long-term temporal pattern of arrhythmia recurrence is a neglected area of research and warrants further investigation.

Mark A. Wood, MD  
Kenneth A. Ellenbogen, MD  
Cardiac Electrophysiology Laboratory  
Medical College of Virginia  
Box 980053  
Richmond, Virginia 23298-0053

Larry S. Liebovitch, PhD  
Florida Atlantic University  
Boca Raton, Florida

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REPLY
We appreciate the interest of Wood et al. in our recently published paper on electrical storm in patients with implantable cardioverter-defibrillators (ICDs) (1). In their comments, they reemphasize the importance of defining electrical storm based on temporal patterns of arrhythmia clustering. As stated in our report (1), the precise definition of this syndrome is still evolving; therefore, we adhered to the most commonly used definition (2,3), realizing that it is still somewhat arbitrary. Specifically, we are aware of the findings of Wood et al. (4) indicating that two episodes of arrhythmia detection by the ICD within 1 h may not necessarily be an unusual finding. It appears important to note, however, that the median number of arrhythmic episodes constituting electrical storm in our patients was 17 within a single 24-h period leading to the need for urgent therapy in most patients (1). Although we had to focus on the timing of arrhythmia clustering, as with any definition of electrical storm, the clinical picture substantially contributed to the definition in our series. Moreover, we were interested in several other issues related to electrical storm, such as precipitating factors, therapeutic measures and potential prognostic implications. In summary, we agree with Wood et al. (4) that more research is needed to arrive at a more precise definition of electrical storm. Our paper aimed to be one step in this direction.

Stefan H. Hohnloser, MD, FACC  
Susanne Credner, MD  
Innere Medizin IV  
J.W. Goethe University  
Frankfurt, Germany

Use of Radial Artery Applanation Tonometry
Cameron et al. (1) reported clinical evaluation of a system that was developed at St. Vincent’s Hospital by us (2) and that uses a generalized transfer function to estimate the calibrated ascending aortic pressure waveform from the radial artery pressure pulse wave. The authors concluded that waveform analysis is of limited value and that simple linear relations are sufficient to generate central from cuff sphygmomanometric pressure values in individual patients. We disagree.

In our own continuing evaluation, we have interrogated our large data base (15,533 reports in 1,604 patients/subjects) and have participated in studies where the estimated calibrated ascending aortic waveform is compared to the pressure wave recorded simultaneously by an intraarterial catheter from the ascending aorta (3–5). In the first group of studies, we have obtained results for systolic, diastolic and augmented pressure, which are very similar to those reported by Cameron et al., and have shown the same wide scatter in values of systolic and augmented pressure for measured radial and estimated aortic waveforms. We take these results to show the potential for generating more precise indexes of left ventricular load and function than those available with the cuff sphygmomanometer alone. In the second group of studies, we have compared directly measured and estimated ascending aortic pressure waves and indexes derived therefrom. We have shown a close correspondence between estimated and measured aortic pressure indexes in individual patients under control conditions and with physiologic (Valsalva maneuver) and pharmacologic perturbations. Indeed, correspondence between estimated and measured ascending aortic measurements generally fell within the AAMI (Association for the Advancement of Medical Instrumentation) requirements for comparing different methods (6), whereas correspondence between measured aortic and upper limb values did not. Virtually identical results have been reported for a similar system (7,8).

The evaluation by Cameron et al. (1) downplays the value of information carried by the arterial pressure waveform and focuses on cuff sphygmomanometric values. Their evaluation is limited by the fact that all measurements (brachial cuff sphygmomanometry with radial tonometry) were taken in the upper limb and none from a central artery. Their comparison of “central” and peripheral mean pressure was simply of the integrated calibrated radial waveform against the mean brachial value determined by an oscillometric method.

We remain convinced that use of the cuff sphygmomanometer can be improved by incorporation of information provided by the pulse waveform in the upper limb. Studies such as that by...
Cameron et al. (1) can only show the problems of an old approach and the potential of the new. Presently, a consensus is emerging that the central pressure waveform generated by the method we have described (2) corresponds well to measured aortic pressure under control conditions and during different perturbations in individual patients (7–9).

Michael O’Rourke, AM, MD
Xiong-Jing Jiang, MB
Medical Professorial Unit
University of New South Wales
St. Vincent’s Hospital
Darlinghurst, New South Wales Australia

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REPLY

In their letter, O’Rourke and Jiang suggest we downplay the information carried by the arterial pressure waveform. We find this a surprising interpretation of our work (1), which in fact discusses aortic pressure augmentation as well as radial artery applanation techniques in a broad context and which illustrates a number of important general considerations of assessment of aortic pressure augmentation.

Our study of 262 treated hypertensive patients examined factors contributing to aortic pressure augmentation as assessed using the PWV Medical Blood Pressure Analysis System (PWV Medical, Sydney, Australia), which uses a generalized transfer function and radial artery tonometry. We found derived indexes of aortic pressure augmentation to be significantly influenced by age, gender, height and heart rate, but not by the different antihypertensive agents used. To our knowledge, this study was the first to use such devices, and we were particularly interested in the practical advantages, if any, of transfer function approaches in comparison to the more widely reported carotid artery applanation techniques. It must be remembered that the use of transfer function techniques creates an extra level of complexity that needs further cautious evaluation before use in inferring central arterial variables.

O’Rourke et al. suggest our analysis is limited by a lack of central pressure measurements; surely this is exactly the benefit proposed for any useful noninvasive device. If central pressures are available, radial applanation becomes superfluous. Similarly, we reject the assertion that the so-called “wide scatter” of blood pressure values can be used to infer a potential for improvement in measurement of variables for which, noninvasively, no absolute value exists for comparison. Comparison of derived central and brachial mean pressures was as described; however, because this is the device’s in-built basis for calibration and therefore of derived central blood pressure, it seems strange that this is called into question by these commentators.

It is premature and unsubstantiated to suggest the evolution of any “consensus” regarding this type of method. Evaluation of the validity of a generalized arterial transfer function, which, we stress, was not the objective of the current study, requires simultaneous invasive and noninvasive assessment of blood pressure. To date, there has been inadequate published work on this topic, and it should be noted that reports of invasive studies, which are sometimes used to justify noninvasive transfer function techniques, have been on small numbers and in select patient groups (2,3). In particular, there appears to be no such prospective evaluation of the PWV system, and we await with great interest the publication of the data base referred to in Dr. O’Rourke’s letter.

James D. Cameron, MD
Barry P. McGrath, MD
Anthony M. Dart, MD
School of Electronic Engineering
La Trobe University
Bundoora, Victoria Australia

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Beta-Blockers—A Forgotten Antiventricular Tachy-Fibrillation Drug Class?

I have two questions regarding the recent study by Pires et al. (1). Seventy-two percent of these patients were on antiarhythmia therapy, and is it not possible that proarhythmic effects of these drugs instituted the terminal episode of cardiac arrest, particularly polymorphic ventricular tachy-fibrillation episodes. Second, would...