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LETTERS TO THE EDITOR

Scope

Heart welcomes letters commenting on papers published in the journal in the previous six months. Topics not related to papers published earlier in the journal may be introduced as a letter: letters reporting original data may be sent for peer review.

Presentation

Letters should be:

- not more than 600 words and six references in length
- typed in double spacing (fax copies and paper copy only)
- signed by all authors.

They may contain short tables or a small figure. **Please send a copy of your letter on disk**. Full instructions to authors appear in the January 1997 issue of *Heart* (page 89).

Should balloon angioplasty be used instead of surgery for native aortic coarctation?

SIR,—The editorial by Rao supporting balloon dilatation of native aortic coarctation is an idiosyncratic review of the literature.[1] There have been numerous papers published on the treatment of coarctation since 1980, but Rao seems to have used a preponderance of his own studies in support of his argument, to some extent excluding many of the more cautious papers. Five of the 11 references cited are Rao's own studies. Incidentally one of these is an abstract and one a case report. It is thus difficult to accept his assertion that "our results accord with the results of *other* workers", because he refers to his own publications or his analysis of the studies reported by others. A "review of published reports", none of which is quoted, is not an acceptable basis to draw the conclusion that "balloon angioplasty is effective".

Treatment in infants—Rao mentions, without referencing, his review from 1994[2] regarding studies published between 1980 and 1991. This includes 11 surgical studies consisting of 607 patients, nine balloon dilatation studies, and a study of his own consisting of a total of 75 patients. In a different approach to the same subject, in 1993, Johnson *et al* compared 18 surgical studies including 1189 patients with eight balloon dilatation studies including 57 patients.[3] This latter important analysis, which gives a slightly different slant to the story, has been overlooked by Rao. The initial mortality reported by Johnson *et al* was similar for both balloon dilatation and surgery; however, the rate of recoarctation after balloon dilatation was 57% compared with 19% in Rao's analysis[2] (see table). It is difficult to escape the conclusion that the analyses in these two important reviews have been performed by different methods, and the editorial neither explains these differences nor resolves the issues. Two recent studies conclude that balloon dilatation, though effective in the treatment of native aortic coarctation in older patients, may not be effective in neonates[4] or infants.[5] The editorial, therefore, would have carried far greater weight if a comprehensive and up to date comparison had been made in the group of neonates and infants in whom the greatest controversy exists.

Aneurysms—In his editorial, Rao reports aneurysms after balloon dilatation in 5–10% of patients but there are no references for this figure. In one of his reviews cited in the editorial, he reports the rate of aneurysms as ranging from 6% to 43%, with an average rate of 10·8%.[6] However, he references a surgical series, in which the incidence of aneurysms is reported to be 30%. On the other hand, there are numerous surgical papers that report much lower rates of aneurysm formation. Later in the editorial, Rao quotes data from a randomised study of surgery and balloon dilatation,[7] but he omits to reveal that aneurysms occurred only in the balloon dilatation group (20% incidence). Rao concludes that some aneurysms are due to balloon dilatation but most are probably due to "structural abnormalities of the aortic wall and/or our inability to deliver 'controlled injury'". It is impossible to deny that aneurysms formed after balloon dilatation and therefore are related to the procedure. The aortic wall response to and the stresses developed during balloon dilatation are unavoidable and must be major factors in the development of aneurysms. Unsupported statements such as those quoted sound like excuses for the technique.

We believe that balloon dilatation has an important role beyond the newborn period in patients with native coarctation. It is not, however, without risk of acute complications or recurrence of coarctation. Selective quotation of results and of older reviews does not produce a balanced editorial and thus an opportunity to clarify the issues and address controversies has been missed.

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1 Rao PS. Should balloon angioplasty be used instead of surgery for native aortic coarctation? *Br Heart J* 1995;74:578–9.
2 Rao PS, Chopra PS, Koscik R, Smith PA, Wilson AD. Surgical versus balloon therapy for aortic coarctation in infants ≤3 months old. *J Am Coll Cardiol* 1994;**23**:1479–83.
3 Johnson MC, Canter CE, Strauss AW, Spray TL. Repair of coarctation of aorta in infancy: comparison of surgical and balloon angioplasty. *Am Heart J* 1993;**125**:464–8.
4 Fletcher SE, Nihill MR, Grifka RG, O'Laughlin MP, Mullins CE. Balloon angioplasty of native coarctation of the aorta: midterm follow-up and prognostic factors. *J Am Coll Cardiol* 1995;**25**:730–4.
5 Mendelsohn AM, Lloyd TR, Crowley DC, Sandhu SK, Kocis KC, Beekman RH III. Late follow-up of balloon angioplasty in children with a native coarctation of the aorta. *Am J Cardiol* 1994;74:696–700.
6 Rao PS. Balloon angioplasty of native aortic coarctation. In: Rao PS, ed. *Transcatheter therapy in paediatric cardiology*. New York: Wiley-Liss, 1993;153–96.
7 Shaddy RE, Boucek MM, Sturtevant JE, Ruttenberg HD, Jaffe RB, Tani LY, et al. Comparison of angioplasty and surgery for unoperated coarctation of the aorta. Circulation 1993;87:793–9.

Compiler	Treatment method	Number of patients	Early deaths (%)	Late deaths (%)	Recoarctation (%)
Rao[2]	Surgery	607	13·5	12·8	11·4
Johnson *et al*[3]	Surgery	1189	12	10	14
Rao[2]	Balloon	75	7	4·2	19
Johnson *et al*[3]	Balloon	57	11	0	57

This letter was shown to the author, who replies as follows:

SIR,—I do not agree with the assessment of Qureshi *et al* that my editorial is an idiosyncratic review of the literature; if it is, it is not as idiosyncratic as the inappropriate critique of Qureshi *et al*.

References—Qureshi *et al* complain that I did not reference the studies of others. The studies of other workers were indeed extensively referenced in references 2 and 3.[1,2] If Qureshi *et al* had only taken time to examine these references, they would have found that 11 published reports—all that was published up to the time of those reviews—were referenced. The abstract that Qureshi *et al* complain about has since been published.[3] With regard to referencing the case report, Qureshi *et al's* critique is symptomatic of inappropriate criticism. The case report[4] was cited when I was suggesting use of umbilical arterial approach for balloon angioplasty in neonates in order to avoid femoral artery damage.

Treatment in infants—Qureshi *et al* quote Johnson *et al's* paper[5] and suggest that I did not consider that paper. Indeed, I was aware of this publication.[6] In my comparison,[2,7] I scrutinised *all* papers published between 1980 and 1991 (a total of 49 surgical papers and 11 balloon papers) and compared them. In an attempt to have comparable time periods during which both surgical and balloon interventions were performed, I examined the pooled results in infants (< 1 year old) who underwent interventions between 1979 and 1990. In contradistinction, Johnson *et al*[5] chose to look at surgical results of patients operated on between 1970 to 1991. With regard to the balloon group, they included patients who had balloon angioplasties between 1982 and 1990. In addition, Johnson *et al* did not include all balloon angioplasty reports published to that date; in my analysis, published in early 1993,[2] there were 11 balloon papers (12 including ours) whereas in Johnson *et al's* analysis,[5] also published in early 1993, there were only eight balloon papers. Although neither my nor Johnson *et al's* comparison from the published reports are ideal, Johnson *et al's* study is restrictive, did not use comparable time periods during which interventions were performed, and did not include all balloon angioplasty papers.

Now, with regard to the paper of Fletcher and colleagues, this was published in March 1995 whereas I submitted the editorial for consideration for publication on January 10, 1995. The addition of data on infants in this paper and that by Mendelsohn *et al* does not change overall results. Furthermore, previous papers from the same institutions which included a substantial proportion of more recent publications were incorporated in the comparison analysis.[2,7]

More importantly, it seems to me that Qureshi and associates missed the point I am making. I have never stated that recoarctation rate in neonates ($\leqslant 30$ days) and infants ($\leqslant 1$ year) is low. In our own study,[3] the recoarctation rate in neonates is similar to that reported by Redington. As I have emphasised since the very first report on balloon angioplasty published by me 10 years ago in *British Heart Journal*,[8] the important feature of balloon angioplasty in the neonate and young infant is that it produces abatement of symptoms of heart failure and hypertension and helps avoid immediate surgery. Should recurrence ensue, it can be treated by repeat balloon angioplasty[3] or even surgery, if one prefers, when the infant is stable and less acutely ill. Additional points of interest are (*a*) mortality with either balloon or surgical therapy is largely dependent upon the associated cardiac defects and not the type of intervention (surgery or balloon)[7] and (*b*) duration of hospital stay and mechanical ventilation and immediate complication rate are lower with balloon than with surgical therapy.[7]

Aneurysms—Unfortunately aneurysms can occur spontaneously, after balloon angioplasty (referenced extensively elsewhere[2]), and after surgery.[2 9] The addition of Shaddy's data to the other data, does not change overall incidence of aneurysms observed in either balloon or surgical groups. Qureshi states that I did not mention the aneurysms in Shaddy's study: this is clearly stated in the editorial, on page 570, left column, paragraph 2, lines 4 and 5.

Conclusion—Unlike Qureshi *et al*, I believe that balloon angioplasty has an important role in the management of sick neonates with aortic coarctation, especially if transumbilical route[4] can be used. In my opinion, a balanced editorial was written with careful consideration to all issues at hand and I continue to believe that the data indicate balloon angioplasty is an effective and safe alternative to surgical therapy of native aortic coarctation.

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1 Rao PS, Chopra PS. Role of balloon angioplasty in the treatment of aortic coarctation. *Ann Thorac Surg* 1991;**52**:621–31.
2 Rao PS. Balloon angioplasty of native aortic coarctation. In: Rao PS, ed. *Transcatheter therapy in pediatric cardiology.* New York, Wiley-Liss, 1993;153–96.
3 Rao PS, Galal O, Smith PA, Wilson AD. Five-to-nine-year follow-up results of balloon angioplasty of native coarctation in infants and children. *J Am Coll Cardiol* 1996; **27**:462–70.
4 Rao PS, Wilson AD, Brazy J. Transumbilical balloon coarctation angioplasty in a neonate with critical aortic coarctation. *Am Heart J* 1992;**124**:1622–4.
5 Johnson MC, Canter CE, Strauss AW, Spray TL. Repair of coarctation of aorta in infancy: comparison of surgical and balloon angioplasty. *Am Heart J* 1993:**125**;464–8.
6 Rao PS, Chopra PS. Reply—The jury is still out regarding balloon therapy for native aortic coarctation (letter). *J Am Coll Cardiol* 1994;**24**:1589–90.
7 Rao PS, Chopra PS, Koscik R, Smith PA, Wilson AD. Surgical versus balloon therapy for aortic coarctation in infants $\leqslant$ 3 months old. *J Am Coll Cardiol* 1994;**23**:1479–83.
8 Rao PS. Transcatheter treatment of pulmonary stenosis and coarctation of the aorta: the experience with percutaneous balloon dilatation. *Br Heart J* 1986;**56**:250–8.
9 Pinzon JL, Burrows PE, Benson LN, Moes CAF, Lightfoot WE, Williams WG, *et al.* Repair of coarctation of the aorta in children: postoperative morphology. *Radiology* 1991; **180**:199–203.

Imaging the thoracic aorta

SIR,—Few people would disagree with Dr Reid's conclusion that magnetic resonance imaging has replaced aortography as the reference standard for imaging patients with chronic aortic disease.[1] He also reminds us that aortic disease can be complex and that occasionally, some patients will require imaging by several techniques before management decisions can be made. Although the clinical presentation of acute dissection of the thoracic aorta can be variable, a significant number of patients present with a characteristic history and confirmatory abnormalities on clinical examination. Deciding how and where to image these patients in an emergency situation requires clear guidance to facilitate urgent potentially life saving surgery.

Our experience suggests that imaging these high risk patients in a non-surgical centre is slow and inaccurate and that most require repeat imaging before management decisions can be made.[2] We advocate that in unstable patients with a high clinical index of suspicion of dissection, medical treatment with intravenous β blockers and/or sodium nitroprusside should be started and that the patient should then be transferred immediately to the surgical centre for both diagnostic imaging and management. Patients with a low clinical index of suspicion of dissection who are in a stable cardiovascular state, should undergo prompt local investigation using a nominated non-invasive technique.[3]

Just as *x* ray gantry rotation has improved the accuracy of computed tomography (CT) scanning, the use of biplane and multiplane imaging has improved transoesophageal echocardiography (TOE) and many of the limitations of echocardiography suggested by Dr Reid are no longer valid. In expert hands spiral CT, magnetic resonance imaging (MRI), and TOE are each excellent imaging techniques.[4] Debate over the relative merits must now occur at a local level and each centre must decide which technique it will use before undertaking emergency surgery. This decision should be based on the available expertise and individual preference.

We have found that after TOE in patients with suspected dissection, repeat imaging using a different technique is rarely necessary to make management decisions.[2] With TOE the cost is minimal and there is no delay associated with patient transfer or assembling ancillary staff. The study can be performed rapidly in the cardiac care unit by one operator and during the study the patient remains accessible to medical and nursing staff. TOE provides detailed information about the morphology and physiology of a dissection including information about other associated complications such as aortic regurgitation and tamponade and these data are usually sufficient to plan optimal management.[2]

Technology will inevitably continue to improve the absolute accuracy of aortic imaging, but I believe that until each cardiac unit has its own dedicated thoracic imaging system TOE will continue to play a key part in the emergency management of patients with dissection of the aorta.

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1 Reid JH. Imaging the thoracic aorta. *Heart* 1996;**76**:3–5.
2 Banning AP, Ruttley MST, Musumeci F, Fraser AG. Acute dissection of the thoracic aorta. *Br Med J* 1995;**310**:72–3.
3 Banning AP, Masani ND, Ikram S, Fraser AG, Hall RJC. Transoesophageal echocardiography as the sole diagnostic investigation in patients with suspected thoracic aortic dissection. *Br Heart J* 1994;**72**:461–5.
4 Sommer T, Fehske W, Holzknecht N, Smekal A, Keller E, Lutterbey G. Aortic dissection: a comparative study of diagnosis with spiral CT, mutiplanar transoesophageal echocardiography and MR imaging. *Radiology* 1996;**199**:347–52

This letter was shown to the author, who replies as follows:

SIR,—I thank Dr Banning for his interest in my editorial and for his comments regarding the investigation of acute aortic dissection. There is certainly merit in the suggestion that all patients with a high index of suspicion of dissection should be imaged in a surgical centre and there is no doubt that patients with a type A dissection require urgent attention from a cardiothoracic surgeon. Unfortunately, for various reasons including the experience of the attending physician, the complexity of presentation, and coexisting disease, the clinical picture is frequently far from clear cut. Other complicating factors then come into play such as local imaging expertise, distance from a surgical centre, cost, and convenience. It is for these reasons that a broad perspective on imaging is necessary in any discussion of aortic dissection.

I wholly agree that in experienced hands transoesophageal echocardiography is a powerful diagnostic tool. It is, however, disappointing that Dr Banning should attempt to advance his thesis by quoting his retrospective study.[1] In this study TOE was carried out by four experienced echocardiographers, and a comparison made with CT performed on various machines of various ages by operators with various degrees of experience. This is the type of study that unfortunately has a tendency to cloud objective assessment of imaging techniques.

Dr Banning's supportive reference to the paper by Sommer *et al* is welcome because this prospective study convincingly demonstrates that there is no statistically significant difference between TOE, spiral CT, or MRI in the detection of acute aortic dissection, and it confirms that spiral CT has a clear advantage in detecting arch vessel involvement.[2] This paper also reinforces the contention that one of the main limitations of multiplanar TOE is the "strong dependence on the investigator's experience and the difficulty to accurately document pathologic findings for follow up studies". Sommer *et al* go on to state spiral CT is fast and easy to perform and is probably the least operator dependent imaging technique.

Finally, I am pleased to agree with Dr Banning that local expertise should be used to best advantage. However, with respect to a single imaging technique for what is a relatively common diagnostic dilemma, I find it difficult to promote any technique that relies heavily on an individual operator and is not readily available.

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1 Banning AP, Masani ND, Ikram S, Fraser AG, Hall RJC. Transoesophageal echocardiography as the sole diagnostic investigation in patients with suspected thoracic aortic dissection. *Br Heart J* 1994;**72**:461–5.