
To the Editor:

I have read with great interest the study by Castleberry et al concerning the therapeutic approach to the treatment of ischemic mitral regurgitation (IMR). The subject is quite complex with the documented lower number of randomized and nonrandomized studies. I believe that the authors have made a huge contribution on this subject, which may also affect guideline suggestions. However, I think there are a few issues that need to be discussed.

IMR, as we very well know, results mostly from the asynergy and dysfunction between the posterior and lateral left ventricular walls, and mostly does not improve after revascularization. Therefore, as outlined in the 2014 American Heart Association/American College of Cardiology Guideline for the Management of Patients With Valvular Heart Disease, severe IMR is a class IIa indication, level of evidence C for surgery. However, there is still no consensus over moderate and moderate-to-severe ischemic mitral insufficiency. The current study may highlight this empty area, but the design of the study may be a limitation while making this proposal. They stated that a patient in any of the groups stayed within that group even if there had been crossovers, which means a patient in the isolated coronary artery bypass grafting group may have received a mitral clip implantation because of progressing regurgitation, but still was evaluated in the isolated coronary artery bypass grafting group. There are no data regarding any progression of IMR in the groups without any surgical mitral valve intervention at first, whether they received additional therapy for IMR, or how their quality of life and New York Heart Association class were affected. Those parameters may strongly affect the fate of the patients, and their survival, as well.

The authors revealed that the decision for mitral intervention concomitant with coronary artery bypass grafting was mostly made when the degree of IMR was higher. So the classical question not yet answered comes: Is it the presence of a higher degree of IMR or is it the presence of mitral surgical intervention that affects the survival rates?

The answer is very strategic in this point of view, because it is always the easy way out to decide not to touch the mitral valve during coronary artery bypass grafting. It is the decision affecting the quality of life and the survival of the patients with IMR, and it is the decision not yet stratified in the literature including the current study. So, I think it is still early to make up our minds on the subject of IMR after reading this valuable study.

Disclosures

None.

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References


Letter by Durukan Regarding Article, "Surgical Revascularization Is Associated With Maximal Survival in Patients With Ischemic Mitral Regurgitation: A 20-Year Experience"
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