Author has nothing to disclose with regard to commercial support.

conclusion of Kim and colleagues² that the current standard of care supports the use of valve substitutes other than homograft aortic valves in the setting of an infected aortic valve or aortic root.

James K. Kirklin, MD
Division of Cardiovascular Surgery
University of Alabama at Birmingham
Birmingham, Ala

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THE MORE I LEARN THE LESS I KNOW

Reply to the Editor:

We thank our colleagues from the Cleveland Clinic Foundation for their interest in our study and acknowledge their expertise in this area specifically. They raise concerns about our conclusions and republish data from 2 early publications. We agree that both are important contributions, and lament choices regarding references necessitated by word limits; other important references were omitted as well.^{1,2} Based on these old data, the senior author (T.M.S.) must confess initial skepticism of findings such as Jassar's,³ which served as a stimulus for our study, as well as a twinge of disappointment in our own findings⁴ as the homograft

operation is such an elegant one. Still the data are what the data are, and a great deal has changed since 2002 – and even more since 1992. Precepts we held inviolate including prohibitions against prosthetic material in an infected field have proven mutable. Vascular surgeons now routinely replace infected aortic grafts with prosthetic grafts in situ, cardiac surgeons operate after shorter courses of antibiotics in endocarditis, and our medical colleagues treat prosthetic endocarditis more frequently with antibiotics.

So perhaps we should clarify our conclusions. We did not state that “there was no significant benefit to allograft use with regard to resistance to infection” or that “such surgery can safely and appropriately be managed in centers ... without access to or experience with allografts.” Rather we concluded that “no significant benefit to the use of homografts was demonstrable.”⁴ On this point, which may seem a fine one, we are actually in violent agreement. An important limitation to most surgical studies is that they are too small “to draw a firm conclusion.” The ability to refute the null hypothesis, as our colleagues highlight, depends critically on study size and event rate. A *P* value of more than .05 does not mean “there is no difference” but rather “there is at least a 5% likelihood that what appears to be a difference is due to chance.” No degree of statistical gymnastics can overcome this fundamental issue. Absence of proof is not proof of absence. If we failed in our conclusions to convey this uncertainty, we apologize and embrace this opportunity to clarify.

Why does this matter? Our colleagues raise the issue of appropriate centers in which such patients should receive care. We appreciate the sentiment that, in the best of all possible worlds, centers managing endocarditis should have allografts available as well as “surgeons experienced with complex aortic valve surgery.” But not every patient, particularly those in rural settings, has access to such centers. We have been struck by the late referral of patients from hospitals that perform cardiac surgery denied intervention because of “a lack of availability of homografts.” Perhaps the answer is regional referral, but in reality, at a population level, one must consider both potential excess mortality in less-experienced centers and mortality due to lack of access. That is why we stated that “lack of availability of homografts should not impede appropriate surgical intervention.”

Are homografts useful? Certainly. Necessary? Maybe not.

Arminster S. Jassar, MD
Joon Bum Kim, MD
Thoralf M. Sundt, MD
Massachusetts General Hospital
Boston, Mass

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