Reviewer's report

Title: Exercise Stress Echocardiography in the treadmill - upright evaluation During and after Exercise. Clinical Applications

Version: 1 Date: 25 April 2013

Reviewer: Albert Varga

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This is an interesting review article written by Cotrim and coworkers about the usefulness of exercise echocardiography in upright position in various pathologies. The Authors used mostly their own data to bolster their conclusions. The final conclusion of the Authors was that treadmill exercise stress echocardiography, performed in upright position could be a valuable tool for the diagnosis of several different pathologies.

I have the following comments and questions:

1. My first and major concern is the tone of the whole article. The conclusions of the Authors are based mostly on studies performed by the same group, sometimes with limited patient numbers. Therefore, I suggest the Authors to tone down somewhat the assertiveness of the phraseology and instead of declarative sentences (for instances, page 11, line 12: “… suggest that this methodology should be applied to the athletes that have symptoms…”; or page 16, last sentence, etc), a bit more cautious interpretation of the data is needed.

2. The paper is too long and sometimes difficult to follow (especially the part about pulmonary hypertension). It would be good to reduce the description of the studies already published.

3. The explanations of the findings are sometimes very speculative (especially the part about the pathophysiology of the appearance of the intraventricular or outflow tract gradients).

4. I very appreciate the efforts made by the Authors in the development of a new stress echo protocol, but to include it in a daily practice a multicentric validation is needed. It would be good also to know how difficult the protocol is, how long it takes to obtain a complete exam, how long the learning curve is. Please, provide a chapter dealing with this problems and with the limitations of the methodology.

5. It would be good to conduct head to head studies to prove the superiority of the new method in different pathologies.

6. I was not convinced why treadmill stress echocardiography performs better than supine (or semi-supine) bicycle echo in patients with ischemic heart disease. The conclusion of the paper by Peteiro (ref 22) was: “Peak treadmill EE provides significant incremental information over post-EE for predicting outcome in patients with known or suspected CAD”.

7. The chapter about aortic stenosis. On page 16, line 8, the Authors “disagree”
with the current recommendations... I suggest the Authors to delete this statement and rephrase the whole part in a more cautious way.

8. The Authors proposed a new test for the evaluation of patients with symptomatic aortic stenosis, which is not in accordance with the current guidelines. Again, I suggest the Authors to be more cautious with the recommendations, since the experiences obtained are based on a limited patient population and case reports.

9. I cannot accept the term: false symptomatic aortic stenosis.

10. The part about prosthetic valves is based on a case report and on an unpublished paper, therefore it should be excluded from the present manuscript.

11. The same problem with the congenital heart disease section.

12. The Authors suggest, that in patients with LBBB dobutamine echocardiography should be used instead of exercise stress echo. As a reference an abstract was given. Please, give an explanation and a stronger proof.

13. There are many grammatical and typographical errors throughout the manuscript.

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

Quality of written English: Not suitable for publication unless extensively edited

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