**Reviewer's report**

**Title:** Serum homocysteine levels are decreased in levothyroxine-treated women with autoimmune thyroiditis.

**Version:** 2  
**Date:** 27 December 2013

**Reviewer:** Lucy Ann Behan

**Reviewer’s report:**

Owecki et al examined the effect of thyroid autoimmunity on homocysteine levels in premenopausal women who were euthyroid either with levothyroxine treatment or without LT4 treatment but all with positive anti TPO antibodies and compared results to healthy matched controls. They found lower levels of homocysteine in antibody positive LT4 treated women compared to healthy controls. This is a well written paper, however I have a number of comments:

**Major:**

Methods and Results sections:
What is the benefit of the ROC curve in this situation? Are the authors proposing that Hcy levels can be used to distinguish between Hashimoto subjects and controls? I do not think this statistic adds anything to this paper.

Results and Discussion sections:
The treated-hypothyroid (HT) TPO positive group had higher FT4 compared to the non-treated group, but the same as controls, while homocysteine levels were lower in the treated-HT group compared to controls but not compared to the untreated-HT counterparts. There was no difference in Hcy levels in the non-treated group compared to controls. The significance of these findings remains unclear by the end of the manuscript. The authors do not postulate a reason why the Hcy is reduced in the treated versus controls but not versus the other autoimmune positive group, which suggests that the treatment may be more likely to be the factor rather than the autoimmunity. Could the authors please discuss this further? The medians of Hcy levels are very similar, it is suprising that the differences were significant.

If Hcy is a marker of atherosclerosis/cardiovascular risk then this premenopausal group of women is a low risk group at the outset. Do the authors think this has an impact on their findings?

Discussion:

Paragraph 3 lines 170-173 – the authors mentions studies referenced 20-22 have been performed and that the conclusions of those studies are not comparable, however they have not stated what these studies examined or what their conclusions were – could the authors please elaborate slightly on these studies?
The authors reference a study by Topaloglu et al which examines Hcy in context of thyroid autoimmunity in euthyroid premenopausal females with Hashimoto thyroiditis recently and state that CIMT was the only value that was higher in the study group. Please provide more detail regarding this study, was CIMT higher in the patients v controls or in the <2.5TSH v >2.5TSH?

Minor:
Typographical error in Subjects and methods line 94 and 95 “tabel”
Typographical error in Table 1, column controls, row BMI – missing a closing bracket “)”

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