Reply to the Letter to the Editor

Reply to Toumpoulis

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We appreciate the comments of Ioannis K. Toumpoulis about our paper concerning the use of immediate ischemic preconditioning based on somatosensory evoked potentials to prevent spinal cord injury [1]. In that study, it was shown that ischemic tolerance could be immediately achieved in the spinal cord against a clinically relevant ischemic insult, since ischemic preconditioning was based on the proper selection of induction times and intervals in dogs. This observation was obtained without significant differences between the groups with regard to proximal and distal aortic pressures at baseline, during the aortic cross-clamping and at the reperfusion period, as mentioned. During the aortic cross-clamping period, low doses of sodium nitroprusside were used to reduce the arterial blood pressure, explaining the described results. Nevertheless, no important hypotension was observed during the reperfusion interval in all the experiments of the three groups and it was not possible to detect any correlation between neurologic or histopathologic outcome with differences in arterial pressure during reperfusion or aortic cross-clamping time intervals.

We agree with Toumpoulis and coworkers [2,3] that hypotension episodes during the reperfusion period may influence the neurologic outcome in these experimental models of spinal cord ischemia after aortic cross-clamping. However, this finding does not appear to have influenced our data.

References


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Letter to the Editor

Do we need to further probe the utility of these (energy) probes?

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I read with interest the recent article by Chaput et al. [1] in the European Journal of Cardio-Thoracic Surgery where they have discussed the long-term implications of conversion to sinus rhythm following valve surgery. I would like to congratulate them for this timely article which has helped to dispel many myths associated with atrial fibrillation versus sinus rhythm in the postoperative period. Lately, atrial fibrillation has been accused to play a role in almost every complication occurring in the early/late postoperative period. No wonder multinational companies jumped to create hype and simultaneously came up with multiple energy probes to perform concomitant anti-AF procedures. Glossy pamphlets accompanying these energy probes have managed to send a strong message to the scientific community. Some authors have claimed curative treatment of AF with radiofrequency ablation techniques, but long-term benefits are debatable [2]. Besides, several other authors have reported potential complications which can occur with the use of these probes [3].

This article emphasises the fact once again that the great majority of patients who are in AF preoperatively continue to be so in the postoperative period without any impact on long-term survival or embolic events [1]. Postoperative AF is a significant risk factor for long-term mortality only for patients who are in preoperative sinus rhythm [1]. Time tested notion that the control of ventricular response and anticoagulation is more important rather than getting obsessed with the rhythm has been emphasised in several studies [4]. What we need is a robust predictive risk model for occurrence of AF post-operatively in patients who are in sinus rhythm before surgery. Risk stratification of these patients as to which of them will develop a persistent AF will be a tall order, as isolated episodes of AF are not uncommon in the postoperative period.

Critics might say that this was an observational study and we should take results from such studies with a pinch of salt. It will be a valid criticism of this study but then we do not have data from randomised trials to prove the benefits of conversion to sinus rhythm postoperatively either. Another limitation of this study is the impact on quality of life following conversion to sinus rhythm which the authors have not tried to answer. Some recent articles have reported a significant improvement in the quality of life in symptomatic AF on medical management undergoing catheter radiofrequency ablation [5]. Whether this benefit extends to patients following valve surgery needs to be evaluated. The benefit will most likely differ depending on preoperative LV function.

It is interesting to note that 43% patients had mitral valve replacement and only 4% had mitral valve repair [1]. Was it an institutional policy or the etiology of the valvular disorder which forced them to perform so many mitral valve
replacements? It will also be worth knowing whether a chordal preservation technique and the choice of prosthesis (mechanical vs bioprosthesis) had any impact on occurrence of AF postoperatively.

We need large multicenter randomised controlled trials of surgical AF ablation procedures. It sounds harsh but in my opinion we need to further probe the utility of these (energy) probes.

References


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Reply to the Letter to the Editor

Reply to Mishra

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I agree with Dr. Mishra’s comments. Concerning the low percentage of mitral valve repair, please note this is a historical cohort from 1979 to 2003. Mitral valve repair is the primary treatment for degenerative and ischemic mitral regurgitation in our institution since 1995. In the cohort of valvular patients studied in this paper it represents 13% of the isolated mitral valve procedures. All patients had posterior leaflet preservation, both leaflets were seldom preserved and the impact of preserving both leaflets has not been evaluated. Mechanical valves and bioprosthesis behaved similarly in term of conversion from atrial fibrillation to sinus rhythm in this study.

Thank you for your interest.

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