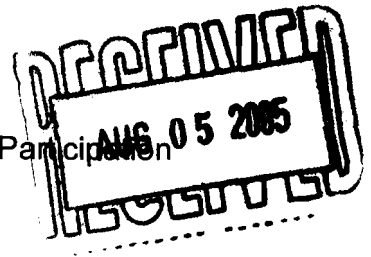


Rev 1

NURSING RESEARCH REVIEW FORM

1st Review



Manuscript #: 2005/136
 Manuscript title: Girls on the Move Program to Increase Physical Activity Participation
 Manuscript type: Regular
 Number of text pgs: 16
 Number of figures: 1
 Number of tables: 4
 Reviewer: Sandra P. Thomas
 Stat reviewer assigned?: No -- do you recommend a stat reviewer?

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: |
 Attention to relevant literature: |
 Theoretical framework: |
 Research design: |
 Data analysis: *see comment*
 Discussion of results: |
 Organization: |
 Writing style: |

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

Value of topic: 5
 Probable reader interest in topic: 4
 Importance of present contribution to nursing: 4
 Priority of topic for publication: 4
 Rank this manuscript for its value: 4

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

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

Comments for Editor only:

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

Raw

Review of Manuscript 2005/136

The researchers have addressed a substantive national health problem: the decline in physical activity among adolescent girls. They designed an intervention, "Girls on the Move," which incorporated individual tailoring. It is laudable that nurse counseling was a component of this intervention, although 10 minutes seems unduly brief and the nurse practitioners received limited training for this counseling function.

Strengths of the manuscript:

- (1) The intervention was theoretically based.
- (2) The manuscript was well written

I do have some concerns about the manuscript. The intervention produced no increase in the girls' physical activity (i.e., at the end of 12 weeks, the intervention group did not differ from the control group in level of physical activity). However, the researchers divided the sample into subgroups for further analyses. Given that the sample size is relatively small (N=77), I question the further division of the sample into even smaller subgroups for further manipulations involving parametric statistics. For example, the analyses reported in Table 4 are based on a subsample of 45 girls, and judging from the reported degrees of freedom, there was even some missing data in this small group of 45. The analyses of small subgroups gives the appearance that the researchers were scrambling to find something of statistical significance to report. On page 16, the researchers themselves spoke of the small sample size and acknowledged inadequate power. I feel it would be "cleaner" and more honest to simply say that physical activity among the intervention group did not differ from the control group, and stop there.

When examining Table 4 closely (the comparison of the "high change" and "low change" subsamples of the intervention group), changes in mean scores that are statistically significant are extremely small. Is it clinically meaningful that the mean score on Benefits of the high change group is 3.82, compared to mean score of 3.59 in the low change group? Likewise, the means for Self-Efficacy (3.06 and 3.48) and the means for Enjoyment (3.91 and 4.35) are also very close. The manuscript does not contain information about the possible range of scores on each instrument, nor about the range of scores in this particular sample. Therefore, the reader is left with questions: Are mean scores around 3 or 4 on the above-named instruments consistent with previous samples of adolescents? Is a mean score on Self-Efficacy of 3.48 a good score? What is the highest possible score?

Because the mean, SD, and range of scores was not reported for all instruments, the reader also wonders: Were the scores on all variables normally distributed? Was there any skewness in the data? If so, how was skewness handled?

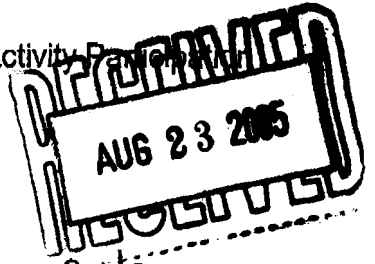
I assume that the Perceived Benefits Questionnaire, the Perceived Barriers Questionnaire, and the PA Self-Efficacy Questionnaire were derived from Garcia et al., although this is not explicitly stated in the section on Instrumentation. This information should be added, and it would be useful to readers to learn if this sample scored similarly to Garcia's 1995 sample. Was Garcia's sample also recruited from a lower socioeconomic area?

Rev 2

NURSING RESEARCH REVIEW FORM

1st Review - regular review

Manuscript #: 2005/136
 Manuscript title: Girls on the Move Program to Increase Physical Activity
 Manuscript type: Regular
 Number of text pgs: 16
 Number of figures: 1
 Number of tables: 4
 Reviewer: Anne E. Norris
 Stat reviewer assigned?: Yes No If not, 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) (missing)
 Attention to relevant literature: (2)
 Theoretical framework: (2)
 Research design: (2)
 Data analysis: (3)
 Discussion of results: (2)
 Organization: (3)
 Writing style: (2)

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

Value of topic: 5
 Probable reader interest in topic: 5
 Importance of present contribution to nursing: as written, 1
 Priority of topic for publication: as written, 1
 Rank this manuscript for its value: as written, 1

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

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

Comments for Editor only:

Important topic, much grist for intervention research mill but would need major revision to justify publication in NR
 Please provide a comprehensive and integrated review of this manuscript.
 Be sure to present a balanced view of the manuscript's strengths and weaknesses.
(see attached)

August 23, 2005

Molly C. Dougherty, PhD, RN
Editor, *Nursing Research*
CB#7460 School of Nursing
The University of North Carolina
Chapel Hill, NC 27599-7460

Re: Girls on the Move Program to increase physical activity participation

Dear Dr. Dougherty,

Thank you for the opportunity to review MS #2005/136. I regret the lateness of this review. I have been traveling and unable to use my office due to building renovations.

The manuscript addresses an important topic (intervening to increase physical activity in middle school girls). However, there are significant problems with the writing (e.g., no statement of purpose, no hypotheses given for an empirical study), design (e.g., underpowered, possible Hawthorne or assessment effect) and data analysis (e.g., subtracted pre-test from post-test and then used a parametric statistic to analyze this difference). The manuscript could be a significant contribution if it was re-worked to take more of a "lessons learned" approach in which the focus is how to use this experience and these data to guide subsequent research on the intervention. Ideally, the manuscript could incorporate demonstrating how to test for effects of threats to validity. I have tried to give the author(s) some guidance as to how they might do this. I would encourage them to submit this as a new manuscript for a new round of revision if you think this type of paper would be of interest to *Nursing Research*. Depending on the quality of thought devoted to the revision, I think the manuscript could be an important paper, but it would be too much for a "Methods Corner" piece unless the author(s) focused solely on testing for the effects of threat(s) to validity.

Sincerely,

Anne E. Norris, PhD, APRN, BC, FAAN
Associate Professor
norrisa@bc.edu
617-552-6810

Rev 2

REVIEW OF MANUSCRIPT #2005/136:

Girls on the Move Program to increase physical activity participation

This paper has some significant strengths. The topic is important. The intervention examined in the study makes sense and could be adopted by APRNs working with middle school aged girls if its effectiveness is demonstrated. It is not unduly complicated or expensive. The instrumentation used in the study also appears very good.

Unfortunately, there are problems with the writing, design, and data analysis that significantly impair the manuscript's ability to contribute to the State of the Science in its present form. I will review each of these problems, but would like to state at the outset that I would encourage the authors to consider revising the manuscript so that it focuses on the lessons they have learned in this study, and communicates to readers how the results of this pilot or first study will be used to guide subsequent research regarding the intervention. How do these results justify further study of this intervention? The manuscript could also be a wonderful illustration of what to do in intervention research when the research design is flawed due to the harsh realities of attempting to conduct intervention research under "real life" conditions. For example, the manuscript could make a significant contribution if the author(s) chose to identify threats to internal validity that occurred in their study and then demonstrated how they used their data to test for the presence or absence of effects related to these threats. Note that interventions evaluated under real life conditions may have greater feasibility for ready adoption and typically produce effect sizes that are more representative of what will occur in "real life" practice settings. Thus, I would argue there is much gold to be mined in a study such as the one described in this manuscript.

A. Writing

The organization and content of the writing could be improved in the following ways:

1. The connection between the theoretical framework, intervention, and intervention outcomes needs to be made clear in the Literature Review section. To do this, more information needs to be provided about the theoretical framework: Both the HPM and the TTM are noted but the HPM model is not described in sufficient detail. A figure is provided but it does not reflect integration of the two theoretical approaches, nor identify which of the constructs depicted will be impacted by the intervention and whether this impact varies for different intervention components. For example, will counseling only have an impact on participants' commitment to action or the relationship between this commitment and health promoting behavior?
2. Literature on contingency contracting (i.e., patients contracting with a nurse regarding implementation of a particular health behavior) is highly relevant to this study, but is not reviewed in the Literature Review.
3. The term "anticipatory guidance" is used on page 3 (line 16), but never discussed again. Are the NPs providing anticipatory guidance to participants in the intervention condition? If so, how does this aspect of the intervention relate to the theoretical framework and to the outcomes?
4. Tailoring (or not tailoring) the intervention to the developmental level of participants needs to be discussed.
5. A statement of purpose and hypotheses or research questions need to be provided in the first part of the paper (i.e., before methods are presented). It would be helpful here to

- emphasize the goal or effect of nurse counseling in contrast to the computerized component, if these effects can be disentangled.
6. The first paragraph in the Procedure section (page 7, lines 11-21) needs to be moved to the beginning of the Instrumentation section. The author(s) also need to explain why PA enjoyment items were not modified (see page 7, line 18, statement in parentheses).
 7. The sentence on page 8, lines 1-2, regarding exclusion criteria for participants needs to be moved to the Participants section.
 8. The paragraph on page 8, lines 4-7, should be integrated into the first paragraph of the Methods section where the study design is described.
 9. The procedure section needs to be revised and expanded so that it contains separate, subheaded paragraphs describing: (a) the school wellness center (WC) (staffing, location of laptop, place where interaction with NP occurred, privacy provided for these activities); (b) NP characteristics (age, gender, ethnicity, years at current position, years of experience, FNP vs. PNP, etc.); (c) NP training (note how much was didactic and how much was role play; how was proficiency with counseling sessions determined?; incorporate content on page 9, lines 18-21).
 10. Specify on page 12 (line 8) what is meant by a researcher being available to "assist" with reporting number of minutes of PA. What did the researcher do? For how many people? Assistance triggered by high reports only or provided to all participants?
 11. On page 15 (line 6-7), the author(s) note that the aim of the study was to test the efficacy of their intervention, but the study design is not adequate for testing efficacy – it is underpowered (page 16, lines 7-11).
 12. On page 17 (lines 7-8), the author(s) recommend tailoring information in the intervention as a means for increasing it's efficacy. This is a good idea, but the author(s) needs to say more – tailoring what information, for whom??
 13. The author(s) identify limited training as a potential reason for study outcomes (see paragraph beginning on pages 17, line 14). However, no mention is made of how the APRNs felt about the training. Also as noted earlier, the reader is not provided enough information about the training and the APRNs to evaluate this potential explanation for study findings. Moreover, inadequate training does not explain the pattern of findings (e.g., some participants in both the control and intervention condition improved and some did not). Thus, the evidence for "any training problem" is quite weak.
 14. The author(s) recommend use of objective measures of PA for future research (page 18, lines 3-9). However, they need to provide examples of these measures.

B. Design

Issues with the design need to be addressed and explored:

1. How is it that there were members of the control group that had significant changes in PA self-efficacy (noted on page 15, lines 2-4). Were these individuals from a particular school? Did they all see the same APRN? Does this finding argue for a Hawthorne effect, an assessment effect, contamination of the control condition? Why or why not? How can study data be used to argue for or against these potential threats to internal validity?
2. How is it that members of the intervention group did not change? Is there a correspondence between not changing in either the intervention or control condition and

participants reporting that “they sometimes, often, or very often did not have anyone to do PAs with them and/or did not have a good place to do PA,” and/or did not have “the right equipment, clothes or shoes” (see page 16, lines 14-17).

3. Was the intervention delivered as intended? What efforts were used (or not used) to track delivery of the intervention.
4. Could the researcher being available to “assist” with reporting number of minutes of PA (page 12, line 8) have contributed to the pattern of results obtained? Why or why not? What could be done in future research to maximize the validity of self reported minutes of PA?Was there anything else about the data collection procedure that could have contributed to the findings?
5. Is an effect size of .15 (page 16, line 10) clinically meaningful given the measurement challenges and the outcomes targeted? If not, what could be done to boost the effect of the intervention so that it produces a larger effect size in future studies?

C. Data Analysis

The good news/bad news for the author(s) is that I am not sure the results of the data analysis are trustworthy. I have two major concerns.

1. Repeated measures ANOVA and ANCOVA were used but it is not clear whether the variances in the dependent variables were homogeneous across the groups. This information needs to be provided or an alternative statistic needs to be used (e.g., mixed model analysis – consult a biostatistician for assistance with).
2. Continuous “difference scores” were created by subtracting week 1 PA values from week 12, but it is my understanding that this procedure produces invalid results when parametric statistics are subsequently used because this procedure ignores the variance associated with scores at each time point. Also, it seems confusing to do this in the same study where a repeated measures approach to data analysis is used because the repeated measures approach compares the two time points *without* ignoring the variance.

It is possible that the authors did not treat a “difference score” as a continuous variable that they then analyzed with correlation and t-test statistics. However the writing is unclear and the text on pages 13 – 14 (beginning on line 22 of page 13) suggests that the author(s) did do exactly this.

The text also suggests that the author(s) created high and low change PA categories by comparing the baseline and 12 week score. It is possible to obtain valid results with parametric statistics by creating a categorical variable that reflects how many units of change a particular individual demonstrated (or that the change was high vs. low) and then using it as a grouping variable in a t-test or ANOVA.

The analysis of change scores (i.e., “difference scores”) is an area of debate in the literature. If the author(s) did subtract week 1 scores from week 2, and disagree with my assessment of their data analysis, they should provide citations that specifically justify the validity of results produced with their approach.