LETTERS TO THE EDITOR

The British Heart Journal welcomes letters commenting on papers that it has published within the past six months.

All letters must be typed with double spacing and signed by all authors.

No letter should be more than 600 words.

In general, no letter should contain more than six references (also typed with double spacing).

Limitations of transoesophageal echocardiography in patients with focal cerebral ischaemic events

Sir—In their study of patients with focal cerebral ischaemic events, de Belder and colleagues showed that transoesophageal echocardiography confirmed the findings of transthoracic echocardiography and detected additional abnormalities. In each case the transthoracic echocardiography was performed before transoesophageal echocardiography. The echocardiograms were interpreted by someone who had not been involved with imaging and who was blind to clinical information. Was the person who reported the transoesophageal echocardiogram blinded to the result of the transthoracic echocardiogram? Was the transoesophageal operator also blinded to the transthoracic result? A lack of blinding might mean that the transoesophageal operator would try harder to demonstrate a lesion he knew to be present and the transoesophageal reporter would look harder to find a lesion he already knew to be present from the transthoracic echocardiogram. While this would mimic the clinical situation it does not tell us anything about the relative sensitivities of the two techniques which can only be determined if the results from each technique are obtained entirely blind to the results of the other.

The main difference between transoesophageal and transthoracic echocardiography in this study was that transoesophageal echocardiography picked up more right-to-left shunts (23% versus 7%). Was this failure to demonstrate by transthoracic echocardiography two thirds of the shunts, which seem to be present, the result of their technique? Others using more aggressive protocols and transthoracic echocardiography have shown an incidence of shunts in healthy individuals similar to that found by de Belder et al with transoesophageal echocardiography.\(^1\) Alternatively could it be that the shunts found by de Belder et al by transthoracic echocardiography were larger (both anatomically and functionally) and therefore the ones most likely to have implications for aetiology of cerebral ischaemic events in their patients?

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These letters were shown to the author, who replies as follows:

Sir—Peter Wilmshurst reminds us about the care needed in determining the relevant sensitivities of different echocardiographic techniques. Some of our patients underwent both precordial and transoesophageal echocardiography at the same visit. The studies were recorded on video tape and were thereafter reported by the individual who was blinded to the clinical details of the patients. In all instances, the precordial study was performed first. Dr Wilmshurst is correct in stating that the interpreter of the transoesophageal studies would not have been blinded to the interpretation of the transthoracic result in these cases. We do not believe, however, that the interpreter tried harder to detect abnormalities in those patients in whom the transthoracic study had already demonstrated them.

We do not believe that poor technique led to the low transthoracic pick-up of right-to-left shunts in these patients. Indeed, four to six precordial contrast studies were performed before a patient was deemed to have no shunt. These were a mitral valve level M mode study and an apical four chamber or subcostal cross sectional study (and in many cases both), all of which were done before and during the Valsalva manoeuvre. We disagree with the suggestion that the less common larger shunts, related to what Thompson and Evans referred to as "pencil" patency (as opposed to "probe" patency) of the foramen ovale may be more likely to give rise to paradoxical embolic events. It may be that these are the shunts that are detectable from the precordium. These hypothetical thoughts are difficult to prove by the use of echocardiography in determining the size of a patent foramen ovale in vivo, nor of accurately determining the size of the associated (and variable) shunt.

The point of our paper, however, was not to give accurate data on the relative sensitivities of precordial and transoesophageal echocardiography in these patients. Given the drawbacks of precordial echocardiography that are overlapped with those of transoesophageal echocardiography, it should be no surprise to anyone that the transoesophageal technique detects more abnormalities. This has now been demonstrated in numerous studies. Our point was that the detection of the abnormalities is far more precise and that patients with low risk of paradoxical embolus may be no surprise with the major exception of right-to-left shunts. Moreover, until we have some proven treatments to minimise recurrence rates when a patent foramen ovale or spontaneous contrast echoes are detected, we believe that the detection of such abnormalities may be of little value for the patient.

Paul Tunick and Izhak Kronzon are wrong in assuming that we did not scan the aorta in our patients. We did—in all cases. We acknowledge their work on the possible relevance of raised thrombotic plaques in the aortic arch. However, in their initial series of three case reports,\(^1\) case 1 was in atrial fibrillation and they cannot be sure that the cerebral event was related to the aortic lesion. In addition, the figure showing the lesion in case 1 was taken from the distal part of the arch; it is difficult to see how an embolus from this area would lodge in a cerebral vessel. Case 3 had severe carotid disease as well as the aortic lesion.

Tunick et al in a more detailed case-control study\(^1\) provided rather more convincing evidence that raised thrombotic plaques in the aortic arch are associated with embolic
events. However, their study was limited by the inclusion of patients with peripheral embolic events, by the lack of data on the size, morphology, and exact position of the atheromatous plaques and because they compared their cases with controls with cardiac disease. In addition, the sensitivity of the transoesophageal technique for aortic arch disease is unknown. Their data were, however, suggestive of a genuine association between the aortic arch lesions and cerebral embolism, although they do not demonstrate cause and effect. We recently performed transoesophageal echocardiography on a 57 year old man with no obvious cause of a stroke, in whom the only abnormality was a protruding plaque in the middle part of the arch (fig). However, such a gross abnormality was not seen in the patients reported in our recent study. We did see evidence of much less dramatic aortic arch atheroma in a part of these patients, but not in enough of them to convince us of its pathogenic relevance, and usually the patient had clinical or carotid ultrasound evidence of diffuse atheromatous disease. We have seen other patients with a peripheral embolic event who had clear evidence of protruding and mobile thrombotic plaques in the descending aorta. We agree with Tunick and Kronzon that these abnormalities would not be detected by precordial echocardiography and are worth seeking. We still, however, have no data on optimum treatment for such patients. Although Tunick et al have, somewhat radically, removed a protruding plaque in one patient, this treatment cannot be recommen-
ded for all such patients.

Transoesophageal echocardiography did show interesting abnormalities in these patients that cannot be detected by conventional precordial echocardiography. However, it may not be possible to demon-
strate anything more than an association of certain abnormalities with clinical events. The accumulating data on the pathology of the foramen ovale, spontaneous contrast echoes, and atheromatous aortic disease do suggest a possible pathogenic role for these features in embolic events. We should now use this technique to evaluate treatments that might reduce the recurrence rate of embolic events in these patients.

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2 Tunick PA, Kronzon I. Protruding athero-
sclerotic plaque in the aortic arch of patients with systemic embolization: A new finding seen by transoesophageal echocardiography.

3 Tunick PA, Perez JL, Kronzon I. Protruding athero-
sclerotic plaque in the thoracic aorta and systemic embolization. Ann Intern Med 1991;115:
423–7.

von Willebrand factor in plasma: a novel risk factor for recurrent myocardial infarction and death

Sir—Raised concentrations of von Willebrand factor antigen (vWFAg), a specific product of the endothelium, reflect damage to the endothelium. Jansson et al [British Heart Journal 1991;66:351–5] reported that high concentrations of vWFAg are present in ischaemic heart disease and predict reinfar-
ction and mortality in survivors of myocardial infarction. Among the 123 survivors entering their study there were 88 cases of hyper-
tension, diabetes mellitus, and smoking. Furthermore, the mean cholesterol concentra-
tion was 7 mmol/l, indicating hyperchole-
terolaemia in an unspecified number of patients.

Jansson et al point out that increased concentra-
tions of vWFAg are found in diabetes mellitus, in the absence of sym-
ptomatic vascular disease, however, hypertension, hypercholesterolaemia, and smoking' also act independently to increase vWFAg. We therefore asked whether vWFAg would still be increased in ischaemic heart disease once these risk factors for atherosclerosis were considered.

We studied 41 non-diabetic patients (mean age 59) six weeks after myocardial infarction (proven by a raised creatine kinase over 130 units/l) with retrosternal chest pain typical or electrocardiographic changes or both. Twenty one of them said that they were current smokers, or smoking at the time of infarction, or had stopped in the year before infarction. Symptom free age and sex matched controls who were normotensive and normaloamicaemic were recruited from individuals attending hospital for endoscopy or repair of varicose veins. Resting systolic and diastolic blood pressures (SBP, DBP) were recorded, and blood was taken for routine measurement of cholesterol and vWFAg (by ELISA). For statistical analysis we used the Mann Whitney and Spearman's methods on Minitab. Both cholesterol (mean (SD): 6-3 (0-9) mmol/l, p = 0.0001) and vWFAg (130 (39) IU/dl, p = 0.0004) were higher in patients than in controls (5-2 (0-9) mmol/l and 104 (37) IU/dl respectively), confirming the work of Jansson et al. SBP and DBP were not increased in the patients with ischaemic heart disease but the values did correlate with vWFAg (SBP r = 0.34, p = 0.033; DBP r = 0.59, p = 0.0001). vWFAg did not correlate with cholesterol.

A subgroup of 20 patients (12 smokers) with ischaemic heart disease also had hyper-
tension (n = 10, blood pressure >140 and >90 mm Hg) or a raised cholesterol concentra-
tion (n = 10, >7 mmol/l) at re-examina-
tion six weeks after discharge from hospital. The remaining 21 patients (12 smokers) were free of these risks. When these two groups were compared we found that those with either of these risk factors had significantly raised blood pressure (136 (18)/84 (10) mm Hg) and cholesterol (6-8 (0-8) mmol/l); vWFAg was also higher (153 (35) IU/dl) than in the patients without these risks (107 (28),

p = 0.0002), which in turn was not higher than in the symptom free controls (figure).

Although vWFAg is generally raised six weeks after myocardial infarction, in many cases this may be because of hypertension or hypercholesterolaemia or both. The presence or absence of smoking did not influence vWFAg concentrations in any group. We suggest that survivors of myocardial infarction without risk factors for atherosclerosis may have minimal evidence of damage to their vasculature. Conversely, in those with risk factors the endothelial insult is main-
tained and such patients may be at risk of further cardiac events. It would be instructive to learn whether Jansson et al could confirm this by performing multivariate analysis in the patients in their study.

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

Sir—Blann et al report that in a case-control study they found higher concentrations of von Willebrand factor antigen (vWFAg) in survivors of myocardial infarction than in controls. Our study on survivors of myocar-
dial infarction was prospective and showed that high concentrations of vWFAg predicted an increased risk of reinfarction and cardio-
vascular mortality in univariate Cox regres-
sion analysis.
Limitations of transoesophageal echocardiography in patients with focal cerebral ischaemic events

Mark De Belder

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