

Nov. 04, 2006

RE: ms # 2006/075 - The Mediating and Moderating Roles of the Cognitive Triad on Adolescent Suicidal Ideation

Dear Dr. Handfinger

Thank you for providing important comments on my manuscript, "The Mediating and Moderating Roles of the Cognitive Triad on Adolescent Suicidal Ideation," which I am resubmitting for publication in *Nursing research*.

In response to the Reviewers' comments and suggestions, I have revised my manuscript as follows.

Reviewer comments – ms # 2006/075

Reviewer #1 –

I appreciate the opportunity to review this interesting paper. The topic is of great importance, as evidenced by objective #18-2, formulated by the United States Department of Health and Human Services, aimed at reduction of adolescent suicide attempts (*Healthy People 2010*). The manuscript is generally well-written and clear, except for awkward phrasing in a few places. The sample size is quite adequate, and the theoretical basis is sound.

Thank you for your support. In addition to revise this paper, an English editor has also edited the manuscript again. Please also give me any comments on awkward sentences. I will try to revise again as best as I can.

My primary concern with the manuscript is the choice of a normal, non-suicidal population to study suicidal ideation. Logically, it seems that researchers should seek to study suicidal ideation in persons who have it. Scores on depressive symptoms and suicidal ideation are quite low in this sample of Taiwanese adolescents. For example, scores on the depression instrument can be as high as 54; the mean score of this sample is only 13.92. Scores on suicidal ideation can be as high as 70 (if my math is correct), yet the average score of the sample is 28. There is no mention in the manuscript that the sample is basically a non-depressed, non-suicidal sample. Without information on the full range of scores, it is not possible to ascertain the degree of skewness in the data, but skewness is extremely problematic for correlation and regression work (I refer the authors to Tabachnick and Fidell for a great discussion of this matter).

Thank you for these important comments. Since suicidal ideation is an important indicator for further suicidal attempts, identification of adolescents with suicidal ideation may be important for preventing the occurrence of suicidal attempts. Many adolescents with suicidal attempts or completed suicide have not been identified about their suicidal ideation. Therefore, we wish to study suicidal ideation in a non-depressed, non-suicidal sample in order to attain primary prevention. A sentence “The sample is basically a non-depressed and non-suicidal sample” has been added in page 10. The full range of scores on suicidal ideation has also been added in pages 13 through 14. The skewness values have been added in the Table 1. To deal with skewness problem, Spearman’s rank correlation was used to examine the interrelationships among the study variables. Data were transformed and used for further regression analysis (p14).

I have a couple of other observations. The statement that “adolescents who appear to have suicidal ideation have not attracted attention of mental health professionals” is untrue. There has been a tremendous amount of attention to this problem in professional journals, at least in the United States and Japan (I do not know about Taiwan). That brings me to my next observation: The literature review needs to be updated. Except for a few 2003 references (and one 2004), recent literature has not been included. Given that this paper, if published, would have a 2007 publication date, readers would expect 2005 and 2006 references. Finally, a very minor matter: Confidentiality and anonymity are two different things. Either data are collected completely anonymously or the researchers should speak in terms of keeping the data confidential.

Thank you again for your expert reminding. The statement that “adolescents who appear to have suicidal ideation have not attracted attention of mental health professionals” has been removed from the manuscript due to cultural differences. Papers published in 2005 and 2006 have been added in the text and references list. Sentences regarding confidentiality and anonymity in page 11 have been revised.

Thank you so much again for support and encouragement. Your expert comments on my manuscript extend my perspectives on this research issues. Nursing Research journal is the top nursing journal worldwide. It will be my great honor if my manuscript can be published in Nursing Research.

Reviewer #2 –

The purpose of this study was to test the mediating and moderating effects of the cognitive triad on the relationship between depressive symptoms and suicidal ideation in a sample of school-aged adolescents. In general this is a well-organized, well-written manuscript. The following are a few suggestions for minor revisions and one major concern that requires attention.

Thank you for your support.

The only suggested change for the abstract is to define the cognitive triad. The problem statement, attention to relevant literature, and theoretical framework are coherently presented building a case for the need for this study. In figure 1a, the interaction between depressive symptoms and cognitive triad needs to be shown more clearly.

I have added a sentence to define the cognitive triad in the abstract. The order of the figures 1a and 1b has been changed in order to conform to that described in the manuscript. In both figures, different types of variables have been more clearly labeled.

Non-experimental can be omitted from the description of the research design. My major concern is a lack of power analysis and justification of the large sample size. While larger sample sizes are generally desired, the sample sizes can be so large that small clinically insignificant changes become statistically significant.

The term “non-experimental” has been omitted from the description of the research design (p9) and abstract. The power was set to .95 in order to justify the large sample size and then used to perform power analysis. Even though a power analysis was conducted to justify the large sample size, the sample was still far beyond the recommended sample size 119. The above statement has been added in page 10.

The instruments are appropriate and presented well. The only question is whether the factor analysis mentioned in line 2 p. 13 was exploratory or confirmatory. In the results section, a change of wording is suggested in lines 8-10 on p. 15 “when the cognitive triad was included in the equation, 30% of the variance was accounted for ($R^2 = .30$) and there was a reduction in the relationship between depressive symptoms and suicidal ideation...”

Thank you for your reminding. The factor analysis is a confirmatory factor analysis. A statement has been added in p14. Also a suggested wording in the results section has been changed in p17.

The references are comprehensive and appear to be up-to-date. Two references—Anderson, 2002 and Kann et al., 2000 are cited in the text and do not appear in the reference

list. Conversely, Andrews and Lewinsohn, 1992 and Zauszniewski, Panitrat, and Youngblut, 1999 appear in the reference list but are not cited in the text. The figures are necessary for those readers who like to peruse the data.

In order to update literature, literature cited Anderson (2002) has been removed. Reference--Kann et al. (2002) has been added in the reference list. Two references -- Andrews and Lewinsohn, 1992 and Zauszniewski, Panitrat, and Youngblut, 1999 have been removed from the reference list.

Both figures 1a and 1b have been revised with coefficients included (p25).

This manuscript has the potential to add to nursing science provided my major concern can be adequately addressed.

Thank you so much again for support and encouragement. Your expert comments on my manuscript extend my perspectives on this research issues. Nursing Research journal is the top nursing journal worldwide. It will be my great honor if my manuscript can be published in Nursing Research.

Reviewer #3 – (stat review)

This paper is very well written and organized. It covers an important topic of interest to Nursing Research readers. The analysis follows the classic Baron and Kenny process for addressing mediation and moderation with a few small issues that need to be adjusted. However, the analysis has a very serious problem.

Serious problem.

Standard regression techniques appear to have been used. These assume simple random sampling. However, cluster sampling within two levels (schools and then classes within schools) was used in collecting the data. Cluster sampling introduces the possibility of correlations within class and/or school clusters that need to be accounted for. Otherwise, the reported significance levels and conclusions are suspect.

Thank you for this important comment. A statement related to cluster sampling “For those schools in which it was not convenient to randomly select classes, classes were selected by teachers, and therefore this type of convenience sample was used for the study” has been added in p10. Also the runs test was used to test for random sampling. The non-parametric runs test showed that the p values were all greater than .05 for three study variables, which indicated that the data were randomly ordered. The above statement has been added in page 14.

Other small issues.

1. Figures 1a and 1b are in the opposite order to that described in the paper.

Sorry for confusion. The order of figures 1a and 1b has been changed (p25).

2. On page 7, it first says no studies have examined these mediating effects, then none have studied these moderating effects. Then on page 8, it says few studies have examined either these mediating or moderating effects. Which is it? none or few?

Sorry again for confusion. The sentence has been revised in page 9.

3. In the first line of page 14, say in what sense the relationship must be "less" rather than just that it must be.

A statement to explain why the relationship must be "less" rather than just that it must be has been added in p15.

4. On page 15, line 9, the reduction is described as "significant". That term requires that a hypothesis test has been conducted. Describe the test that was used in the Methods Section and report a p-value here.

A hypothesis test (Sobel test) has been added in page 15 and a p-value has been reported in p17 and Table 1 (p. 25).

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I believe the above revision improve the relevance of my paper to NR's international nursing paper and would greatly appreciate your consideration of this manuscript for publication in *Nursing Research*. I look forward to hearing from you.

Sincerely,
Hsiu-Ju Chang, Ph.D., R.N.
College of Nursing
Taipei Medical University
250 Wu-Hsing Street
Taipei, Taiwan, R.O.C.