Reviewer's report

Title: Serum Thyroid-Stimulating Hormone Levels are not Associated with Exercise Capacity and Lung Function Parameters in two Population-Based Studies

Version: 2 Date: 2 April 2014

Reviewer: Stephanie MacNeill

Reviewer's report:

The authors have submitted an interesting study of the potential impact of thyroid dysfunction on cardiorespiratory health and exercise capacity. Compared to other studies in this field they have used a large data sample from the general population. By studying such a population they can make inferences to the wider population. Crucially, however, they have few patients who would be classified as having hypo/hyperthyroidism and as such direct comparisons with published studies of select populations become difficult.

The research question is interesting and well defined. The data are sound and well controlled.

I propose the following revisions:

MAJOR

1. The researchers have not described any of the demographic characteristics of patients with high/low TSH. This would be helpful to put subsequent results into context. While the regression models adjusted for many characteristics, details of the resulting regression coefficients are not presented.

2. The authors rightfully identify in the Discussion that their analyses are based on a population based sample with few subjects with clinically relevant hyperthyroidism. Have the authors performed a sample size calculation to determine how many patients would be required to identify meaningful changes in spirometry when classifying TSH by <=0.3, 0.3-3 and >3? The authors make reference to low power in the Discussion, but do not provide figures.

MINOR

1. Please indicate whether the lack of consistency in how the blood samples were taken (non-fasting in SHIP and fasting in SHIP-TREND-0) might have influenced the interpretation of TSH levels.

2. The participation rate for CPET and spirometry is low. It would be helpful to have some details of how participants differed from non-participants.

3. The authors used median regression for their analyses. While certainly an acceptable method of analysis it would be helpful if the authors provided some insight into their reasoning for this choice.
4. Table 1 provides details of the age distribution of participants in both studies. By presenting only the median and 25th/75th percentiles – and not explicitly stating the study eligibility criteria in the methods – the reader does not have a full sense of the age distribution. Please quote the minimum and maximum as well as interquartile statistics.

5. It would be helpful to report what proportion of the cohort were currently taking treatments for hypo/hyperthyroidism and incorporate this into the analysis.

6. The presentation of results in table 2 is unclear. It would be helpful to more obviously separate out the different models for the different measures of TSH.

7. In the discussion the authors state that their data “provides evidence that serum TSH levels are not associated with spirometry and exercise capacity”. It would be prudent to rephrase this as not having found evidence of an association.

DISCRETIONARY

1. The authors define all of the abbreviations used in the manuscript in the methods section. It would, however, be helpful if these are used consistently through the paper and tables. For example, in the text they make reference to VO2@AT while this is written out long-hand in the tables. Such terms should be consistently reported.

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

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.