LETTERS TO THE EDITOR

3-Dimensional Echocardiographic Assessment of Left Ventricular Dyssynchrony

An Alternative Viewpoint

The results in the article by Sonne et al. (1) confirm those previously published by ourselves and others with respect to the dependence of 3-dimensional systolic dyssynchrony index (SDI) on age, left ventricular (LV) function, and QRS duration (2–6). However, the authors have derived conclusions that are in direct conflict to their previous (surprisingly not quoted here) work (7–11) and that of many others (2,12–14).

They state that determining the nadir of a low-amplitude and frequently noisy regional volume curve is difficult. However, noisy curves are (in our collective experience) rare and occur because of: 1) multiple manual editing of the 3-dimensional endocardial boundaries; 2) low temporospatial smoothing setting on the software; and 3) inclusion of datasets with poor image quality and stitching errors. It is surprising that apparently no patients were excluded from this study because of inadequate image quality or atrial fibrillation/irregular R-R intervals, nor did the authors validate datasets for stitching errors or check the software selected nadir.

The 2 dilated cardiomyopathy (DCM) examples in Figure 3 (1) are not representative and have obviously been chosen to make a point in that they both had much greater SDIs (17.8% and 16.7%) than the quoted means (8.7% and 9.1%) for the groups. In addition, proportional (rather than absolute volume curves, as in Figure 3 [1]) usually are used to validate the position of the nadir, which makes error less likely.

There are no data or statistical analyses presented to support the speculative hypothesis that noise is a source of error in this technique, and we are not told how frequently this issue occurs.

Because the interobserver variability of SDI measurements in most (2–6,12–14) studies is not high, this would also make “noise” an unlikely common phenomenon. The presence of “frequently occurring noise” would clearly increase the variability of measurements. Given that they have chosen examples to illustrate the point from the extreme end of their high SDI spectrum, one must assume that it is not really that common.

This same group have previously validated regional volume curves against cardiac magnetic resonance in a variety of different LV pathologies (15,16), and so it is surprising that they have now chosen to criticize the same basic technique.

The use of standard deviation as a way of deriving SDI also is criticized by the authors. We would agree that it is not a perfect statistical technique for describing the dispersion of events; however, it is widely understood and has been used successfully in other echocardiogram dyssynchrony indexes, where noisy curves are significantly more common.

The most surprising statements are that SDI is not useful for the selection of patients for cardiac resynchronization therapy (CRT) or the follow-up of its effects. However, only 32 patients with DCM were included, none of whom had CRT or follow-up, so it is difficult to understand how this conclusion can be justified, especially because these authors have previously advocated the use of SDI in CRT (8–11).

All patients with DCM apparently had an SDI that was greater than their normality threshold of 4%. This is different to an SDI threshold for response to CRT, which has been shown in several published studies (2,8,12–14) to be much greater than the normality threshold. The equivalent to this would be stating that because a normal LV ejection fraction (EF) is >55%, the LVEF cannot be used as part of the CRT selection criteria because all patients with DCM have an EF <55%, whereas we know that an EF ≥35% is an important value for CRT selection.

In 2006, this same group (9) stated that “ASI [assynchrony index] (real-time 3-dimensional echocardiography [RT3DE] SDI) should be measured in all patients with DCM and in patients who are candidates for CRT, irrespective of QRS width. The use of RT3DE provides a rapid assessment of LV (dyssynchrony) of the entire ventricle. The QRS width should not be used as a criterion for CRT indication because DCM patients without left bundle branch block have increased asynchrony index in 55% of cases.” It is interesting that the authors now state that because 100% of DCM patients have increased SDI, it cannot be used for CRT case selection. How is it that the prevalence of dyssynchrony in DCM patients, investigated by mostly the same authors, could have changed so dramatically during a short period of time?

The RT3DE SDI has been shown in several studies (2,12–14) involving larger groups of patients, who actually had CRT, to be a useful tool that can form part of the selection criteria for this technique. It is not perfect and will be improved. However, we believe that the conclusions drawn by the respected authors in this study are not justified on the basis of the data they have currently or previously presented.

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REFERENCES

We are well aware of the previous publications on the various applications of real-time 3D echocardiography (RT3DE), including those published by your group and obviously by ours. We greatly respect your work and your opinions, even when you disagree with us. We also are aware that some of the findings from our recent study might be interpreted as controversial and have anticipated a debate after its publication. In our view, such a healthy debate is a legitimate part of the work of scientists, and it is what differentiates science from nonscientific theories that cannot be disputed, proved, or disproved.

We believe that it is important to report findings, even when they do not fall within the common tenets and may thus warrant controversy. Generally speaking, we believe that publishing only noncontroversial findings while withholding findings contradicting previous publications is a dangerous approach that risks endorsing and perpetuating what may at times be only partial truths. There are many claims in your letter that we would like to briefly dispute, one by one, within the limited space allocated for this response.

Regarding the claim that our report contradicts our own previous publications, the unexpected findings of our study were as follows: (1) the normal range of the systolic dyssynchrony index (SDI) was half the magnitude of that previously established in smaller groups of normal subjects when a slightly different segmentation scheme was used; and (2) as a result, all patients with dilated cardiomyopathy (DCM) had abnormally high left ventricular (LV) dyssynchrony irrespective of QRS duration. These findings have important clinical implications for the selection of patients for cardiac resynchronization therapy and may partially explain the difficulties encountered by other investigators (1) and more notably in several recent multicenter studies.

Your claim that this study contradicts our own work was supported by a statement that we chose to cite only publications by others while “hiding” our own. The list of our publications you provided to prove this point consisted of 4 abstracts (references 8 to 11 in Monaghan et al. [2]). Two of these abstracts described our initial results in small groups of patients that led us to design the study by Sonne et al (3). The other 2 abstracts focused on epicardial pacing in patients with single ventricles, which are not relevant to this discussion. Of note, all 4 abstracts should not have been cited because they were published before 2006, i.e., more than 2 years earlier, and thus citing them is not allowed according to the JACC instructions for authors.

Importantly, your list of our “undisclosed” publications contained no peer-reviewed articles, which would endorse the use of RT3DE-derived SDI in patients with severe LV dysfunction, simply because such articles do not exist. In fact, one article you mentioned (reference 7 in Monaghan et al. [2]) focused on LV dyssynchrony and compared RT3DE and tissue Doppler imaging measurements of dyssynchrony in a group of 122 patients with a