Reply to the Letter to the Editor

Reply to Takagi et al.

Munir Boodhwani a, Marc Ruel b,∗

aDivision of Cardiothoracic Surgery,
Beth Israel Deaconess Medical Center,
Harvard University, Boston, MA, USA
bDivision of Cardiac Surgery and Department of Epidemiology,
University of Ottawa, Ottawa, Ont., Canada

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In response to our meta-analysis [1] comparing surgical versus percutaneous treatment of isolated LAD disease, Takagi et al. [2] present, in the form of a letter to the editor of the European Journal of Cardio-thoracic Surgery, the results of a ‘sub-meta-analysis’ of a subset of randomized trials where only minimally invasive bypass techniques were utilized for surgical revascularization. The authors justify the need for this ‘sub-meta-analysis’ by stating that there was a significant heterogeneity in the method of surgical revascularization used between the studies included in our meta-analysis. Furthermore, they state that because the type of surgical revascularization was predictive of between-study heterogeneity, it is appropriate to explore the subset of studies where only minimally invasive surgical revascularization (MIDCAB) was employed. The authors identified and included five randomized trials (four of which were included in our study) and report that the pooled estimate favors MIDCAB with regards to major adverse cardiac events (MACE) (risk ratio of 0.41 [0.27, 0.60]).

The authors have succinctly presented the results of a pooled analysis of five studies, but it is worth asking the question whether this sub-meta-analysis adds any new information to that already presented. Although we had a priori identified various sources of between-study heterogeneity and chosen a random-effects model for analysis, we found that all tests for heterogeneity were non-significant (P = 0.44 for mid-term MACE in the randomized trials); this is similar to the findings of Takagi et al. [2] (P = 0.89).

Secondly, although we found that the type of surgical treatment was a univariate predictor of between-study heterogeneity, other univariate predictors were also significant, including the type of percutaneous treatment, duration of follow-up, and start year of the study. During meta-regression, only the start year of the study remained a significant multivariate predictor of study heterogeneity. In this regard, the start year of the study likely captures many key aspects of a study including the types of surgical and percutaneous treatments used as well as length of follow-up. This suggests, on statistical grounds, that a ‘sub-meta-analysis’ is not justified because statistically there does not appear to be significant heterogeneity to warrant considering this subset of trials separately. Not surprisingly, the findings presented by Takagi et al. [2] are similar to those previously presented by us.

Finally, the focus of our meta-analysis was the difference in the combined rates of mortality and myocardial infarction, because these, in our opinion, represent the most clinically meaningful endpoints that may change clinical practice. Differences in MACE, which are driven primarily by target vessel revascularization, are potentially less meaningful as many patients and their physicians may accept the possibility of repeated percutaneous interventions over surgery, provided that the risk of myocardial infarction or mortality is not higher. Furthermore, differences in MACE favoring surgery have been repeatedly demonstrated in adequately powered randomized trials. We are therefore of the opinion that future studies comparing CABG to PCI should focus on the clinically meaningful endpoints of mortality and myocardial infarction and be adequately powered to detect such differences.

References


∗Corresponding author. Address: University of Ottawa Heart Institute, Room H-3401, 40 Ruskin Street, Ottawa, Ont., Canada K1Y 4W7. Tel.: +1 613 761 4720; fax: +1 613 761 4713. E-mail address: mruel@ottawaheart.ca.

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Letter to the Editor

Temporary left ventricular pacing after cardiac surgery

Angel L. Fernández a,∗, José B. García-Bengochea a
Daniel Sánchez b, Julian Alvarez b

aDepartment of Cardiac Surgery, University Hospital, 15706 Santiago de Compostela, Spain
bDepartment of Anesthesiology, University Hospital, 15706 Santiago de Compostela, Spain

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We read with great interest the article by Flynn et al. [1] which compared the potential benefits of left ventricular (LV) pacing versus the right ventricular (RV) pacing in the early postoperative period after open heart surgery.

Flynn et al. [1] describe three pacing modes with the same atrial pacing system: active and inactive (negative and positive) leads were placed upon the right atrium in a bipolar pacing configuration. RV pacing was performed using two leads (active and inactive) placed on the RV in a bipolar pacing configuration. LV pacing was realized by using the LV lead as the cathode (active) and a RV electrode as the anode (inactive). This LV arrangement is called split bipolar configuration [2,3]. Flynn et al. [1] consider that this configuration affords univentricular LV pacing. Surprisingly,
other authors who have studied LV and biventricular pacing in surgical patients consider that this split bipolar configuration is a mode of biventricular pacing [4,5].

The aim of the study of Flynn et al. [1] was to assess the potential benefits of LV pacing in comparison to RV pacing. However, the mode of stimulation of the RV (bipolar) was different from the LV (split bipolar) and therefore the hemodynamic differences that they have observed may be related, at least in part, to different RV and LV pacing configurations.

From our point of view, Flynn et al. [1] could obtain LV pacing with their two LV leads by using one of them as a cathode (active) and the other as an anode (inactive) in a bipolar configuration. Reversing the polarity of LV wires is a simple method to change the site of LV activation.

Another possibility for LV stimulation could be placing two leads (active and inactive) immediately left to the mid left anterior descending coronary artery and two more leads (active and inactive) in the free LV wall. Anterior pacing of the LV could be obtained using one of the LV anterior wires as active electrode and the other as inactive electrode (bipolar configuration). LV posterior pacing could be obtained in a similar way using the LV posterior leads.

Different authors have used distinct terminology for the same cardiac pacing configuration. A clear and precise definition would be desirable.

References


*Corresponding author. Tel.: +44 191 950 212; fax: +44 191 950 227. E-mail address: alf@secardiologia.es (A.L. Fernández).


Reply to the Letter to the Editor

Reply to Fernández et al.

Michael J. Flynn*, Janet M. McComb, John H. Dark
Department of Cardiac Surgery and Cardiology,
Freeman Hospital, Newcastle-on-Tyne NE3 1DE, UK

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It is a pleasure to reply to the constructive comments of Dr Fernández and co-authors, which highlight current problems in cardiac electrophysiology. The term cardiac re-synchronization therapy (CRT) is increasingly utilised to define attempts at modification of cardiac electro-pathophysiology or aberrant conduction in order to improve haemodynamic performance. As indicated by Rosanio et al. [1], the exact indications for CRT, as well as technical application problems continue to pioneer groups in electrophysiology.

The aim of our study was to investigate the effects of LV pacing on haemodynamic performance compared to the standard practice of RV pacing. The origins of this study evolved from the evidence that much of the haemodynamic benefit of biventricular pacing is derived from the effects of pacing on LV function [2]. Our relatively small study demonstrated that with active lead placement on the LV lateral wall, i.e., posteriorly, a significant improvement in haemodynamics was achieved. Specific to Fernández and co-authors’ nomenclature criticism regarding split biventricular pacing versus LV pacing, this demonstrates a limitation in current definitions.

Similarly, we agree that some authors would have defined our method of LV pacing as biventricular pacing. Recent surgical studies have described separate active lead placement on both the RV and the LV [3,4]. We accept this author’s suggestion regarding alternate placement of the inactive lead. However, this questions the implications of the site of inactive lead placement, which is also a matter of debate.

Undoubtedly, the possible benefits of CRT upon myocardial dysfunction remain undefined. However, beneath the broad title of CRT, specific definitions of biventricular pacing and LV pacing are now required. Finally, we sincerely thank Dr Fernández and co-authors for this excellent contribution.

References


*Corresponding author. Tel.: +44 191 2843313; fax: +44 191 2226587. E-mail address: barradrum@hotmail.com (M.J. Flynn).


Letter to the Editor

‘Trivial tricuspid regurgitation’ — is the impact really trivial?

Pankaj Kumar Mishra*

Glasgow Royal Infirmary, 16 Alexandra Parade, Glasgow G31 2ER, UK

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