Letter by Guazzi Regarding Article, "Right Ventricular Function in Heart Failure With Preserved Ejection Fraction: A Community-Based Study"

To the Editor:

I read with interest the article by Mohammed and coworkers[1] on the prevalence and prognostic significance of right ventricular (RV) dysfunction in a large set of patients with heart failure with preserved ejection fraction. RV function was assessed by tricuspid annular systolic peak excursion (TAPSE) in 500 of 562 patients and by semiquantitative assessment in the whole population. Interestingly, patients with either mild or moderate to severe RV dysfunction by semiquantitative assessment compared with no RV dysfunction were more likely to present with atrial fibrillation, permanent pacing, and diuretic treatment. On the contrary, just a lower TAPSE tertile was reflective of a worse clinical condition.

The analysis is strengthened by an 8-year outcome follow-up. Findings are new and stimulating, addressing a topic of growing attention in the heart failure with preserved ejection fraction syndrome that has been clearly underestimated in the past.[2–4] I would like to extend the discussion a little as follows.

TAPSE is an endurance index of RV systolic function that, although feasible, easy to obtain, and clinically meaningful, does not reflect the contractile state of the RV but just its motion and is afterload dependent. In a previous study by our group[3] performed in a group of patients with either systolic or heart failure with preserved ejection fraction, we obviated this potential disadvantage by examining the TAPSE versus pulmonary artery systolic pressure relationship, looking at the first as a variation in length and the second as a developed pressure or force. This in vivo length-to-force relationship provided robust clinical and prognostic insights that were stronger than those provided by TAPSE alone. It should be interesting to know how this approach may work in the present population and whether it may help to differentiate disease severity and prognosis at a further step.

The other consideration I would like to raise is merely technical. I was surprised that semiquantitative analysis of RV function was indeed superior to that of TAPSE. I wonder whether the less striking prognostic TAPSE validity can be explained as a consequence of combining the validated M-mode–derived measure with a 2-dimension–derived assessment. There was actually a good correlation between the 2 types of measurements, but correlation per se is not a guarantee of equivalent clinical performance.

Disclosures

None.

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