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.2 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 analysis3 (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 recognises 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.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 controversies exist.

Aneurysm—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%.4 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 lower rates of aneurysm formation. Later in the editorial, Rao quotes data from a randomised study of surgery and balloon dilatation,5 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 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 quoting of old and new 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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<table>
<thead>
<tr>
<th>Reference</th>
<th>Patients</th>
<th>Early deaths (%)</th>
<th>Late deaths (%)</th>
<th>Recurrence (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rao et al</td>
<td>Surgery</td>
<td>12 (14)</td>
<td>6 (8)</td>
<td>11 (4)</td>
</tr>
<tr>
<td>Johnson et al</td>
<td>Balloon</td>
<td>10 (10)</td>
<td>5 (7)</td>
<td>14 (14)</td>
</tr>
<tr>
<td>Rao et al</td>
<td>Balloon</td>
<td>57 (60)</td>
<td>11 (11)</td>
<td>57 (57)</td>
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5 Mendelson et al. Operated TR, Crowley DC, Sandhu SK, Kocis KG, Beekman RH III.

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 3.1,12 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 their letter—were referenced. The abstract that Qureshi et al complain about has since been published.12 With regard to referencing the case report, Qureshi et al's critique is symptomatic of inappropriate criticism. The case report 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 paper13 and suggest that I did not consider that paper. Indeed, I was aware of this publication.4 In my comparison,1 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 only the results of studies of infants who underwent interventions between 1979 and 1990. In contradistinction, Johnson et al13 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 1993,14 there were 11 balloon papers (12 including ours) whereas in Johnson et al's analysis,11 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.17

More importantly, it seems to me that Qureshi and associates missed the point I
am making. I have never stated that recoarcta-
tion rate in neonates (≤30 days) and in
faunting <1 year is low. In our own study,
the recoarctation rate in neonates is simi-
lar to that reported by Redington. As I have
emphasized since the very first report on bal-
loon angioplasty published by me 10 years
ago in Br Heart J, the most significant feature
of balloon angioplasty in the neonate and
young infant is that it produces abate-
ment of symptoms of heart failure and
hypertension and helps avoid immediate
surgery. Should recurrence ensue, it can be
treated by repeat balloon angioplasty1 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 highly dependent upon the associated cardi-
defects and not the type of intervention
(surgery or balloon)2) and (b) duration of hospital
stay and mechanical ventilation and immediate
complication rate are lower with balloon than with surgical therapy.3

Aneurysms—Unfortunately aneurysms can
occur spontaneously, after balloon angi-
plasty (referred extensively elsewhere4),
and after the administration 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 incidence of aneurysms. 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 neonates
with aortic coarctation, especially if tran-
subliminal route1 can be used. In my opin-
ion, a balanced editorial was written with
careful consideration to all issues at hand
and I hope that the data that indicate
Balloon angioplasty is an effective and
safe alternative to surgical therapy of native
aortic coarctation. P SYAMASUNDAR RAO

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Imaging the thoracic aorta

SIR,—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 reminds us
that aortic dissection is an emergency that
occasionally, some patients will require imaging by several techniques before man-
agement decisions can be made.2

The clinical presentation of acute dissection
of the thoracic aorta is 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 in
the acute setting 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
must 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 nitroprusside and/or sodium
nitroprusside should be started and that
the patient should then be transferred immedi-
ately to the surgical centre for both diagnos-
tic 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 tomographic (CT)
scanning, the use of biplane and multial
plane imaging has improved transesophageal echocardiography (TOE) and many of
the limitations of echocardiography suggested
by Dr Reid are no longer valid. In expert
dr hands spiral CT, magnetic resonance imag-
ing (MRI), and TOE are each excellent imaging techniques.4

Debate over the rela-
tive merits must now occur at a local level
and each centre must decide which tech-
nique it will use before undertaking emer-
gency surgery. This decision should be
based on the available expertise and individ-
ual preferences.

We have found that after TOE in patients
with suspected dissection, repeat imaging
using a different technique is rarely neces-
sary to make management decisions.5 With
TOE the cost is usually £300, however 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 infor-
mation about the morphology and physiol-
gy of a dissection including information
about other associated complications such
as thoracic aortic regurgitation and/or aortic
aneurysm. In expert hands these data are usu-
ally sufficient to plan optimal
management.6

Technology will inevitably continue to
improve the absolute accuracy of aortic
imaging, but currently each cardiac
center 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 thoracic aorta.

ADRIAN P BANNING
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1 Reid JH. Imaging the thoracic aorta. Heart 1996;76:3–5.
3 Banning AP, Masani ND, Ikrar S, Fraser AG, Hall RJF. Transesophageal echocardiography as the sole diagnostic investiga-
tion in patients with suspected thoracic aortic dis-
4 Sommer T, Pehuke W, Holzhört N, Smekal A, Hüttenbrink B, de M. A prospective study of 88 patients with thoracic aortic
dissection: a comparative study of diagnosis with spiral CT, multplanar transesophageal echocar-
diography and MRI imaging. Radiology 1996;199:547–52

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 suspi-
cion of dissection should be imaged in a
surgical centre but there is a 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 concludes that there is
a 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 each of these areas. However, ultrasound is a
powerful diagnostic test. It is, however,
disappointing that Dr Banning should attempt
to advance his thesis by quoting his retro-
spective study.1 In this study TOE was carried
out by four experienced echocardi-
ographers, and a comparison made with CT
performed on various machines of various
ages by operators with various degrees of
experience. Each of these studies has its
advantage. However, 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 rela-
tively common diagnostic dilemma, I find it
difficult 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.

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1 Banning AP, Masani ND, Ikrar S, Fraser AG, Hall RJF. Transesophageal echocardiography as the sole diagnostic investiga-
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