Reviewer’s report

Title: Translation and validation of Berlin Questionnaire in primary health care in Greece.

Version: 1 Date: 26 October 2012

Reviewer: Nick Antic

Reviewer’s report:

The authors have looked at the utility of a Greek version of the BQ in diagnosis of OSA. Under diagnosis of OSA remains an important issue. The manuscript is well thought out and well written.

Major Compulsory Revisions

There needs to be some review of the conclusions in the abstract. I'm not sure I agree that this version of the BQ is a valid and reliable instrument for identifying those with OSA. As a screening questionnaire you seek a low false negative rate and at an AHI > 5 the sensitivity is only 76%. The original Netzer paper had a sensitivity of 86% Specificity 77%. I’d like to see discussion around how this BQ might be used and a little more measured commentary. At all 3 cut points the sensitivity and specificity has limitations, neither ruling in or out OSA with confidence. What about the 24% false negatives at an AHI > 5? Do they matter? Is that level of sensitivity adequate? Can the authors explain what the sensitivity and specificity does not change very much at AHI > 5, 15, 30. In the Netzer manuscript Specificity increased as the AHI threshold increased and sensitivity fell. Why has that not occurred here?

Can the authors please refine the methods around how they scored sleep and specifically respiration. Given the AHI is the gold standard measurement it must be absolutely clear how it was scored as different scoring techniques change the AHI substantively and likely explain some of the differences above. The reference they quote for scoring respiratory events is wrong, R&K was not for respi events, (Ref 14) and furthermore whilst they say the used AASM 2007 criteria they don't define if they used Alternative or recommended criteria. Indeed the scoring system they've used for hypopneas seems to combine Recommended and Alternate criteria. Furthermore these scoring systems are based on AASM consensus, not ATS (page 7 line line 5) describe. This needs tightening and revision

Can the authors define how they recruited people in Primary Care. Were they induced for example but being told it was a snoring and OSA study, and does that explain the high OSA prevalence? I think the most likely explanation for the high prevalence is the relative sensitivity of the scoring system making AHI > 5 not really the right cut point for OSA diagnosis. Ruehland Sleep 2009 describes the impact of different scoring systems on AHI very well and should be quoted.
Minor Revisions

diurnal somnolence is not assessed by the ESS, rather excessive daytime sleepiness (top of page 6)

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I am a CI on a RCT comparing CVS risk reduction between a control group and CPAP therapy in a high CVS risk cohort (SAVE study) funded in part by an untied research grant from Philips Respironics ($US 5 Million)

I have received honorariums and lecture fees from Resmed valued at approximately $10000