Reviewer's report

Title: Evaluation of effectiveness of class-based nutrition intervention on changes in soft drink and milk consumption among young adults.

Version: 2 Date: 10 July 2009

Reviewer: Steve Hertzler

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In general, the authors did well regarding their responses to the comments of the reviewers. However, it is important to note that nearly every reviewer pointed out the issue regarding the use of estimated marginal means. In this reviewer’s opinion, there are still serious concerns regarding the use of this technique. For example, when examining the total soda beverage intake data in Table 1 in the manuscript, it is noted that there were no significant differences for either the males (n=9, P = 0.093) or females (n=70, P = 0.100). Yet, using the marginal means technique, all of a sudden a significant difference shows up (P = 0.033). Given that the P-value for the 70 females in the study was 0.100, it is difficult to see how the data from 9 males, that was not statistically significant by itself, was powerful enough to render the overall data now statistically significant.

Another example is total milk intake. It shows up as being a statistically significant increase in the females but, when the marginal means technique is used, now the overall difference is not statistically significant even when the number of females greatly exceeds the number of males. These types of findings look more like statistical anomalies or playing games with numbers than a difference that has a true meaning regarding soft drink or milk intake. The undeniable fact of this study is that females were greatly overrepresented relative to males in this study. In fact, one could argue that the inclusion of data from just 9 males really contributes little to this study at all. The number of males is too small to draw any conclusions about their eating habits anyway. So, why include them and bring up this contentious issue about weighted or marginal means and sample bias? Perhaps the best solution is to simply omit the data from males, or wait on publishing this study until the authors have been able to sample enough males so that some sort of reasonable conclusion can be drawn. If the authors choose to publish the article as it stands right now, it might be best to put the focus on the changes that happened in the female group and state that the number of males sampled was too small to do meaningful statistical analysis or draw relevant conclusions. For example, what does even the statistically significant increase in low fat milk intake among the males really mean when it comes from a sample of just 9 males? It would be foolish to attempt to generalize that data to the male college student population as a whole. It is risky enough to do it even with the small number of females sampled, let alone the much smaller number of males. This is a pretty significant statistical issue and would need to be seriously reconsidered by the authors in order for this manuscript to be suitable for publication.
In addition, I think that the authors perhaps misunderstood the question regarding the statement about the nutrition class being a cost effective way to use manpower and resources to effect behavior change among the collegiate population. Cost-effective compared with what? For the authors to be able to support such a statement, they would have needed to present data showing what the cost of this type of approach is and then compare it with another approach with regard to cost-effectiveness. The authors did not do this type of analysis and it was not a goal of this study. Therefore, the authors should delete this sentence unless they have a way of supporting the contention that this classroom approach is somehow more cost-effective than other approaches.