Reviewer’s report

Title: Reliability of three-dimensional color flow Doppler and two-dimensional pulse wave Doppler transthoracic echocardiography for estimating cardiac output after cardiac surgery

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Reviewer: Rodolfo Citro

Reviewer's report:

In the paper entitled "Reliability of three-dimensional Color flow Doppler and two-dimensional pulse wave Doppler transthoracic echocardiography for estimating cardiac output after cardiac surgery" the Authors evaluated the reliability of three-dimensional color flow (3DCF) and two-dimensional pulse wave Doppler (2D-PWD) transthoracic echocardiography for estimating cardiac output after cardiac surgery.

Three different methods (third generation FloTrac/VigileoTM [FT/V] system as the reference method, 3DCF, and 2D-PWD) were used to estimate CO before and after interventions (baseline, after volume expansion, and after a dobutamine test).

The authors concluded that 3DCF could accurately estimate CO in post-cardiac surgical patients compared with a FT/V system and that the two methods could be considered interchangeable. Conversely, also if 2D-PWD echocardiography was not accurate as the 3DCF, its ability could track directional changes.

There are some issues that have to be resolved.

Major comments:

1) The small study population (only 20 patients) drastically limits the significance of results.

2) Overall it is a good idea, but the inclusion criteria are the main problem. The Authors state that enrolled "adult cardiac surgical patient". Please remove "adult", as the age < 18 and > 80 is an exclusion criteria. Among exclusion criteria there are aortic diseases and arrhythmias, but which ones? Were all patients in sinus rhythm? Were also extrasystoles considered arrhythmias? Extrasystoles might affect 3DCF analysis. Why was mitral regurgitation not excluded? Also it affect CO. Please revise inclusion and exclusion criteria.

3) It is not clear if study population is prospectively enrolled. Please specify in Methods.

4) In Results the Authors state that only three patients needed low doses of norepinephrine (< 0.05 μg/kg/min) to maintain their blood pressure. This is very strange, because they underwent major surgery (on pump surgery?) that has always an important hemodynamic impact. Thus, are
the patients pre or post surgery selected and enrolled? Were data collected after many days from surgery? Please specify.

Moreover, norepinephrine increases preload effects, increasing peripheral vasomotor tone, and increases afterload, increasing mean arterial pressure (MAP) (See for example Maas, J. J., Pinsky, M. R., de Wilde, R. B., de Jonge, E., & Jansen, J. R. (2013). Cardiac Output Response to Norepinephrine in Postoperative Cardiac Surgery Patients. Critical Care Medicine, 41(1), 143-150). Therefore, it can either increase or decrease cardiac output and in 3 patients CO is certainly not real but pharmacologically sustained. This aspect should be described in limitations as bias of selection, because it can significantly affect the results.

5) After general anesthesia, data were collected in patients underwent endotracheal intubation. This aspect is not clear. Enrolled patients were sedated via propofol and remifentanil and ventilated using intermittent positive pressure ventilation. Both propofol and remifentanil, as well as PEEP, have effect on haemodynamics. This is a limitation in calculating CO.

6) In Background enrolled patients are defined "hemodynamically stable patients". What does it mean?

7) Were all coronary artery bypass graft performed on pump? Please specify.

8) In Results the Authors state that all patients had no metabolic abnormalities after surgery. This is confounding and very strange.

9) In Background the Authors state: "Hemodynamic optimization has been shown to improve postoperative outcomes for moderate- and high-risk patients and for cardiac surgical patients". Conversely, enrolled population has a moderate-low risk as emerges from EUROSCORE and from no necessary of inotropic and vasoactive drugs in ICU. The Authors should specify that their population represent a subset of ICU patients after cardiac surgery.

10) It is not clear if the Authors acquired LVOT VTI in the apical five chamber view or apical long axis view in all patients. Is the final LVOT VTI value the mean of two?

11) The Authors should show also other echocardiographic findings, especially LVOT parameters, SV, CO. Please add a table.

12) Pearson's coefficients were 0.260 between the CO-FT/V and CO-PWD measurements. This finding is really not strong. Thus, the Authors should stress that this result is not conclusive.

13) Were SBP, DBP and MAP collected with intra-arterial blood pressure monitoring?

14) Table 2 is not representative. Please add a graphic.

15) What might be clinical relevance of present study and in which types of patients (those with good acoustic window, etc) 3DCF and 2D-PWD might be useful? Please comment on the number of patients that were excluded from this study due to poor image quality. Of note, all patients had sternotomy. This is absolutely important as it highlights the low feasibility of obtaining these parameters.
Minor comments

1) Specify what means PAC (Page 5, Line 9)

2) Correct "verses" (Page 25, Line 23)

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