Reviewer's report

Title: Echocardiographic implications of pulmonary acceleration time and estimated vascular resistance for evaluating possible pulmonary hypertension.

Version: 1 Date: 4 February 2013

Reviewer: Luna Gargani

Reviewer's report:

Major compulsory revision.

This is an interesting paper about the usefulness of the acceleration time to predict increase pulmonary pressures at resting echocardiography.

The main flaw of the study is methodology, since patients have not been properly selected. Even if the study is retrospective, you should select a consecutive slot of patients in a pre-determined time frame. Selecting only patients with adequate Doppler signal for measurements introduces a bias. Then, you should specify the number of patients of your initial population. This would provide at least information on feasibility, which is especially important for the parameter you are evaluating, AT.

Please, differentiate in the text PH diagnosis (which can be made only by right heart cath) and increased PASP (which you can observe by echo). When you state that “a large group of patients with PH defined as SPAP above 38 mmHg” you should specify that these are patients “likely to develop PH”, as the European Guidelines suggest.

You should specify the criteria you used to select the 29 patients who underwent right heart cath.

The sentence “However, due to poor correlation of RAP estimation from echocardiography and RHC, a general addition of 8 mmHg was used for SPAP estimation as explained in result section” in the Methods section shows another flaw in your methodology: you cannot change your criteria according to your results. Then you state that “We therefore decided to either use just the trans-tricuspid valve gradient (TTVG) or make an estimation of systolic pulmonary pressure (SPAP) by adding 8 mmHg to the TTVG (8 mmHg was the mean RA pressure measured by RHC in the subgroup) as the best estimation of SPAP”. You should apply the same criterium to all patients to determine RAP, even if the echocardiographic-derived values do not correlate with the invasive ones.

It seems unlikely that the PVR calculated by the Abbas’s formula had such a different strenght of correlation with invasive PVR, compared to the ratio of TTVG and TVI RVOT. Please, provide an explanation.
The last two sentences of the Background section should be part of the Methods section.

The paper needs English mother-tongue revision.

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

**Quality of written English:** Needs some language corrections before being published

**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