Non-cardiac chest pain: a useful physical sign?

Editor,—Non-cardiac chest pain is a major problem in general practice, in outpatients, and on the wards. Some patients are “certain” that their pain is not cardiac but this is of course not the end of the matter. Professor Mayou and others1 have demonstrated that these patients are still in trouble at follow up. Some are given the label “musculoskeletal” but this is not very convincing without a clear explanation. I suggest that this diagnosis can be supported in many cases by demonstrating reproduction of the pain by passive spinal movements. Production of symptoms by passive movements is well known to orthopaedic specialists.2 Flexion, extension, lateral flexion, and rotation of the thoracic spine are the basic movements, and sometimes the position has to be held at the extreme for a least five seconds to reproduce symptoms—but I do not know whether the method of holding the position is not well known. Fifteen consecutive medical senior house officers and eight casualty officers were asked how they would demonstrate that chest pain was musculoskeletal and the best that they could do was percussion or direct pressure.

In six months of cardiology outpatients there were 27 patients (16 women and eight men) with a provisional diagnosis of angina whose history indicated another diagnosis. Their discomfort was either too unpredictable, too long or too short in duration, perhaps with paraesthesia in the fingers, or did not respond appropriately to antianginal drugs. One patient seemed to have oesophageal spasm and reflex and responded to appropriate treatment. One had pain reproduced by pressure on a costochondral joint. Of the remainder all but three had their discomfort reproduced in part or completely by passive spinal movements. Two patients also had their symptoms reproduced by percussion over the spine; both had vertebral collapse and insulin-dependent diabetes mellitus in a secondary deposit. Two patients said that they were pale and sweaty during the pain, and pallor and sweating were reproduced by passive thoracic rotation and relieved by return to normal; a concurrent ECG was unchanged. Seven patients had exercise tests, two of which were positive and they await catheterisation; their pain has not responded to antianginal medication so there is doubt that they have angina.

Of 20 consecutive patients with typical angina and positive exercise tests, only three had some discomfort in the chest on thoracic spinal rotation and they were clear that it was a different pain from their angina.

Review of the 27 patients with non-anginal pain at one year showed that seven had definite relief from non-steroidal anti-inflammatory drugs and one from physiotherapy. None had been admitted to hospital and those that still had pain seemed resigned to their symptoms but not anxious.

The exact origin of musculoskeletal pain remains obscure. It seems likely that the pain is referred along the intercostal nerves where it is exacerbated by inspiration, presumably from pain fibres.

Coronary disease and inflammatory spinal disease can co-exist and this physical sign does not exclude angina but its presence in the absence of any objective evidence of myocardial ischaemia—for example, no ECG changes during pain, is helpful in the differential diagnosis. Perhaps more important it demonstrates that the pain is mechanical, often allowing confident discharge without a backup medical plan. I have helped patient and giving the patient a fairly clear explanation of the symptoms, which does far more than negative tests to avoid chronic ill health.

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fibrillation frequency, none has demonstrated quality of life benefits. We accept that alternative atrial pacing sites and novel pacing algorithms may allow pacing to improve quality of life without the need for AV node ablation. With regard to the complications of AV node ablation cited, the mitral regurgitation reported in two patients by le Tourneau et al was moderate before ablation and became severe after ablation. Improved ventricular filling (achieved by good rate control) may well worsen mitral regurgitation and we advocate that patients with moderate mitral regurgitation undergo valve surgery before ablation, in patients with PAF this may well reduce left atrial pressure sufficiently to reduce the frequency of atrial fibrillation episodes. Possible deterioration of left ventricular function associated with long term right ventricular apical pacing has to be weighed against the possibility of patients developing tachycardia induced left ventricular dysfunction if they continue to be exposed to frequent prolonged episodes of rapid atrial fibrillation. Indeed, several studies have shown improvement in left ventricular systolic function after ablation and pacing for PAF. In addition, alternative sites for right ventricular pacing (or even biventricular pacing) may reduce the potential for long term left ventricular dysfunction.

In summary, we feel that either a one or two stage procedure is acceptable. A pragmatic approach might be to offer patients the choice of ablation and pacing at one sitting, which will improve symptoms but require pacemaker dependence, or a two stage procedure, which may avoid the need for ablation but may carry a second admission for symptomatic improvement. Given the high symptom burden of PAF patients being considered for ablation and pacing, we suspect many will choose the first option.

Management of polycythaemia in adults with cyanotic congenital heart disease

Editor,—I welcome Thorne’s editorial reiterating the pitfalls of overzealous venesection in young adults with cyanotic congenital heart disease. As she states there is now a body of opinion highlighting the detrimental effects of inappropriate venesection. The evidence these conclusions are based on, however, is sparse and retrospective. This will unfortunately not only be a feature of a relatively new field such as adult congenital disease practice until multi-centred collaboration and prospective studies are planned. Despite these limitations the work we have at present points towards the principles expounded by Thorne. A recent paper by Ammash and Warnes, not mentioned in Thorne’s editorial, provides further evidence regarding the lack of association between stroke and a high haematocrit. This study of symptomatic patients followed for 3135 patient-years did not identify an association between red cell mass and stroke. Of particular interest was the finding that iron deficiency and recurrent venesection were independent risk factors for cerebrovascular events. This study not only refutes the belief that we must treat haematocrits that are “too high” but positively demonstrates the dangers of venesection. In a recent statement from Perloff et al that venesection should only be performed for “temporary relief of significant, intrusive hyperviscosity symptoms”.

Unfortunately, in the UK this message is not getting through, even to cardiac specialists let alone general physicians or haematologists who may only occasionally come into contact with this patient group. In our recent survey of consultant cardiologists in Scotland, there was a great variation in practise regarding the perceived indications for and techniques of venesection. Only a minority of consultants caring for those patients appeared to be aware of the dangers of inappropriate venesection or the appropriate management of iron deficiency. Many patients were still being submitted to regular venesection for dubious indications with no fluid replacement and no monitoring for the onset of iron deficiency. In this specialist area, presenting simple guidelines such as those drafted by Thorne to a wider audience will hopefully redress this issue and improve patient care. It would be interesting to repeat a similar national survey of consultant practise in a year or so to determine whether this message is getting through.

Myocardial infarction in young people with normal coronary arteries

Editor,—Williams and colleagues recently described two patients in their early 20s presenting with acute myocardial infarction (AMI) in the absence of significant coronary artery disease. They conclude that the best investigation for subacute coronary events in young patients with AMI particularly, documentation of bridging during angiography may be inadequate and complete evaluation using intravascular ultrasound and Doppler is admissible.


Letters
block and is indeed recommended by the British Pacing and Electrophysiology Group. Maintaining AV synchrony with a physiological AV interval increases cardiac output both at rest and on exercise with normal and impaired left ventricular function. Therefore, the use of VVIR and VVI pacing is inappropriate for six of the 12 patients in their study who were in sinus rhythm with complete AV block.

DDD (or DDDDR) pacing would have been more appropriate in these patients; therefore, this study compares two suboptimal pacing modes. Dual chamber pacing would almost certainly have resulted in better exercise capacity. The loss of AV synchrony with VVI or VVIR modes probably also accounts for the heterogeneity of results assessing exercise capacity in this small group of patients. If dual chamber pacing had also been compared the results may well have shown an improvement in exercise capacity across the board. The results of the UK-PACE trial comparing VVI, VVIR, and DDD modes in higher degrees of AV block are awaited, but as with previous studies are expected to show that preservation of AV synchrony is the preferred mode on symptomatic, among other, grounds.

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This letter was shown to the authors, who reply as follows:

DDD is the best rate response system for subjects with normal sinus node function; compared with VVIR it offers improved quality of life and less pacemaker syndrome. The evidence that the haemodynamic improvement of DDD over VVIR automatically translates into an increase in treadmill exercise capacity is not so well established, and as such we do not accept the explanation of Somauoro and Connelly.

The purpose of our study was not to compare the benefits of various pacing modes, rather it was to use rate responsive pacing (in this case VVIR) as an instrument to compare the validity of various measurements of exercise capacity. It was methodologically unfortunate that some of our subjects were VVIR rather than DDD paced, but this was a reflection of the then accepted practice at the time when they had their original units implanted. The interesting observation we did make was that a treatment that is known to improve both symptoms and treadmill exercise capacity did not lead to an improvement in customary daily activity. This leaves us with the problem of divining the likely clinical benefit from the results of any study measuring treadmill exercise capacity. Just because someone can exercise harder in the laboratory does not mean that they will do so at home—but a quality of life questionnaire will show you if they do so with greater ease.


CORRECTION

Effects of reconstructive surgery for left ventricular anterior aneurysm on ventriculoarterial coupling (Heart 1999;81:171–76).

The incorrect Figure 5 was published in this paper. The correct figure and caption are shown below.

Figure 5 Pressure–volume loops in a single case before and after endoventricular circular patch plasty (EVCPP) repair. Straight lines indicate the Emax and Ea slopes. Emax increased slightly (from 1.3 to 1.63 mm Hg/ml.m⁻²), while Ea decreased (from 1.64 to 1.41 mm Hg/ml). SV did not change (from 69 to 71 ml), neither did V₀ (from −16.7 to 23.3 ml). End systolic pressure decreased (from 112.85 to 100.0 mm Hg). Note the different time of maximum systolic pressure before and after surgery.