Author’s response to reviews

Title: Health State Preferences Associated With Weight Status in Children and Adolescents

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Version: 2 Date: 12 January 2011

Author’s response to reviews: see over
January 12, 2011

Natalie Pafitis MSc
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Dear Ms. Pafitis,

My coauthors and I were pleased to receive your letter regarding our manuscript (MS: 9794268554503581) titled “Health State Preferences Associated With Weight Status in Children and Adolescents,” which is under consideration for publication as an original article in BMC Pediatrics.

We thank you for the helpful suggestions from the reviewers and for the opportunity to revise the manuscript accordingly. Attached is a detailed list of our responses and the revised manuscript. In addition, as requested in your letter, we added information to the manuscript regarding informed consent and ethics committee review (page 4, 2nd paragraph).

We look forward to your final decision regarding publication of our paper.

Sincerely,

Mandy Brown Belfort, MD MPH
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Reviewer 1

(1) . . . you also discuss the Apple Study. It seems that the baseline HUI for participants in that study would be the first reported health utilities in obese children. You need to discuss your findings in relation to their baseline findings.

The APPLE study was an obesity prevention intervention in a sample of school children, the majority of whom were not overweight or obese at baseline. We clarified this point on page 11, paragraph 3. The APPLE study reported overall health utilities in their sample before and after the intervention. In contrast, we reported health utilities separately for overweight/obese and normal weight children. Thus, the health utilities in the 2 studies are not directly comparable.

(2) See also, deBeer and Gemke chapter, Health-Related Quality of Life in Obese Children and Adolescents in Handbook of Disease Burdens and Quality of Life Measures, 2010

The deBeer and Gemke chapter lists several studies reporting health status or health related quality of life in overweight and obese children and adolescents, and we cite some of those studies in our introduction (references 5-7). Our study is unique in that we report health utilities, which reflect the value that an individual or society places on the health status. We added some text to the Introduction (page 3, 2nd paragraph) to clarify the difference between health status and health utilities.

(3) Page 4. In the Health Status and Utilities Section, remove second “provides” from sentence starting “An advantage of the HUI over many other instruments . . . ”

We made the suggested correction.

(4) Page 4. Use of Health Utilities Index with children . . . it is validated for use in children as young as 8. Provide more detail about this measure and its use in children. And, explain why you did not use it with children 5-7 years old?

The Health Utilities Index (HUI) can be used for children as young as 5 years of age and we used it for all participants in our study. The HUI questionnaire can be given in 2 ways: (1) directly to the participant, for children 8 years and older; and (2) to a proxy (e.g. parent) who answers the questions on behalf of the participant, for children 5 years and older. Proxy respondents are used commonly when participants are not able to answer questions reliably, for example due to young age or disability. We revised p. 6, 1st paragraph to clarify this point.

(5) Page 9, not sure comparison of utilities in children and adults should be compared. Is there some support in the literature for comparing adult to children utilities?

In response to this question, we added the following text to the discussion (page 10, 2nd paragraph): “Domains of health may have different relative weights for children as compared with adults and result in a different level of overall quality of life for the same health state in patients of different ages. Since there has been little data on health utilities in pediatric health states to suggest where differences may be greatest, more study of health utilities in pediatric health states is needed.”
(1) The word "provides" is repeated twice on page 4.

We made the suggested correction.

(2) I am not an expert in health utilities, but I suspect many readers may not be either. Having read the paper several times, I still do not fully grasp the meaning of health utilities. Essential to the paper's impact is ensuring that the readership is able to understand the premise of the paper. I would suggest that the authors expand the explanation of health utilities so that a more general readership can more fully understand the meaning of these findings. It would be helpful to do this both in the introduction and conclusions. If this were a health services research journal, this would not be necessary. However, for the likely more general readership of BMC Pediatrics, I believe this is needed.

To explain the concept of health utilities and differentiate health utilities from health status, we revised the Introduction (page 3, 2nd paragraph), and added some additional text in the Methods section under “Health status and utilities” (pages 4 and 5), and in the Discussion section (page 10, 2nd paragraph).

(3) The authors do not mention a major limitation, which is that they recruited the sample from a primary care clinic and an obesity clinic at a tertiary care center. I cannot find anywhere in the paper where they report what proportion of the obese and overweight children were drawn from the obesity clinic, and what proportion from the primary care clinic. One would surmise that families attending an obesity clinic are much more concerned and, simply put, 'unhappy' about the child's obesity status, and therefore, if I'm understanding health utilities correctly, would certainly have lower health utilities. At minimum, the authors need to clarify repeatedly in the title, abstract, and throughout the paper that they are comparing non-overweight children with obese children seeking care in a tertiary care obesity clinic. My concern here may be rooted, however, in my limited understanding of health utilities (which then perhaps speaks to the need to better explain it for the readership).

We agree with the reviewer that a limitation of our study is the use of a tertiary care center-based sample of patients, and we now highlight this point in the discussion section (page 12, 2nd paragraph). We also specified the source of our participants in the last paragraph of the introduction (page 4, 1st paragraph), and used the term “convenience sample” throughout the manuscript.

The majority of the overweight/obese participants (38/42=90%) were recruited from the primary care clinic rather than the obesity clinic, and we have clarified this point on page 4 under “Study design and participants.” Given the small number of overweight/obese participants from the obesity clinic, it is unlikely that the type of clinic from which participants were recruited impacts our findings substantially.

(4) Combining overweight and obese children into one group seems problematic, as for this outcome, one would suspect that the real findings would be driven by obese children. This speaks again to sample size considerations. This deserves at least a comment (which I could not find in the manuscript as presently written).

We added a comment about limitations of our study related to sample size in the discussion section (page 12, 2nd paragraph).