



## Reviewer 1

### Problem Statement:

1. **Reviewer Summary:** "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 (Original Manuscript) does not fulfill the promise of either the manuscript title or the presentation of the theoretical framework, i.e., that this is a 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)." **Response:** Clarified the purpose statement in the manuscript to match the statement in the abstract (see p.3).

### Literature Review

1. **Reviewer Summary:** "The literature review is brief, but adequately addresses the key accomplishments and gaps in prior research in this area."
  - A) "A relatively minor issue is that the link between PSE (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." **Response:** Clarified the link between PSE (a cognitive construct) and symptom self-management (a behavioral construct) in the Research Hypotheses Related to the Theoretical Model (see p 4-5). The discussions of the results have been changed. Revised Figure 1, the Hypothesized Theoretical Model, to reflect that symptom self-management behaviors is an untested assumption of the theoretical framework and assessing symptom self-management behaviors was beyond the scope of this study.
  - B) "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 addressed. 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." **Response:** Revised the sentence in the first paragraph of the Discussion section (see p. 16). Clarified the link between PSE for fatigue self-management and symptom self-management behaviors is an untested assumption of the model (see p. 4-5), and revised Figure 1 (see Figure 1).

### Theoretical Framework

1. **Reviewer Summary:** "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." **Response:** Clarified the link between PSE for fatigue self-

management and symptom self-management behaviors in the Research Hypotheses Related to the Theoretical Model section (see p. 4-5). Revised Figure 1, the Hypothesized Theoretical Model, to reflect that symptom self-management behaviors is an untested assumption of the theoretical framework and assessing symptom self-management behaviors was beyond the scope of this study

#### Research Design:

1. **Reviewer Summary:** “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.”  
**Response:** Revised the manuscript to indicate the adequacy of the sample size to test the model (see p. 12-13).
2. **Reviewer Summary:** “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 (continuous variable) was dichotomized is not clear. However, these are relatively minor points.” **Response:** Revised to give rationale with supporting evidence from the literature why the stated physiological and contextual patient characteristics were included in the hypothesized theoretical model (see p. 6-8). Revised to clarify why the type of cancer was coded as lung cancer and other cancer diagnoses (see p. 6). Revised to clarify that the variable co-morbid conditions was measured and placed in the path model as a continuous variable, and later dichotomized to calculate the odds ratio and 95% confidence interval (see p. 13).
3. **Reviewer Summary:** “Clarification is needed regarding the “surgery during” variable. It appears that data on “surgery during” were collected later in the study and not at baseline. If this is the case, “surgery during” should not be conceptualized as a predictor of baseline CRF in the theoretical model.” **Response:** The “surgery during” variable was conceptualized as a predictor to account for those that had surgery during chemotherapy at baseline and those that had a condition warranting future surgery at baseline. Clarified that the timing of surgery in relation to the baseline interview is not made specific in the data set in the Limitations Section (see p. 20).

#### Data Analysis:

1. **Reviewer Summary:** “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.”  
**Response:** The model was formulated from the synthesis of the TOUS and Bandura’s Self-Efficacy Theory integrating various relationships within the two models. The measurement model parallels the theoretical framework and this theoretical framework guided the study. It is the strength/weakness of these relationships that would provide the strength/weakness of the overall model. We wanted to emphasize these relationships so we kept them separate versus simply hypothesizing that the data would fit the model as a whole. If we only focused on the overall model then we would not put enough emphasis on CRF. In order to increase the strength of the overall model, we needed to test the individual hypothesized relationships

within the model and eliminate those relationships that reduced the strength of the overall model and keep those that increased the strength of the overall model. Clarified the strategies used to test the model in the Data Analysis section (see p.10-12).

2. **Reviewer Summary:** “The updated approach to the Baron and Kenny method of assessing mediation is strength of the data analysis. Readers steeped in classic Baron and Kenny (i.e., 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 the 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.”

**Response:** Revised manuscript includes further explanation in evaluation of model fit (see p. 12).

3. **Reviewer Summary:** “Despite the strengths of the data analysis, there are a few areas that need clarification.”

A) “For example, the procedures for handling missing data are entirely unclear.” **Response:** Clarified the software used and procedure implemented for missing values analyses and imputation. More description given in regards to the amount and pattern of missing values (see p. 10).

B) “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 (from the Original Submission, P. 10, lines 17-22). **Response:** The model was formulated from the synthesis of the TOUS and Bandura’s Self-Efficacy Theory integrating various relationships within the two models. The measurement model parallels the theoretical framework and this theoretical framework guided the study. It is the strength/weakness of these relationships that would provide the strength/weakness of the overall model. We wanted to emphasize these relationships so we kept them separate versus simply hypothesizing that the data would fit the model as a whole. If we only focused on the overall model then we would not put enough emphasis on CRF. In order to increase the strength of the overall model, we needed to test the individual hypothesized relationships within the model and eliminate those relationships that reduced the strength of the overall model and keep those that increased the strength of the overall model. Clarified the strategies used to test the model in the Data Analysis section (see p.10-13).

C) “Finally, it is not clear how Hypothesis 2 (a reciprocal relationship between CRF and other symptoms) was tested.” **Response:** Hypothesis 2 was worded to make clear what strategies were used in testing this relationship (see p 15.).

#### **Discussion:**

1. **Reviewer Summary:** “The discussion section is the weakest of the manuscript in that it doesn’t focus in any depth on the theoretical implications of the 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 and 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.” **Response:** The Discussion Section was revised to give a more in depth theoretical analysis of the findings for each of the hypotheses with implications given for practice and research. Particularly targeted was the focus hypothesis of the study, Hypothesis 3, investigating whether PSE for fatigue self-management mediates the relationship between CRF severity and PFS. Baron and Kenny’s conceptual definition of mediation was used to explain why PSE for fatigue self-management is important to achieving outcomes such as PFS. For clarity, a separate Discussion section was provided on further findings so as not to detract from the discussion of the main hypotheses of the study. See p. 16-20.

2. **Reviewer Summary:** “Finally the first sentence of the conclusion section is misleading: it is PSE, not CRF that provides the mediating mechanism.” **Response:** Revised this sentence for accuracy (see p. 20).

#### Reviewer 2

1. **Reviewer Summary:** “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 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., Original Manuscript, p. 5, lines 20-23) and in the explanation of findings (Original Manuscript, p. 16, lines 2-3).” **Response:** Referenced work from other populations included in the revised manuscript (See p. 4-5).
2. **Reviewer Summary:** “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 consideration” guided adjustment of the model for fit (line 17).” **Response:** Revised to provide the rationale for the physiological and contextual patient characteristics (disease-related and demographic variables) that have been hypothesized to influence CRF severity for Hypothesis 2. (See p. 6-8.) Expanded the description of the theories that we are drawing from.
3. **Reviewer Summary:** “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.” **Response:** The final model depicts the various relationships between CRF severity, PSE for fatigue self-management, and PFS. Based upon the theoretical framework, knowing these relationships (i.e., direct and indirect paths ultimately influencing PFS) provides insight into potential interventions that may lead to improved PFS for persons with cancer. For example, knowing that PSE for fatigue management functions as a mediator between CRF and PFS in persons with cancer provides

the foundation for nurses together in research and clinical practice to design efficacy enhancing interventions to optimize PFS.

4. **Reviewer Summary:** "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." **Response:** All direct and indirect paths are identified in the *Testing of the Fit of the Hypothesized Theoretical Model* section of the paper. Revised manuscript includes further explanation in evaluation of model fit (see p. 10-16).
5. **Reviewer Summary:** "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 findings were adequately presented. Making these links clearly and more convincingly would be necessary to supporting the validity of the findings." **Response:** Revisions were made to provide the rationale behind the physiological and contextual patient characteristics that were hypothesized to impact CRF severity, the analyses section was enhanced to provide greater clarity, and the discussions section was modified to provide greater detail regarding the significance and applicability of the finding to research and practice.

### Reviewer 3 .

1. **Reviewer Summary:** From the Original Submission, 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." **Response:** Indicated for clarity that fatigue is not included as one of the symptoms in the measure of average symptom severity in the location as recommended (see, p. 4).
2. **Reviewer Summary:** From the Original Submission, Page 7, Line 10. "Twenty-five patient characteristics were included in the hypothesized theoretical model, but only half of these are described." From the Original Submission, 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." **Response:** The number of patient characteristics, some composed of more than one variable, was clarified in the manuscript (see p. 6-8). Specified which patient characteristics were eliminated in the model after preliminary screening. Likewise, specified which patient characteristics that had some but not all variables eliminated in the model after preliminary screening (see p. 14).
3. **Reviewer Summary:** From the Original Submission, 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." **Response:** Restated the explanation about the identical distributions as recommend by Reviewer 3 (see p. 8).

4. **Reviewer Summary:** From the Original Submission, 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."  
**Response:** Revised both areas to reflect a composite score rather than a standardized score (see p. 8-9).
5. **Reviewer Summary:** From the Original Submission, 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."  
**Response:** Specified the number of experts who evaluated the content validity of the self-efficacy measure and the process used to develop the tool (see p.9-10).
6. **Reviewer Summary:** From the Original Submission, 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."  
**Response:** Clarified the software used for missing values analyses and imputation. More description given in regards to the amount and pattern of missing values (see p. 10).
7. **Reviewer Summary:** From the Original Submission, 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 nonnormality (e.g., skewness, kurtosis, and perhaps the median) should be added to the variable descriptives Table 1."  
**Response:** Reported the measure used to evaluate the symmetry and peakedness of the distribution, Fisher's Measure of Skewness and Kurtosis. Skewness and Kurtosis statistic reported in the variables descriptives table, Table 1 (see p. 10 referring to Table1).
8. **Reviewer Summary:** From the Original Submission, 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."  
**Response:** Revised the description of the RMSEA as recommended (see p. 11).
9. **Reviewer Summary:** From the Original Submission, 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."  
**Response:** Revised the definition of the comparative fit index as recommended (see p. 12).

10. **Reviewer Summary:** From the Original Submission, 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."  
**Response:** Revised to give more description to the two variables that were dummy coded, surgery prior and surgery during. Indicated that the dummy coded variables were included in the regression models and path models after dummy coding (see p. 7).
11. **Reviewer Summary:** From the Original Submission, Page 13, Lines 13, 20. "Include the upper bound as well as the lower bound for the confidence interval of RMSEA."  
**Response:** Added the upper bound confidence interval of the RMSEA (see p.14-15).
12. **Reviewer Summary:** From the Original Submission, 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."  
**Response:** Clarified that the constrained model corresponds to a model of total mediation (see p.16).
13. **Reviewer Summary:** From the Original Submission, Page 17. "There are three major issues that need to be addressed as study limitations."
  - A) "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." **Response:** Muthen (1989) recommends the use of MIMIC models when group membership is hypothesized to moderate the relationship between a predictor and an outcome variable. In our study, this type of hypothesis was not specified. Therefore, the otherwise useful MIMIC models were not used. (Muthen, B. (1989). Latent variable modeling in heterogenous populations. *Psychometrika*, 54, 557-585.)
  - B) "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, an argument could be made that current physical functioning might influence current self-efficacy. A recent article by Maxwell and Cole (*Psychological Methods*, 2007, vol. 12, 23-44) offers other reasons why cross-sectional data may yield biased estimates of mediation processes." **Response:** The model does not prove directionality. The model allows for testing of hypotheses that conforms with assumptions of directionality which supports the idea of causation. Added in the Limitation Section that the "cross-sectional design of the study precludes determination of causation among the variables examined," (see p. 20).
  - C) "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." **Response:** The goal of the study was not to prove the model right or wrong but rather to find predictors that explain a substantial portion of the findings. Added in the Limitation Section that the "nature of this analysis was exploratory and needs replication through a prospective design, (see p. 20)."