Correspondence

Velocity dependent passive muscle stiffness

We refer to the interesting study by Lee et al concerning quantification of velocity dependent properties of the elbow flexors in patients with spasticity and rigidity.1 Their main finding was a velocity dependent increase in reactive torque in both groups, although this was only related to muscle length in subjects with spasticity.

However, the authors’ conclusion that their observations reflect stretch reflex hyperexcitability under gravity is difficult to understand, as is their assumption that the contribution of passive mechanical change, if any, should be minor and uniform during stretch. The investigators used online monitoring of muscle activity in biceps and triceps to ensure that the limb was relaxed before and during the test. The effect of a voluntary contraction or an “unrelaxed limb” on baseline torques derived from slow stretches (fig 3, p 622) might partly demonstrate the distortion of the torque angle curves that would have been evident if a reflex contraction of the biceps muscle had resulted from the faster stretches.

Thilmann et al have examined biceps stretch reflex responses in patients with spasticity secondary to stroke.2 Stretches of similar magnitude to those used by Lee et al invoked bursts of biceps activity with shorter latencies and shorter duration than those in age matched controls. Lee et al have avoided inducing activity in the biceps muscle during elbow extension.3 Therefore their findings are consistent with passive muscle elements, not stretch reflex responses, being responsible for the observed velocity dependent increase in reactive torque. This phenomenon has been reported previously in the calf muscles of normal subjects4 and of individuals with spasticity secondary to spinal cord injury.5

The data reported by Lee et al have demonstrated a velocity dependent stretch related behaviour of the elbow flexor muscles which is viscoelastic. The velocity dependent increase in reactive torque in both spastic and rigid muscles was significantly greater than in normal muscles, with the relation between reactive torque and limb position (muscle length) in spasticity differing from rigidity or normal tone. The most likely source of this velocity and length dependent resistance is alteration of the contractile component of the muscle, although connective tissue and noncontractile proteins within the sarcoplasmic cytoskeletons also contribute to passive muscle tension. An increase in the number of weakly attached actin–myosin crossbridges in relaxed muscle, and/or a slower than normal detachment rate during passive stretch, may contribute to hypertonia, especially in chronic cases.6 As crossbridge detachment occurs at a steady rate during stretch, such mechanical adaptations could account for some of the velocity dependent resistance observed in this study.

The importance of distinguishing between reflex and non-reflex sources of resistance to stretch lies in the need to target treatment interventions appropriately. An overemphasis on the contribution of stretch reflex excitability to motor disability in patients with spasticity may lead to an inappropriate treatment focus.

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References


Transoesophageal echocardiography: which stroke patients benefit most from this investigation?

In a recent article in this journal, Strandberg et al reported 441 patients who were investigated by transoesophageal echocardiography (TOE) after ischaemic stroke or transient ischaemic attacks. They looked for cardiac sources of embolism. They found that in patients who were in sinus rhythm and without any cardiac disease, 8% have been identified as having patent foramen ovale. The authors believed that only patients with patent foramen ovale were a frequent finding in this group of patients and are interested in the authors’ diagnostic and therapeutic approach in these patients with suspected paradoxical embolism. Our special interest in stroke patients with patent foramen ovale results not only from own experience but also from the recently published large prospective study in stroke patients with patent foramen ovale.7 This has not shown any differences in the recurrence rate of stroke in patients with patent foramen ovale between those treated with warfarin and those treated with acetylsalicylic acid.

In routine clinical practice, echocardiography is not only relevant to decisions about anticoagulant treatment in stroke patients, but also to other forms of medical treatment—such as ACE inhibitors or β blocking agents in left ventricular dysfunction accompanying dilated cardiomyopathy or left ventricular aneurysm, or antibiotic treatment in infective endocarditis. Furthermore, echocardiographic findings may prompt cardiac surgery, for example in patients with left atrial myxoma, aortic aneurysm, mitral or aortic stenosis, or infective endocarditis. Thus some of the major and minor risk factors for cardioembolic stroke8 are disorders which might require treatment other than by anticoagulant drugs. The authors do not indicate how often such therapeutic decisions, “beyond” anticoagulation, were prompted by echocardiographic findings. In conclusion, we agree that TOE is a useful tool in stroke patients to clarify diagnostic and therapeutic issues. The important question, however, as to which stroke patients should undergo TOE remains unresolved.

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**Author’s reply**

We would like to thank Drs Stöllberger and Finsterer for their interest and noteworthy opinions concerning our paper.

By “anticoagulation drug,” we meant only oral anticoagulation with warfarin. The contraindications to this treatment were allergy to warfarin, a bleeding tendency, hepatic or renal insufficiency, serious hypertension, endocarditis, alcohol abuse, pregnancy, and dementia.

The decision to start or continue anticoagulation after a stroke with a clear cardiac source of embolism is a difficult one and must be made after careful thought. When a major risk factor for a cardiac source of embolism was detected using transoesophageal echocardiography (TOE) and none of the contraindications mentioned above was present, the patient was given anticoagulant drugs. In cases with a minor risk factor for a cardiac source of embolism the decision to start anticoagulant treatment was based on the number and type of risk factors. In the study population, anticoagulant treatment was usually started within one to three days from the event, after haemorrhagic transformation had been excluded by brain imaging.

When patients who were in sinus rhythm and had no history of cardiac disease were evaluated as an independent group, anticoagulant treatment appeared to be based on TOE data in 22 of 286 patients (8%). In this group, 12 thrombi were found on TOE: three in the left atrial appendage, seven in the aortic arch, and two in the descending aorta. One patient was treated mainly for left ventricular wall motion abnormality. Nine patients were receiving anticoagulant drugs on the basis of a patent foramen ovale, and in all these cases right to left shunting was detected using a Valsalva manoeuvre. A patent foramen ovale was quite a common finding in our study group (67/441), but only 12 patients were receiving anticoagulant drugs on that basis. In our study we concentrated only on the decision to start anticoagulant treatment. We agree with Drs Stöllberger and Finsterer that both transthoracic echocardiography (TTE) and TOE give useful information with respect to medical treatment or cardiac surgery: TTE and TOE are useful tools in patients with stroke or transient ischaemic attack if the patients are candidates for active treatment.

**Reference**


**Author’s reply**

I am perplexed that Dr Horwitz should have concluded that my comments regarding “sentinel” bleeds were “offensive”. Having worked in Edinburgh for over a decade, I am well aware of Professor Gillingham’s reputation. Although I doubt many other readers will have similarly misinterpreted my words, I am happy to reassure Dr Horwitz that no disrespect was remotely intended, to Professor Gillingham or anyone else.

However, I stand by my message that the term “warning leak” should be abandoned. The prospective evidence now available, and quoted in my paper, indicates that most such “warning leaks” are probably the result of recall bias from previous retrospective studies. In some cases there may indeed have been a true subarachnoid haemorrhage that was unrecognised at the time (by either the patient or their medical attendants), although the study by Linn et al suggests these account for a minority—in which case let us be honest and call them “missed subarachnoid haemorrhage”, rather than “warning leaks”. The key educational point is that all doctors should appreciate the potential importance of a true sudden onset headache, and refer them accordingly to the appropriate unit for further investigations.

**Reference**

Transoesophageal echocardiography: which stroke patients benefit most from this investigation?
C Stöllberger and J Finsterer

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