To the Editor:

In their Letter to the Editor, Drs Berenfeld, Pertsov, and Jalife criticized the study claiming that we did not address technical issues and overinterpreted our data. Surprisingly, the issues that they raised and their concerns over the interpretation of the results were addressed and available through a careful reading of our article.

Berenfeld et al. criticized the study claiming that we did not address technical issues and overinterpreted our data. Surprisingly, the issues that they raised and their concerns over the interpretation of the results were addressed and available through a careful reading of our article. They also state that our study "provides important and useful information, particularly in regard to the existence of action potential duration gradients during VF." With respect to these comments, we should clarify that we did not focus on the nature of wave organization in ventricular fibrillation (VF), nor did we show the existence of action potential duration (APD) gradients during VF.

We correlated APD gradients measured in hearts with a normal rhythm (either paced or sinus rhythm) with the distributions of mean activation intervals and fast Fourier transform (FFT) spectra around the epicardium from the same hearts in VF. The main conclusion is stated in the title of the article.2

Berenfeld et al.3 deduced that we used an incorrect definition of "dominant frequency" as a monolithic frequency rather than the "maximum frequency" that can be found in more complex, multiple peak spectra. On the contrary, we adhered to the definition of dominant frequency as the maximum frequency, as used by Zaitsev et al.3 The problem, however, is how to objectively select a "maximum frequency" as a monolithic frequency rather than the "dominant frequency," but we showed in Figure 6 a more complete 3-dimensional plot of all frequencies as a function of x-y position on the heart.2

Berenfeld et al. argued that our failure to observe large areas with a monolithic dominant frequency was the result of insufficient spatial resolution. As discussed in our study (page e56, lines 18–22, right column), we also felt that the greater spatial resolution of CCD cameras could result in simpler FFT spectra. However, it is misleading to focus only on pixel number, because there are more important differences in our experiments such as higher sampling rate, greater signal to noise ratio, narrower depth-of-field, and species differences (see Discussion).2 It should be emphasized that, although possible, it is unlikely that increasing the pixel number can account for simpler FFT spectra. For example, if each photodiode (viewing 0.81 × 0.81 mm² of epicardium) is subdivided into 25 (5 × 5) CCD pixels that record the same maximum frequency, it is unlikely that their sum (the diode signal) will result in a spectrum with multiple peaks of approximately equal amplitudes. It could happen if adjacent CCD pixels detected a common frequency but at vastly different phases, an unlikely event in electrically coupled myocardium (no ischemia), particularly across such small areas.

Berenfeld et al. also objected to our measurements of local conduction velocities and interpretation. First, we are fully aware that transmural propagation at breakthrough sites can introduce errors, and the study described a reasonable method to delete these sites from the analysis (see Materials and Methods, page e50, 4th paragraph, right column).2 Second, the analysis of velocity vectors was not limited to 5 sites on the apex and base but was carried out throughout the epicardium (except breakthrough sites). Third, we disagree with Berenfeld et al. that poor spatial resolution may hamper measurements of velocity. Sampling theory tells us that to measure the velocity of propagating waves, conditions for both spatial and temporal resolution must be satisfied.4 For spatial resolution, the wavelength of the moving wave must be ≥ the distance between 2 pixels and for temporal resolution, the sampling interval must be ≤ the time taken by the wave to propagate between adjacent pixels.4 During VF (no ischemia), reasonable ranges for velocities and frequencies are 0.1 to 0.3 m/s and 10 to 30 Hz, respectively. Hence, the shortest wavelength (3.3 mm) is greater than 2 pixel distances (1.6 mm) and at a sampling interval of 0.5 ms and 0.81 mm between pixels, we can measure a velocity of up to 1.62 m/s. Fourth, we were extremely cautious in our appraisal of the local velocity vectors data and did not overinterpret our results. We clearly stated in the discussion and quote: "Hence, it should be emphasized that local conduction velocities measured in the present study may contain components of transmural propagation even after precautions were taken to eliminate zones of highly synchronous activation. For this reason, the analysis of conduction velocities should be limited to test for the occurrence of Wenckebach-style conduction and should not be overinterpreted to provide evidence for the wave breakup or the mother rotor hypotheses" (page e57, lines 29–37, left column).2

In conclusion, we would like to thank Berenfeld et al. for expressing an interest in our work and for drawing attention to this contribution to the field. We firmly believe in the highest possible standards of critical self-evaluation and welcome all serious well-reasoned criticism.

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Letter to the Editor: Ventricular Fibrillation: Mother Rotor or Multiple Wavelets?
Bum-Rak Choi, Tong Liu and Guy Salama

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