Post-tubal-sterilization syndrome

Sir,

I am puzzled by the letter from Dr Boyd (June *Journal*, p.272), which suggests that tubal ligation causes menorrhagia. Dr Boyd says that 18.3% of his sterilized patients later had a hysterectomy, implying that this is a very high figure.

In fact, calculations which I made some years ago[1] suggested that 20% of all British women eventually have a hysterectomy.

Perhaps I am completely wrong, but I do think that we need a lot more evidence before we can start telling our patients that sterilization will make their periods heavier.

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Reference

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Medical Foundation for the Care of Victims of Torture

Sir,

During prolonged study leave I have had the opportunity of spending a small amount of time each week at the Medical Foundation for the Care of Victims of Torture.

My work has involved me in meeting victims of torture from many countries and interviewing them on behalf of refugee organizations. Sometimes this is to validate stories which may enable them to seek asylum in this country, more frequently to help them obtain necessary medical treatment in this country, but also to give them advice concerning specific medical or psychological problems. During recent months I have also been involved in organizing a small group for the victims of torture from several different countries, which has provided an interesting forum for the victims to share their thoughts and feelings about the past and talk about how their experiences affect them to the present day. I have found the work immensely interesting and rewarding.

The work depends in part upon the voluntary assistance of doctors and many are able to offer help either occasionally or on a regular basis. There is clearly a need for other doctors who feel that they might be interested in this work to contact the Foundation, where I am sure their offer will be most gratefully and sympathetically received.

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General practitioners and alternative medicine

Sir,

When Skrabanek and McCormick (May *Journal*, p.224) criticize Anderson and Anderson (February *Journal*, p.52) for their 'bias and beliefs', they reveal their own. In their letter they say nothing positive about alternative medicine and then give a series of one-sided comments.

Firstly they suggest diluting Anderson and Anderson's findings by excluding techniques such as manipulation and hypnosis because they are 'not alternative'. Why then did the British Medical Association examine them? Reference is then made to trials in 'reputable journals' showing that acupuncture is a placebo response yet the reader is not informed of equally reputable work suggesting the opposite.[1,2]

The reader is then guided to a 'detailed critique of homoeopathy' published in 1853 which is said to be unanswered. The answer to a theoretical critique is not more theory but scientific testing. There has been extensive homoeopathic research since then — good and bad — with one review citing over 250 studies.[3] Why was none of this mentioned and why was no reference made to the recent major study in the *Lancet*[4] which failed to find evidence in favour of the placebo hypothesis?

Finally they call on us to rid medicine of magic. Why do they wish to cloud the issue with accusations of magic? Let us stick to the facts, and when dispute arises put the matter to the test.

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4. Reilly DT, Taylor MA, McSharry C, Aitchison T. Is homoeopathy a placebo response? Controlled trial of homoeopathic potency, with pollen in hayfever as model. *Lancet* 1986; 2: 881-886.

Sir,

Skrabanek and McCormick's letter of criticism (May *Journal*, p.224) on the paper by Anderson and Anderson (February *Journal*, p.52) is itself hardly a model of academic impartiality, and as far as homoeopathic medicine is concerned, the only complementary discipline for which I am qualified to speak, it is ill-informed.

Skrabanek and McCormick make the point that only two doctors in the survey population of 222 practice homoeopathy and that there is no evidence that doctors wishing to study have difficulty in finding teachers. But there is no focus for the teaching of homoeopathic medicine in Oxfordshire. In Bristol alone, where there is, at least eight general practitioners use homoeopathy and 15 to 20 of those from the region regularly attend monthly clinical meetings. Sixty general practitioners attended the last symposium in Bristol, 24 are currently booked for a course in Cardiff and another 20 for a course in Plymouth. So where there are provisions for training in homoeopathy there is evidently an appetite. The problem is the scarcity of provisions, not only for doctors who wish to train and provide homoeopathy for their patients, but for patients whose doctors cannot fulfil their legitimate expectations in this respect and who have nowhere to go. And such problems are not of limited relevance. In a recent sample week of data collection, two of the most experienced general practitioners in Bristol gave homoeopathic medicine in 80% of over 200 surgery consultations.

Apart from the actual use of 'complementary' therapies, the Andersons reported a considerable proportion of doctors discussing the matter with patients or referring them to other practitioners (41.0% and 18.0% respectively in the case of homoeopathy). This level of patient enquiry and doctor response demands an adequate understanding of the subject. Skrabanek and McCormick ask 'what harm can ensue from homoeopathy without 'recognized' training?' They should read the article on severe cutaneous reactions to alternative remedies,[1] and the reply discussing the myth of harmlessness.[2] They would be enlightened by the reference to aggravation of symptoms in the trial of

homoeopathic potency versus placebo[3] by Reilly, a College member, in a paper which emphatically contradicts their assertion that critiques of homoeopathy remain ignored and unanswered. Critiques, incidentally, which are to be found in the increasingly self-critical pages of contemporary homoeopathic journals, as well as in the reference (was the date, 1853, really correct?) that they quote.

The proper relationship between doctors' interest in complementary therapies (which might not be unrelated to patient advantage), their role in primary care, already discussed in this *Journal*,[4] and appropriate provision of training does need to be carefully assessed, but the comment that 'The interest ... is not a sufficient reason for teaching them how to earn money by employing it' better befits the hustings than an academic department of general practice. Argument that is casuistic and sometimes economical with intellectual honesty is the bane of debate on this issue, among all parties. We are capable of better, and our patients certainly deserve better.

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Post-viral syndrome

Sir,

The review of the post-viral syndrome by Dr Archer (May *Journal*, p.212) undoes the (minimal) progress made for reasonable treatment of myalgic encephalomyelitis patients in the last 30 years. It is absurd to write of 'a balanced view' as though such a view would be in some way reasonable because it was a fine British compromise. We are dealing with facts, and with people's lives — to talk of 'a balanced view' is unscientific and perpetuates a wholly unnecessary cruelty.

A hundred years ago there would have been no problem for myalgic encephalomyelitis patients. No laboratory tests existed and diagnoses would have had to be made on case histories and observation. Doctors would have been required to believe what their patients were (repeatedly) telling them. In fact the problem of myalgic encephalomyelitis probably did not exist a hundred years ago because it is almost certainly a man-made illness with a variety of causes including stress, pollution of all our foods and the over use of medically administered drugs. There is no reason to think that any virus implicated is other than a trigger factor to an already compromised immune system.

Until the medical profession relinquish their obsession with drug treatment, with so-called psychiatric illnesses and with laboratory testing we are unlikely to see any progress and thousands will continue to suffer unrecognized and untreated from what is clearly a physical illness.

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Sir,

While there is some merit in Dr Archer's review of the post-viral syndrome (May *Journal*, p.212) the overall effect may well be harmful since he gives some credence to the view that the illness is not organically based. Most doctors would now accept that in any illness involving the central nervous system, neurological and psychological symptoms are so inextricably mixed that they cannot easily be differentiated. Of course a sufferer from any chronic illness needs psychological support particularly if his symptoms include depression and his illness runs a relapsing course or leads to permanent disability. But I would maintain that the harm done in labelling a patient with an organic illness as hysterical far outweighs any possible damage caused by calling hysterical symptoms organic. After all, the diagnosis in early cases of multiple sclerosis is in many cases made solely on the history and in the absence of positive physical signs or abnormal tests.

I would take issue with Dr Archer's 'evidence for hysteria' on several grounds. First, cases were occurring in the general population before the Royal Free Hospital outbreak. Secondly, three epidemics have occurred among men in military barracks. Thirdly, among the neurological findings in the Middlesex Hospital epidemic which the psychiatrists attributed to overbreathing, five patients had extensor plantar responses and three had ocular paralysis with double vision. Similarly, 20% of the cases admitted to Lawn Road Infectious Diseases Unit in 1956 from north west London had extensor plantar responses.

Dr Compston and colleagues[1] have pointed out that 'while a diagnosis of hysteria had been seriously considered at the time of the outbreak, the occurrence of fever in 89%, of lymphadenopathy in 79%, of ocular palsy in 43% and of facial palsy in 19% rendered it quite untenable'.

Dr Poskanzer of the Department of Neurology at Harvard Medical School summed up my feelings when he wrote

'The articles of Drs C.P. McEveady and A.W. Beard are of considerable concern. Their erroneous conclusions about this illness may impair future investigations of similar outbreaks. It is apparent that the authors failed to do their homework and demonstrated a surprising lack of information about the principles of epidemiology and of psychiatry. It is clear that sporadic cases of this disease cannot be readily identified. It is only in the epidemic form that the distinctive epidemiological features allow characterisation. Instead of ascribing ME to mass hysteria or psychoneurosis, may I suggest that the authors consider the possibility that all psychoneurosis is a residual deficit from epidemic or sporadic cases of ME'.[2]

I would only add that modern science may well prove him right.

General practitioners who are interested in this syndrome would do well to read a recent book by A.M.Ramsey.[3]

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2. Poskanzer DC. Epidemic malaise. *Br Med J* 1970; 1: 420-421.
3. Ramsey AM. *Post-viral fatigue syndrome: the saga of Royal Free Disease.* London: Gower Medical Publications, 1987.

Sir,

Dr Archer (May *Journal*, p.212) suggests that a balanced view of the post-viral syndrome must combine the hypotheses that it is an organic illness and that it is hysterical in nature. There is no doubt that some cases of the epidemic form are hysterical, as are some cases in any epidemic, but that is no reason to accuse sufferers with the condition of functional overlay.

The diagnosis of post-viral syndrome is made on a history of excessive muscle fatiguability with prolonged time to recovery following an acute viral illness. These patients also all complain of psychological symptoms, including tiredness, lack of concentration and poor