Letters to the Editor

CHRONIC FATIGUE SYNDROME

Sir, The above article purported to be a review of the current state of knowledge on chronic fatigue syndrome and possible underlying pathogenesis.[1] As a review it failed in its objectives. This was a very one-sided paper which barely mentioned that there is a large body of opinion as well as published literature which suggests that chronic fatigue syndrome has much more of a psychological than a physical basis. Looking through the 89 references there are many which feature the names of Behan. However there are absolutely no references to the seminal observations of Professor Simon Wessely and his colleagues at King's in London. The text of the paper focuses very much on the proposed physical/post-viral hypothesis and there is very little discussion of psychology or psychiatry (except on p152 in which the link is fairly briefly dismissed).

Were I to have no previous knowledge of chronic fatigue syndrome or fibromyalgia, I would read this review and automatically assume that this was a post-infection problem characterised by abnormal haematological, biochemical and immunological investigations. However, many patients with chronic fatigue syndrome have a faulty system of belief and demonstrate clearly abnormal illness behaviour. By focusing on a viral aetiology the chronicity of fatigue may well be extended iatrogenically. Assuming that the *Proceedings* are peer-reviewed I am surprised that the referees failed to comment on the one-sided nature of this alleged review.

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Sir, The pessimistic view of the treatment of chronic fatigue syndrome (CFS) taken by Professor Behan and his colleagues is both sad and surprising.[1] It is sad because the reviewers do not mention the evidence from randomised controlled trials suggesting benefit from some combination of graded activity and/or cognitive behaviour therapy.[2] It is surprising because the authors report that they have found no benefit from evening primrose oil in the treatment of CFS. Is the Professor Behan who reports this negative experience related to the Professor Behan who found that 80% of patients showed substantial improvement three months after the end of a randomised controlled trial of the same treatment, compared to only 18% of patients receiving placebo?[3] Whom do we believe?

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Sir, We have read carefully Dr Reilly's comments regarding our recent review on chronic fatigue syndrome (CFS).[1] Few people now, certainly those with clinical experience of the illness and elementary understanding of neuroscience, would hold the view that CFS is a primary psychological disorder. To be sure, some patients with CFS may have atypical depression and patients with depression may be mistaken because of overlap of some symptoms of CFS and, indeed, for these reasons, we collaborated in most of our previous studies with senior psychiatric academic colleagues. The presence, however, of psychiatric symptoms is common in many physical disorders and particularly so in disorders with a neurological aetiology. The mere absence of a diagnostic marker of an illness in no way makes the disease psychogenic. These accruing data favour an organic basis for CFS and can suggest that it may belong to the newly-described channelopathies.[2]

Dr Reilly's assumption that we suggested CFS 'was a post-infectious problem characterised by abnormal haematological, biochemical and immunological investigations' reflects a lack of proper reading of our review since we do not promote this message anywhere. Also, his attempts to equate CFS with fibromyalgia is incorrect since these two disorders are not synonymous despite sharing some common symptoms.

Professor Wessley and Dr Hotopf should not be too saddened by our view regarding treatment. To the best of our knowledge, there is no cure for CFS. However, certain forms of therapy may halp and indeed whilst we have no experience of graded activity or cognitive behaviour therapy, these remedies may certainly benefit a subgroup of CFS with predominantly psychological disorders. Our earlier experience with high doses of essentially fatty acids showed that patients claimed to be improved but no-one was cured.[3] Certainly our experience has been that high doses of essentially fatty acids may help cardiac palpitations and other non-specific symptoms such as profuse sweating, without having any substantial effect on fatigue.[3] Indeed, there is some experimental data to show that high dietary fatty acids may abolish the ischaemic arrhythmias induced in animals.[4] How such essential fatty acids may work is unknown but there is evidence that they can alter ion channels.[5,6] Our search to date for treatment has shown that small doses of amytriptyline together with amantadine affect the best amelioration of symptoms and may even have a beneficial effect on fatigue.

We felt that papers written by psychologists or psychiatrists on CFS may have a bias towards patients with a psychogenic aetiology and, in fact, may not be the same patient population which we ourselves are familiar with. The currently accepted definition of CFS excludes psychiatric disorders. Psychiatric symptoms in CFS are merely a reflection of the lesion and type of dysfunction that occurs within the central nervous system. We know, for example, that patients poisoned with ciguatera toxin, producing a known sodium channel disorder, may develop classical CFS.[2] Similarly, patients exposed to botulinum toxin, again causing a channelopathy, may also develop CFS.[2]

The development of a rational mode of therapy and future research, whilst recognising that psychological symptoms may definitely occur, has to be based on the acientific knowledge which has accrued. To quote from the most senior scientist involved for the longest time in studying CFS, ProfessorAlexis Shelokov, what is needed is 'reproducible laboratory procedure to allow objective, non-perceptual definition of cases belonging to at least one varient of the syndrome of chronic fatigue. Once that is accomplished, it will become possible to differentiate other varients of the syndrome and define them as to aetiology, epidemiology and pathogenesis.'[7] This is the view we strongly adhere to.

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LESSONS FROMA SYMPOSIUM ON MOVING POINTS IN RESPIRATORY MEDICINE

Sir, In the management of patients presenting with a solitary pulmonary nodule (SPN), the latter a topic of discussion in the above symposium, the greatest challenge is to reduce the resection rate for benign lesions, given that 96% of nodules selected in a radiological screening proved to be non-malignant.[1] According to two authoritative reviews, 50-60% of resected SPNs prove to be benign,[2,3] and this percentage already excludes nodules presumed benign by radiographic appearance.[3] The implication, therefore, is that a clinical decision-making process which generates a positive predictive value of only 50% for the diagnosis of malignancy must be fundamentally flawed, and many patients with benign lesions are subjected to inappropriate diagnostic lung resection. The nested polymerase chain reaction for detection of *Mycobacterium tuberculosis* in material obtained by fine needle aspiration from SPNs,[4] improves the non-operative diagnostic rate for infective granulomas, which comprise the largest or, at the very least, the second largest subgroup of benign SPNs,[5,6] enabling non-calcified tuberculomas to be distinguished from malignant nodules.[7] Clinical decision analysis, including cost-benefit analysis, indicated that open lung biopsy is the preferred strategy in surgically fit candidates aged >30.[8]

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