Author’s response to reviews

Title: Sleep Assessment in a Population-Based Study of Chronic Fatigue Syndrome

Authors:

Elizabeth R Unger (eru0@cdc.gov)
Dr Rosane Nisenbaum (ran7@cdc.gov)
Harvey Moldofsky (h.moldofsky@utoronto.ca)
Angela Cesta (a.cesta@utoronto.ca)
Christopher Sammut (c.sammut@utoronto.ca)
Michelle Reyes (myr9@cdc.gov)
Dr William C Reeves (wcr1@cdc.gov)

Version: 2 Date: 7 Jan 2004

REFEREE #1
#1 Q-1 I would have found it interesting to have more of an analysis of the gender-based differences

The information is not available from this population.
#1 Q-2 Is SAQ available and what is the cost involved?
All three referees questioned specifics of the SAQ. It is copyrighted. Reference 26 gives the website.
We have added an additional sentence in Discussion noting this.

REFEREE #2
#2 Q-1 It would be useful to know whether there was an association between sleep problems and
presenting to a health-care practitioner, if this information is available.
The information is not available from this population.
#2 Q-1 The authors found an association between the presence of reported sleep problems and the
"wellness" score, but not fatigue itself. It would be interesting and clinically useful to know if there
was a significant association between sleep problems and cognitive symptoms or dysfunction. At
least one study has suggested that the cognitive symptoms of CFS are related to sleep dysfunction.
We did not do cognitive testing in this study. Virtually all of those with CFS in this population
self-report difficulty with memory and concentration so statistical comparison would not be useful in
this instance.
#2 Q-2. Table 5 could be included in the text.
We believe it better in a table.
#2 Q-3. It would really help the readers to see the SAQ as an appendix, although I note it has a
copyright for reproduction.
The SAQ is copyrighted. Reference 26 is a link to the group that owns the copyright.
#2 Q-4. Although the odds ratio is a legitimate statistic to use, these data produce very large ORs,
because of relatively small prevalence rates of not having sleep problems. For instance in table 2,
the OR (95% CI) of any sleep problem in Medical and psychiatric exclusions is 32.6 (12.9, 82.7). In
contrast, the relative risk (95% CI) based on the same figures is 3.2 (2.0, 5.1). Use of the ORs make
the risk seem larger than is reasonable. The use of RRs would be easier to interpret.
Our statistician believes OR are more appropriate.
#2 Q-5. Page 4: The Epworth scale is an interval scale, not continuous.
Fixed.
#2 Q-6. Page 10: "normed" is not a verb in my dictionary!
I cannot argue with one who speaks the Queen's English, so we changed the wording.
#2 Q-7. Page 11: Since the groups are not matched, let alone pair-matched, any comparison has to
be a group one, not "pair-wise".
This was a language problem and has been fixed in Methods.
#2 Q-8. Table 1: How are the 41 non-fatigued individuals included in the 339 sample of fatigued. These 41 individuals must be carefully chosen and described, since they act as the comparison threshold group for all other comparisons. We believe this was appropriately explained in Methods.

#2 Q-9. Table 2: Please put EDS in full: excess daytime sleepiness. Done.

#2 Q-10. General points about tables: If there is space raw figures for prevalence is always valuable. We believe the tables are optimal as they are and did not revise.

#2 Q-11. The Sleep Assessment Questionnaire (SAQ) has been copyrighted. This study has shown that this measure may be clinically useful for assessment. How will clinicians and researchers get access to the SAQ? Do they have a web-site? As noted above reference 26 gives the website. We have added an additional sentence in Discussion noting this.

REFEREE #3

#3 Q-1 to avoid confusion about the number of factors, I would indicate "five of the six factors" or indicate why sleep schedules were not tested. We Changed the text as suggested.

#3 Q-2 I would not put on the same level polysomnograms and MSLT (p. 6 L.4 and ss). Although MSLT have been demonstrated to be useful - and they may be in the present case - they have not been very well correlated with other sleepiness measures so far and they are far less used than PSG in formal sleep studies. We took out the text on MSLT.

#3 Q-3 Although it is not formally wrong, the sentence on L.1, P. 7 may be understood as this study being the first to accurately describe CFS and could be rewritten. We rewrote the text.

#3 Q-4 Sleep questionnaires: p.9 It would be important to stress that the SAQ was not factor-analyzed based on the same population than the one studied here. Done.

#3 Q-5 P. 10 L. 6: I would split the sentence in two, to keep the same structure for all scores. I couldn't figure out what this comment meant.

#3 Q-6 The Wellness and the Fatigue scales are introduced in the statistical section, while they belong to the Methods and are referred to in the Abstract only in the Results part. They should either be removed completely from the text or more clearly supported. This has been rectified and a sentence added to the abstract.

#3 Q-7 It would be useful to stress in the Methods section that the 1994 CDC criteria exclude some but not all psychiatric conditions. As space is not limited here, perhaps an option would be to reproduce the selection criteria as an appendix, for the reader who is not familiar with them. For the same reasons, it would be interesting to have as appendix the items of the SAQ if they are not protected by copyright. We rewrote sections of the Methods to explain exclusionary psychiatric conditions in more detail. The SAQ is copyrighted (as we note in text).

#3 Q-8 Stat section: you should precise distribution data and explain why and where you use a combination of parametric and non-parametric analyses (for KW mostly). We believe this was adequately explained in the Methods section.