Letter to the Editor

Letter by Jalife et al Regarding Article, “Quantitative Analysis of Localized Sources Identified by Focal Impulse and Rotor Modulation Mapping in Atrial Fibrillation”

Nothing is more irredeemably irrelevant than bad science
—John C. Polanyi
German-Canadian chemist
Nobel Prize for Chemistry in 1986

To the Editor:

In 1998, we introduced a unique phase mapping algorithm that markedly enhanced the characterization of complex spatiotemporal patterns of cardiac fibrillation.1 We used a potentiometric dye and video imaging to record the dynamics of transmembrane potentials at many sites during fibrillation.1,2 Transmembrane signal at each site exhibited strong periodic components at 8 to 15 Hz and was seen as a quasi-closed trajectory in 2-dimensional phase space that could be represented by its phase around the circuit.1 Spatial phase maps at each instant revealed drivers of fibrillation in the form of rotors displaying long-lasting topological defects or phase singularity points at a few sites. We also demonstrated that such drivers are caused by a singularity event termed wavebreak. Subsequently, the combined use of phase and dominant frequency (DF) maps of the optical movies led to the demonstration that atrial fibrillation (AF) in the isolated sheep heart was characterized by a hierarchy of DF domains where the highest domain (DFmax) corresponded to the location of the dominant rotor.3,2 More recent data suggest that rotors may underlie both paroxysmal and persistent AF in humans.4–12 Thanks to the recent introduction of mapping technology that uses a multielectrode basket catheter and is designed to identify highly localized drivers capable of maintaining AF, the field of AF ablation has begun to move away from purely anatomically based strategies. During the last several years, ablation studies using novel mapping procedures specifically designed to assess the mechanism(s) underlying potential AF sources have been reported with promising results. Such mechanistically based ablation techniques are a direct result of insights into the dynamic behavior of reentrant sources (rotors) derived from experimental optical mapping studies and computer simulations of AF dynamics.

A case in point is the work that has been published recently in a series of articles by Narayan et al,9,10 and others13 who have offered a mechanistically based tool that already has shown substantial scientific validity and integrity of the overall study. In addition, the protocol is littered by issues that have contributed to raise skepticism in the minds of a few opinion leaders about its usefulness. In particular, the basket catheter system uses a proprietary algorithm (Rhythm View; Topera Inc., CA), which makes the methodology difficult to evaluate because the electrograms from which the activation maps are obtained are not the primary display. Although not unique to the Rhythm View system, extracellular signals are subject to artifacts,14 and ventricular activity often contaminates atrial recordings; thus, appropriate location of the basket catheter and QRST subtraction are both paramount. Also the basket catheter often provides far from optimal electrode tissue contacts at many poles, and the splines are sometimes not equidistantly separated once they are deployed in the atria. Therefore, the raw interspline spatial resolution offered by basket catheters is poor. Consequently, the appropriate amount of interpolation when scarce or poor quality data are present is difficult to determine. Furthermore, it should be considered that interpolation of phases is inherently biased toward detection of rotors as the algorithm is devised to demonstrate rotational activity. Thus, a focal activation might be displayed as rotational activity if the wavefront reaches the surrounding electrodes sequentially.15 Nevertheless, although the FIRM-guided approach to AF ablation is not universally accepted, even with its inherent limitations, it is one of the first mechanistically based methodologies that have reported promising results in the long term16 and after its first multicenter validation.17

The study by Benharash et al16 published recently in Circulation: Arrhythmia and Electrophysiology, aimed at testing the FIRM procedure by quantifying the spectral properties and regularity of atrial electrograms recorded at rotors sites during AF ablation. The authors included a retrospective analysis of 24 consecutive patients undergoing AF ablation, with a significant percentage of patients having a previous history of multiple failed ablation procedures. Benharash et al16 used the Topera system to determine the sites of rotor activity. Rotor sites were targeted for AF termination, slowing of AF cycle length or organization. Further analysis included activation maps using unipolar signals from the basket catheter, Shannon entropy, and DF.

Contrary to what has been shown by others,13,17 the report by Benharash et al16 did not observe any significant differences between rotor sites and atrial areas without rotors. Neither DF analysis nor Shannon entropy analysis showed any differences between sites. There was a low rate of AF termination using FIRM-guided ablation. Notably, atrial unipolar signals were poor in all cases and only an average of 20 signals was suitable for analysis. Furthermore, a substantial area of the left atrium was not considered for analysis because of inability of the basket catheter to cover the entire endocardial surface. These authors concluded that, in conjunction with pulmonary vein isolation, catheter ablation at FIRM-identified rotor sites resulted in AF termination or organization to atrial tachycardia in only 4 of 24 patients. Such outcomes were so remarkably poor that the article prompted an editorial that called for additional studies to better understand persistent AF mechanisms.18

Close examination of the data presented by Benharash et al16 reveals that, in their attempt to test the validity of FIRM-guided analysis, they committed an unsettling array of mistakes that likely contributed to their failure, which unfortunately has become a source of confusion for readers of Circulation: Arrhythmia and Electrophysiology who are unfamiliar with the topic of rotor mapping. Clearly, rigorous and detailed analysis of their results raises significant concerns about the scientific validity and integrity of the overall study.
Of greatest concern in the Benharash et al16 report are the osten-
sible difficulties they had in properly deploying the basket catheter, which
certainly may explain their poor signal recordings. For exam-
ple, they use Figures 6 and 7 as evidence supporting their contention
that even using the NavX electroanatomic mapping system they were
unable to annotate activation times automatically because of poor sig-
nal quality of the basket catheter. This is precisely why phase map-
ning should have been performed. The lack of identifiable activations
is what is actually expected at the core region of rotors. By excluding
these electrodes, the authors basically eliminated the possibility of find-
ing what they were looking for. A more appropriate approach would
have been to quantify where those low-amplitude signals were located;
if they were at FIRM-based rotor sites, then the data would have been
consistent with the presence of a rotor. Unfortunately, QRSST subtrac-
tion images were not shown for Figure 6 that may have revealed that
atrial signals may be detected unimpinged. Therefore, unless otherwise
revealed, the best interpretation one can give to the data in Figure 6
is that basket positioning was poor, as reflected by electrograms that
often displayed more ventricular than atrial signals! Indeed, Figure 1B
and 1C show cases selected by the authors where baskets were placed
in the left ventricle based on published studies on the pitfalls of basket
placement (compare with Figure 1H and I of Narayan et al19), whereas
in Figure 1A, the basket was undersized and floated in the atrium (com-
pare with Figure 1J of Narayan et al19). Similarly, unless otherwise
demonstrated, the apparent change in the chirality of the rotation in
Figure 7 of Benharash et al16 was most likely the result of inappropriate
marking of the activation timing, which in unipolar recordings should
be the most negative slope rather than the peak as the authors mistak-
enly have done. Again, phase analysis would have helped here. Phase
analysis gives no particular weight to the activation timing and consid-
ers as equally important all the phases of the action potential.

An additional limitation that affected the results of Benharash
et al16 is their lack of attention to detail in the electrophysiological
approach they have used to generate their phase maps. Narayan et
al complemented their development of FIRM mapping from basket
catheters with monophasic action potential catheter recordings to
define action potential duration and regional conduction restitution,4
which were essential to identify rotor activity and slow conduction
areas. The latter was completely missed by Benharash et al.16 As such,
their results can hardly be compared with those generated by others
using the FIRM-guided approach.

DF is yet another example of poor signal analysis in Benharash et
al. The authors report in Figure 5 and Table 2 that there are no differ-
ences in DF or Shannon entropy distributions between rotor sites and
other regions; by itself, this similarity in distributions does not preclude
the presence of a driving rotor and seems to demonstrate that their
rotor sites were probably fibrillatory activity rather than the dominant
rotor at the highest DF site. In addition, the data presented as evidence
for FIRM rotor identification do not display anything that may sug-
gest the presence of a rotor by FIRM or any method. In fact, no single
FIRM map or electrogram is provided to verify that DF is measured
correctly from the appropriate location at the precise time period anal-
alyzed, which is critical because rotors meander in space.21,22 Even if
their DF measurements (by FIRM) are correct, a frequency of 3.6 Hz
is not particularly high (reflecting a cycle length of 278 ms), and proba-
bly resulted in 1:1 propagation to large areas as suggested by the relatively
homogeneous DF map in their Figure 4 and may reflect atrial tachy-
cardia without fibrillatory conduction. Furthermore, although Shannon
entropy has been previously used to detect rotors using bipolar signals,
the use of Shannon entropy to detect rotors with unipolar signals has
not been validated. Therefore, it is unsurprising that so-called rotor
identification in the article by Benharash et al failed to terminate AF.

In conclusion, although skepticism facing FIRM mapping and
ablation is healthy and necessary, the article by Benharash et al16 does
not have the material or the scientific quality to counter the claims of
the FIRM approach. Clearly, ablation failure does not mean that there
are no rotors/foci driving AF. We submit that the battery of mistakes
committed in the design, conduction, and interpretation of the results
contributed more than anything to such a failure. Therefore, although
we agree that on available evidence, the relative contribution of sus-
tained or transient rotors to the mechanism of human persistent AF
remains uncertain18 and requires further support, we firmly propose that
the evidence provided by Benharash et al16 should not be taken as
contributing constructively to such an uncertainty.

Disclosures

Dr Jalife is on the Scientific Advisory Board of Topera Inc. Dr
Berenfeld is the Scientific Officer and has equity in Rhythm Solutions,
Inc. The other author reports no conflicts.

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