-related fatigue on physical functioning through perceived self-efficacy for fatigue self-management. The topic is likely to be of interest to readers and the manuscript is generally well written, but several areas need revision, primarily to clarify the methods section and extend the discussion of the study's limitations.

Page 5 (line 19): The authors should make it clear at this point (as they do later in the manuscript) that fatigue is not included as one of the symptoms in the measure of average symptom severity.

Page 7 (line 10): Twenty-five patient characteristics were included in the hypothesized theoretical model, but only about half of these are described. On page 13 (line 10), it is stated that 16 were retained in the model after preliminary screening. It is important to specify which characteristics were evaluated and not retained. The full list of those considered at each step in the process might be made available as supplemental material on-line.

Page 8 (lines 21-22): It is unclear how identical distribution of observations on two measures would "ensure" that all data are similar and address the same effects, given that distributional characteristics alone would not guarantee that two variables were even correlated. Just state that the distributions were identical and let the Cronbach's alpha of .85 indicate the high degree of correlation between these two items.

Page 8 (line 23) and page 9 (line 8): Taking an average across items does not standardize the resulting score. Standardization incorporates a measure of variability in the transformation. In this case, taking the mean simply results in a composite score having the same range of possible values as each of its component items, often a useful characteristic, but not the same as standardization.

Page 10 (line 1): The number of nurse experts who evaluated the content validity of the self-efficacy measure should be specified, and the process they used described.

Page 10 (lines 11-15): Specify the software that was used to impute missing values (the citation given is a book). Also give more information about the percentage of missing values for individual variables. Overall, only 2.9% of the observations were missing, but the statement that 50% missing values per variable was used as a cutoff for imputation suggests that some of the variables may have had unusually high rates of missingness.

Page 11 (line 2): It is stated that the estimation method was chosen because of appropriateness for nonnormal data, but it is not clear whether nonnormality was an issue

in the dataset. If it was, descriptive statistics related to normality (e.g., skewness, kurtosis, and perhaps the median) should be added to the variable descriptives in Table 1.

Page 11 (lines 10-12): The definition given of the root mean square error of approximation (RMSEA) - the average absolute discrepancy between observed and predicted covariances- actually is the definition of the root mean square residual (RMR). The RMSEA incorporates a penalty for model complexity.

Page 11 (lines 16-18): The Comparative Fit Index is defined here as the amount of covariation in the data that can be reproduced by a given model. A preferable definition is that it indicates the proportional reduction in the chi-square (or the fit function) achieved by a particular model compared to a null model in which the variables are specified to be uncorrelated. "Amount of covariation reproduced by a model" might lead a reader to think this is comparable to the proportion of explained variance in a regression model, which would clearly be misleading, given CFI's typically high values.

Page 13 (lines 7-8): I assume that multiple regression was used to screen categorical predictors having more than two levels by testing a set of dummy coded variables. If any of these predictors were initially included in the path model (all those in the final model were dichotomous), the authors need to specify how they were incorporated into the path model for estimation.

Page 13 (lines 13, 20): Include the upper bound as well as the lower bound for the confidence interval of RMSEA.

Page 14 (lines 9-10): In presenting the results of the test of mediation, clarify that the constrained model (where the direct path is set to zero) corresponds to a model representing total mediation.

Page 17: There are three major issues that need to be addressed as study limitations. The first is possible bias in coefficients due to measurement error in the predictors. The authors might address their reasons for not implementing a MIMIC model, which could have incorporated estimates of measurement error. A second set of issues relate to the cross-sectional nature of the dataset, which is mentioned without elaboration. Discussion should include potential theoretical implications of measuring self-efficacy and physical functioning concurrently, such as ambiguity of directionality. Although current self-efficacy logically would seem to influence future physical functioning, an argument could be made that current physical functioning might influence current self-efficacy. A recent article by Maxwell and Cole (*Psychological Methods*, 2007, volume 12, 23-44) offers other reasons why cross-sectional data may yield biased estimates of mediation processes. The third issue that needs to be addressed is the exploratory nature of this analysis given the substantial degree of model trimming that occurred (the degrees of freedom were reduced from 50 to 12 between initial and final model). The need for replication should be emphasized.