Letter to the Editor

Prediction of Primary Slow-Pathway Ablation Success Rate according to the Characteristics of Junctional Rhythm Developed during the Radiofrequency Catheter Ablation of Atrioventricular Nodal Reentrant Tachycardia

I was interested in the study published by Dr. Bagherzadeh et al.[1] and congratulate them on their publication. There are some points in the aim of their study, method, and results, however, which I believe require further clarification.

1. The Introduction states that "Thus far, the most conventional marker and end point for successful radiofrequency (RF) ablation has been considered the loss of the inducibility of atrioventricular nodal reentrant tachycardia (AVNRT); be that as it may, it has been reported to be not inducible in up to 10% of patients. This disadvantage has prompted scientists to focus on identifying an accurate alternative end point for predicting the success rate of the slow-pathway RF ablation of AVNRT." Also, the Discussion somehow repeats that: "Loss of the inducibility of AVNRT has been considered the end point for successful slow-pathway RF ablation; however, AVNRT is not inducible in up to 10% of patients during ablation. Nowadays, JR developed during the slow-pathway RF ablation of AVNRT has been identified as a sensitive surrogate end point for successful AVNRT ablation". I take from these statements that given the unfeasibility of AVNRT induction in 10 % of cases during the electrophysiology (EP) study in conjunction with the clinical presence of arrhythmia (and probably other factors that convince the operator that the patient's clinical arrhythmia is most probably AVNRT), when the operator decides to ablate the slow pathway, the presence of JR during ablation can be a surrogate for non inducibility. Nevertheless, in this article this is not the case. As the esteemed authors have mentioned in all their cases, arrhythmia was inducible and non inducibility was their gold standard for successful ablation. It is known that the 12-lead surface electrocardiogram (ECG) can only be suggestive of AVNRT, and after the completion of EP study and appropriate maneuvers can the diagnosis be confirmed.[2] All the authors' patients, therefore, must have had inducible arrhythmia and the above-mentioned sentences in the Introduction and Discussion seem unrelated to this study and its stated aims. Investigation into the said issues would require another study design whereby- for example- in a group of patients with documented supraventricular tachycardia (SVT) compatible with AVNRT but non-inducible in the EP laboratory, the effect of ablation and presence of JR during ablation could be studied in other surrogates such as evidence of dual physiology or even the clinical outcome.

2. In their ablation technique, the esteemed authors report that after each RF application, inducibility was checked after Isoproterenol infusion. I think this is not a common practice,[3] and most operators perform pacing maneuvers first and only if the arrhythmia is not inducible, do they tend to resort to Isuprel infusion. Furthermore, not only should the Isoproterenol dose and infusion duration be mentioned but also there should be a mention of the number of cases in which arrhythmia became inducible only after Isuprel infusion.

My other question is concerned with the RF power utilized in the study. Let us consider this scenario: If the investigators obtained JR but the highest temperature amounted to- for example- 47, did they stop the ablation or did they continue with higher powers? In my opinion, the power settings and policy on the minimum acceptable temperature rise in the study need elucidation.

3. I am none the wiser as to why the patients who needed more than 5 RF applications were excluded. Surely, this must have significantly influenced the average lesion number.

4. In the Results, the following points should be further elaborated upon:

A. The authors report that "Initial successful RF ablation with the loss of AVNRT inducibility was achieved in 43 (57.3%) patients using a total of 119 (73.5%) RF energy applications." What is missing here is a clear definition of the phrase: "initial success". If it means that these were the cases in which the first RF application was successful, why is the mean number of the RF applications in this group 1.77 rather than 1?

B. It is also confusing that in Table- 1, in the successful RF application group (43 patients), the mean number of the RF applications is 1.77 and the total number of the RF applications is 119 (which is expected to be: $43 \times 1.77 = 76$) and in the group with at least one unsuccessful RF application which contains a total number of 32 patients with a mean RF application of 2.69, again the total number of the RF application is 43, which is not equal to $32 \times 2.69 = 86$.

Needless to say, these figures should be corrected or clarified.

References

1. Bagherzadeh A, Rezaee ME, Moshkani Farahani M. Prediction of primary slow-pathway ablation suc¬cess rate according to the characteristics of junctional rhythm developed during the radiofrequency catheter ablation of atrioventricular nodal reentrant tachycardia. J Teh Univ Heart Ctr 2011;6:14-18.
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3. Jackman WM, Beckman KJ, McClelland JH, Wang X, Friday KJ, Roman CA, Moulton KP, Twidale N, Hazlitt HA, Prior MI. Treatment of supraventricular tachycardia due to atrioventricular nodal reenty by radiofrequency catheter ablation of slow- pathway conduction. N Engl J Med 1992;327:313-318.

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