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
- 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 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. 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 analysis2 (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 notwithstanding 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 or infants.3,4 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%.5 However, he refers to 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 lower rates of aneurysm formation. Later in the editorial, Rao quotes data from a randomised study of surgery and balloon dilatation,6 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 developing 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 quoting of results of old 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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5. Mendelsohn MM, Tiedt TR, Crowley DC, Sandhu SK, Kocsis KC, Beekman RH III.

<table>
<thead>
<tr>
<th>Treatment method</th>
<th>Number of patients</th>
<th>Early deaths (%)</th>
<th>Late deaths (%)</th>
<th>Recoarctation (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rao1</td>
<td>Surgery</td>
<td>607</td>
<td>13.5</td>
<td>12.8</td>
</tr>
<tr>
<td>Johnson et al2</td>
<td>Surgery</td>
<td>1189</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>Rao2</td>
<td>Balloon</td>
<td>75</td>
<td>12</td>
<td>4.2</td>
</tr>
<tr>
<td>Johnson et al3</td>
<td>Balloon</td>
<td>57</td>
<td>11</td>
<td>0</td>
</tr>
</tbody>
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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 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 my review—were referenced. The abstract that Qureshi et al complain about has since been published.2 With regard to referencing the case report, Qureshi et al’s critique is symptomatic of inappropriate criticism. The case report3 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 paper4 and suggest that I did not consider that paper. Indeed, I was aware of this publication.6 In my comparison,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 my results in order to compare those who underwent interventions between 1979 and 1990. In contradistinction, Johnson et al5 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 1993,7 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 not revealed in my report, 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.27 More importantly, it seems to me that Qureshi and associates missed the point I
am making. I have never stated that recorac-
tation rate in neonates (<30 days) and
infants <1 year is low. In our overview,
the recoracation rate in neonates is similar
to that reported by Redington. As I have
emphasised since the very first report on bal-
loon angioplasty published by me 10 years
ago in Britain, the potential interest in the
improved results of balloon angioplasty
remains high and young infant in that is it produces abate-
ment of symptoms of heart failure and hypertension and helps avoid immediate sur-
ery. Should recurrence ensue, it can be
treated by repeat balloon angioplasty or
even surgery, if one prefers, when the infant is
stable and less acutely ill. Additional
points of interest are (a) mortality with either therapy is still dependent upon the associated cardiac
defects and not the type of intervention (surgery or balloon) and (b) duration of hospital stay and mechanical ventilation and immediate complication rate are lower with balloon than with surgical therapy.

Aneurysms—Unfortunately aneurysms can
occur spontaneously, after balloon angio-
plasty (referred extensively elsewhere2), and after surgery.3 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 men-
tion the aneurysms in his letter. Shaddy's study
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 newborns with aortic coarctation, especially if trans-
subbimal route1 can be used. In my opin-
ioin, a balanced editorial was written with
consideration to all issues at hand and I
believe that the data indicate that care-
ful balloon angioplasty is an effective and
safe alternative to surgical therapy of native aortic coarctation.

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Imaging the thoracic aorta

Sin,—Few people would disagree with Dr
Reid's conclusion that magnetic resonance
imaging has replaced aortography as the ref-
erence standard for imaging patients with
chronic aortic disease.1 He also remarks that
aortic imaging and the interpretation of
results depend upon the associated cardiac
morphology. Occasionally, some patients will require imaging by several techniques before ma-
agement decisions can be made. Although the clinical presentation of acute dissection of
the thoracic aorta may be variable, a sig-
ificant number of patients present with a
characteristic history and confirmatory
anomalies on clinical examination.

Deciding how and where to image these
patients is an important clinical require-
ment. Imaging the thoracic aorta can
be

used to guide surgical planning and in
patients with an acute dissection there may be
urgent attention from a cardiac surgeon.

Unfortunately, for various reasons
including the experience of the attending phys-
icians, the admission history, 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 sur-
gery center, cost, and convenience. For these
reasons that a broad perspective on imaging is necessary in any discussion of
aortic dissection. I wholeheartedly agree
with Dr Banning that the presence of an
aneurysm in the thoracic aorta should be
imaged using several modalities and Dr
Banning's supportive reference to the
paper by Sommer et al is welcome because
this prospective study at a single center
states 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 the assessment of complex
disease.2 This paper also reinforces the
contention that one of the main limitations
of multiplanar TOE is the "strong dependence
on the operator's experience and the diff-
culty to accurately document pathologic
findings for future follow-up studies". Sommer et al
have used spiral CT and TOE and I agree with Dr
Banning that local expertise should be used
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 Reid JH: Imaging the thoracic aorta. Heart 1996;76:3-5.
3 Banning AP, Masani ND, Ikram S, Fraser AG, Hall RJ, Transoesophageal echocardiography as the sole diagnostic investigation in patients with suspected thoracic aortic dissec-

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

Sin,—I thank Dr Banning for his interest in
my editorial and for his comment regarding
the investigation of acute aortic dissection.
There is certainly merit in the suggestion
that all patients with a high index of suspi-
cion of dissection should be imaged in a
surgical centre. This is the type of study
that unfortunately has a tendency to cloud objec-
tive assessment of imaging techniques.

Dr Banning's supportive reference to the
paper by Sommer et al is welcome because
this prospective study at a single center
states that there is no statistically significant
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