Reviewer’s report

Title: Non invasive Evaluation of Cardiomechanics in patients undergoing Mitraclip procedure

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Reviewer: Gillian Whalley

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This is a small retrospective study that seems to be suffering from the lack of prospective planning.

Major revisions:

Methods –
Selection of patients – were ALL patients who underwent the mitral clip procedure enrolled? It would seem unlikely, given the following statement; “…single expert operator using echocardiography the day before the procedure and at discharge from the hospital in all patients.” This suggests that only patients who had the same echo operator were included. If these are not all patients, then please explain how many procedures were completed and how many were excluded from this analysis. Adding the dates of study recruitment would be helpful.

In general, the echo methods are scant. Please include more details, such as echo machine, whether measurements measured online or offline and all at the same time, how were they measured (eg LV volumes, and elastance).

Results – there are no data presented about timing of the post-procedure echoes, other than saying these were done at discharge. The timing is important. I imagine these echoes were done very close to the procedure, and although I understand the immediate reduction in the degree of MR, I find it surprising that other parameters have regressed so quickly, ie left atrial size has gone down, but the mitral gradient has not. The timing of these changes needs to be discussed in terms of mechanism and unloading of the heart.

Table 1 – I don’t understand how the EF has gone down, when the stroke volume has increased, in the presence of a reduced end-systolic LV volume, and a reduced (but not to the same extent) end-diastolic LV volume. This reduction in EF is seemingly then ignored throughout the remainder of the manuscript.

The authors should check the data in the last row of the table for possible duplication of numbers

The authors report no correlation between Ees and EF and since this is a major component in their discussion of why LV EF doesn’t matter, it would be good to see this graph. Were the measures of LV function related to any measures of
severity of MR.

I am not sure the graphs add much more than could be addressed in the text or table.

Discussion –
The authors assert “The observed reduction of the ejection fraction must not be considered as an index of decreased function and instead should be considered as a consequence of a lower preload, as evidenced by the reduction of the left atrial and ventricular volumes.” Without any evidence to support this, other than a lack of correlation between Ees and EF.

The authors state:
“Furthermore our study suggests that data traditionally obtained invasively can be monitored at the bedside during and after the hospitalization of such challenging cases.” They have not correlated these data with invasive data so cannot conclude this.

The discussion is not deep enough. The current study is mentioned briefly and other literature reported, but the mechanisms and challenges are not speculated upon enough. This is largely a confirmative study of others’ work and suffers form being small and retrospective.

**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 I have no competing interests.