Letter to the Editor

Fading magic of maze procedure

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I read with interest a recent article by Reston and Shuhaiber [1] where they have analysed the reported clinical outcome of maze-related surgical procedures for intractable atrial fibrillation (AF). I congratulate them for this excellent meta-analysis [1]. I would like to add a few comments.

Maze procedure is an excellent example of a technique which was grabbed by an impatient scientific community which believes in evidence-based practice but easily accepts and gets carried away by camouflaged data. As appropriately pointed out by Reston and Shuhaiber [1], the literature evaluating the clinical outcomes after maze procedure suffers from several shortcomings, particularly small sample sizes and selection bias. I would like to add (without daring to elaborate any further!) here a publications bias as well.

Maze-related procedures might be helpful in eliminating AF, but the long-term benefits in terms of mortality, morbidity and quality of life is yet to be proved. Most of us tend to forget that Cox et al. described this procedure after performing most of the maze surgeries in patients with lone AF and not those with valvular heart disease. Besides, the classical Cox maze procedure is technically complex, time consuming and leads to an increased rate of postoperative bleeding and need for permanent pacemakers. This led to several simpler modifications. In the absence of randomised controlled trials comparing the efficacy and/or utility of these procedures, we are suffering from tubular vision—everybody is seeing and finding what he or she wants to see in the statistical data presented.

This meta-analysis identified only four randomised studies out of which two studies had noncomparable patient groups [1]. Furthermore, a closer look reveals that two of these studies used radiofrequency ablation (RF). Data obtained from RF procedures cannot be compared to any other surgical technique as RF has got some unique advantages such as decreased incidence of postoperative bleeding. Apart from the outcomes which the authors have addressed in their meta-analysis, it will be interesting to know the impact of various techniques on AF recurrence rate, left ventricular systolic function and the quality of life.

Recently, Chaput et al. [2] reported that the conversion to sinus rhythm did not improve long-term survival or reduce the incidence of embolic complications after valve surgery. The efforts involved and the benefits obtained from conversion to sinus rhythm (if at all attained) are really questionable in patients with prosthetic valves who in any case will remain on anticoagulants. This question becomes particularly relevant if one takes into account the complications reported even with modified anti AF procedures [3,4]. We should not forget that in lone AF rate management gives comparable results to rhythm management [5]. In other words surgeons should not be made to believe that medical management of chronic AF is a thing of past. It is still very much relevant.

References


Reply to Mishra

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