Authors have nothing to disclose with regard to commercial support.

risky to recommend otherwise. This is in addition to a devoted, skilled multidisciplinary IE team with advanced clinical assessment capabilities and surgeons experienced in complex aortic valve IE surgery.

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AORTIC VALVE HOMOGRAPHS: EXPERIENTIAL ATROPHY SPAWNS EVOLUTION OF STANDARDS

Reply to the Editor:

I read with interest the recent letter by Hussain and colleagues,¹ written in response to an article by Kim and colleagues,² which puts forth the assertion that variability in outcomes with homograft aortic valves for infectious endocarditis may relate to differences of expertise in the surgical implantation of homografts. As a longtime proponent of homografts and avoidance of foreign bodies (such as the prosthetic sewing ring of mechanical or bioprosthetic valves) in the infected aortic root, I am

sympathetic to their comments. The virtues of the homograft as a surgical tool include its relative resistance to infection^{3,4} (as nicely shown in the hazard curves in the Hussain correspondence), the acceptable durability in patients older than about 40 years, and its applicability in a wide array of geometric distortions of the aortic root after removal of infected tissue.⁵

Despite the hazard curves indicating a more favorable profile of susceptibility to infection (or reinfection),^{3,4} however, the analysis by Kim and colleagues² and those of others^{6,7} suggest that comparable freedom from reinfection can be achieved with alternative valve substitutes. It is worth noting that the two hazard plots in the Hussain correspondence are 14 and 24 years old.^{3,4} Without deprecating the value of these depictions, the current era (during the past decade) has seen an increased emphasis on more extensive excision of infected tissue^{7,8} and possibly improved antibiotic regimens. Thus these comparative hazard depictions may not completely reflect current experience.

This discussion is important because despite my personal preference (and that of Hussain and colleagues¹) for aortic valve homografts in the infected aortic valve or root, most practicing cardiac surgeons are not well versed in the technical nuances of homograft surgery. This in part follows a general movement of opinion away from the use of aortic homografts. They are now rarely used in many congenital heart surgery centers; homograft tissue is less often used for neonatal arch reconstruction, in part because of the known propensity for anti-HLA antibody formation with homografts (which confounds potential suitability for a future heart transplant). More attractive valved conduits are available for right ventricle-to-pulmonary artery reconstruction, conduits that have less tendency toward extensive calcification and are more suitable for subsequent percutaneous valve implants. In aortic root surgery for patients wishing to avoid chronic anticoagulation, aortic valve or root replacement with a stentless porcine valve prosthesis is a simpler procedure than homograft implantation.

An additional issue is the expense of aortic homografts (they are typically more than twice as expensive as other valve substitutes) and the cost of maintaining a supply of cryopreserved valves. Furthermore, if all patients with endocarditis were referred preferentially to centers with ongoing expertise in and support for homograft surgery, many if not most patients' families would suffer additional economic hardships in travel, lodging, and time away from home.

In summary, although my personal bias toward homografts for the infected aortic valve or root is concordant with the stated view of Hussain and colleagues,¹ I believe that available evidence is consistent with the

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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.

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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.

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