

Chronic Fatigue Syndrome: Editorial Bias in the *British Medical Journal*

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ABSTRACT. A literature search identified all papers published on chronic fatigue syndrome (CFS) and myalgic encephalomyelitis (ME) in the *British Medical Journal* between 1995 and 2000. Analysis of the findings revealed a bias towards the views of one school of thought and a lack of papers on the immunological or virological aspects of CFS. This contrasts with the mainstream American journals, which generally covered a much wider range of subjects and views. We examine the arguments for and against covert editorial policies, and summarise the results of discussions with the relevant individuals and organisations. [Article copies available for a fee from The Haworth Document Delivery Service: 1-800-HAWORTH. E-mail address: <docdelivery@haworthpress.com> Website: <<http://www.HaworthPress.com>> © 2004 by The Haworth Press, Inc. All rights reserved.]

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INTRODUCTION

Chronic fatigue syndrome (CFS) is a common, potentially disabling illness which carries a substantial socio-economic burden (1). The true prevalence of this disorder is unknown but may have been underestimated in the past (2). Without doubt, it deserves serious consideration both from research scientists and physicians in clinical practice.

The *British Medical Journal (BMJ)* is the official organ of the British Medical Association, the largest professional body of physicians in the UK. Readers expect the journal to publish original research on CFS and to keep them up-to-date with developments documented elsewhere. The publication of a broad range of views allows practitioners to make informed decisions and is an essential part of the scientific process. For clinical and epidemiological purposes, patients with CFS are currently defined by the criteria which were developed by the International Study Group for the Centers for Disease Control and Prevention (CDC) in 1994 (3).

In September 2000, we reviewed the content of the publications on CFS which have appeared in the *BMJ* since 1995 to see if the nature of the papers reflected the global research, clinical opinion and changes in the diagnostic approach in relation to this condition.

METHODS AND RESULTS

A search of MEDLINE for publications in the *BMJ* between January 1995 and August 2000 on CFS and myalgic encephalomyelitis (ME) identified 41 publications. They included 6 original papers, three editorials, one review, a case history, a book review and a number of letters. A similar analysis using the *BMJ*'s own search engine plus an independent database identified three additional items, all written by the *BMJ* editorial staff. To analyse the content of the articles and the diagnostic criteria used by the authors in case selection, we targeted all original research papers (including short reports), editorials and review articles identified by this search. There were six papers featuring original research (4-9).

Not a single paper used the latest CDC criteria for case selection. Where reported, the definitions used for case selection were the Oxford guidelines (10) though one paper was unclear but noted that their patients also fulfilled "the more restrictive" criteria for neurasthenia (6). This implies that they used the Oxford guidelines.

Only one report of original research included findings inconsistent with a psychiatric explanation (4). One challenged the adequacy of a

measure to assess abnormal illness behaviour (5) while two supported the management of CFS using cognitive behavioural therapy (CBT) and/or graded exercise (7,8). In a paper which confirmed previous observations of altered neuroendocrine control in CFS, the authors concluded, without any direct experimental evidence, that the abnormal responses might be the result of prolonged inactivity or a disturbance of the sleep-wake cycle (6). Moreover, a short report surveyed attitudes to ME, thought to be a subgroup of CFS, but the authors misrepresented the illness and in their discussion, reinforced the negative stereotype of patients who have the condition (9).

The only review during this period was an extract taken from Clinical Evidence. All the authors were mental health professionals (11). There were also three editorials on the illness (12-14). One was written by two psychiatrists (12). Another dealt with childhood CFS and was written by a pediatrician (13). The third, a commentary on the Royal Colleges Report, was written by a virologist (14). All three editorials expressed views consistent with a psychiatric explanation of CFS. The book review covered a text co-authored by two of the aforementioned psychiatrists and was highly complimentary (15). However, given that it was written by a former colleague of one of the authors (not declared), this is perhaps not surprising.

To see if the *BMJ* had published a greater proportion of articles on the psychiatric aspects of the illness, we compared the original papers, reviews and editorials with those published in comparable American journals. The analysis of the contents in the American journals can be found in Appendix 1. The frequencies of biomedical/non-psychiatric and psychiatric papers were compared using Fisher's exact test (two-sided), which is appropriate for 2×2 tables where the numbers per cell are low.

As shown in Table 1, the *BMJ* published fewer articles which focused on neurological, immunological and other biomedical aspects of the illness (4,6). In terms of the number of psychiatric papers which focused predominantly on psychiatric factors or speculated that a psychological or behavioural factor played a major role in aetiology and recovery, there were significant differences between the *BMJ* and the American journals, except for the *Annals of Internal Medicine*.

An analysis on the number of papers dealing predominantly or solely with psychiatric factors (5,7,8,11,12) also revealed significant differences between the *BMJ* and comparable American journals when pooled, and with the *American Journal of Medicine* when assessed on its own (see Table 2).

TABLE 1. Non-psychiatric and psychiatric articles published in *BMJ* compared to *Am J Med*, *Ann Int Med*, *Arch Int Med* and *JAMA* between 1 Jan. 1995-31 Aug. 2000.

Journal(s)	Articles				<i>BMJ</i> vs. Journal(s)
	Non-Psychiatric		Psychiatric [#]		
	N	(%)	N	(%)	
<i>BMJ</i>	2	(20)	8	(80)	n.a.
<i>Am J Med</i> , <i>Ann Int Med</i> , <i>Arch Int Med</i> , <i>JAMA</i>	38	(84)	7	(16)	P = .0002
<i>Am J Med</i>	27	(82)	6	(18)	P = .0007
<i>Ann Int Med</i>	1	(50)	1	(50)	P = .4545
<i>Arch Int Med</i>	6	(100)	0	(0)	P = .0070
<i>JAMA</i>	4	(100)	0	(0)	P = .0150

[#]Broadly-defined, see text

TABLE 2. Pro-psychiatric and other articles published in *BMJ* compared to *Am J Med*, *Ann Int Med*, *Arch Int Med* and *JAMA* between 1 Jan. 1995-31 Aug. 2000.

Journal(s)	Articles				<i>BMJ</i> vs. Journal(s)
	Pro-Psychiatric		Other		
	N	(%)	N	(%)	
<i>BMJ</i>	5	(50)	5	(50)	n.a.
<i>Am J Med</i> , <i>Ann Int Med</i> , <i>Arch Int Med</i> , <i>JAMA</i>	5	(11)	40	(89)	P = .0117
<i>Am J Med</i>	4	(12)	29	(88)	P = .0204
<i>Ann Int Med</i>	1	(50)	1	(50)	P = 1.0000
<i>Arch Int Med</i>	0	(0)	6	(100)	P = .0934
<i>JAMA</i>	0	(0)	4	(100)	P = .2208

COMMENTS AND DISCUSSION

Between 1995-2000, none of the published papers on CFS used the currently accepted international diagnostic criteria to define their patient population. Nor did they use the older CDC definitions of 1988 and 1992. Researchers tended to prefer the Oxford guidelines developed in the UK

(10). These can be used to select two groups of patients: those with chronic physical and mental fatigue and those with a more specific post-infectious fatigue syndrome (PIFS). However, none of the papers in question mentioned considering patients for the latter. Since the Oxford guidelines for chronic fatigue are less specific than all the other published criteria, the conclusions of these papers may not be applicable to every patient suffering from CFS.

Most editors of medical journals respect the need for balance in the content of papers, particularly on controversial topics such as CFS. However, in relation to CFS, the editors and reviewers clearly leaned towards the psychological and psychiatric aspects during the period in question. The fact is that most of the papers in the journal emphasized the role of inactivity, mood disorders and/or maladaptive beliefs. The paper by Lane et al. was the only report focusing on the possible role of non-psychiatric influences on fatigue in CFS during this time (4). There were no papers on the immunological or virological aspects nor any item referring to new research on these topics published elsewhere. We believe that the result has been to give readers the impression that the majority of patients with CFS are suffering from phobic avoidance and following unhelpful advice.

Responding to earlier suggestions of bias, the editor had claimed that: “We don’t consider ourselves to be pushing any theory: We are simply sorting among the 5000 papers submitted to us to find the best” (16). However, this is hard to believe, given the inadequacies in the various papers supporting the psychiatric view. For example, Sharpe et al. (7) revealed in their report that the patients who were going to receive CBT spent more than twice the amount of time in bed compared to the comparison group, despite having the same level of disability and fatigue. This and one other finding suggest that the researchers had inadvertently included more people with psychological problems in the treatment arm, and this alone could explain their favourable response to CBT. The paper on graded exercise was similarly flawed in terms of patient selection and did not provide any long-term follow-up (8).

The claim to publish ‘best evidence’ is also difficult to reconcile with the uncritical review of treatments (11). Firstly, this overlooked a number of relevant trials in order to support their contention that CBT was “effective” and that graded exercise produced “substantial improvements.” Secondly, it disregarded notable flaws in the ‘successful’ trials, such as the fact that the only symptoms assessed were fatigue and emotional distress and that all had included patients with psychiatric disorders; a subset likely to respond to CBT and exercise (7,8). Thirdly, the review failed to

note that the follow-up of Sharpe et al. had not confirmed the initially reported differences between the groups. In our view, this paper was partial, biased and clearly misleading.

Another example is the editorial which suggested that many doctors still advocate the “rest cure” and implied that most patients take that advice (12). The fact is that the strategy described has not been recommended in relation to the clinical management of CFS in any medical journal in the past two decades, nor has it been advocated by the two national patient groups during this time. The most commonly used coping strategy is pacing, but this was not mentioned here, or in the review.

Sadly, errors are not always corrected, nor flaws discussed. Although multiple criticisms and shortcomings of the review of treatments were reported in the electronic form of the journal (eBMJ), none were published in the paper version despite the promise made by the *BMJ* to a colleague. Moreover, in a personal communication to another colleague, the editor of Clinical Evidence accepted that there were shortcomings in the review but this was not made known to the readers of the *BMJ*.

Further evidence of editorial bias can be found in articles written ‘in-house.’ In the past, one of the *BMJ* editors revealed his support for the psychiatric explanation and reinforced the negative stereotype of the CFS patients by writing a factually incorrect commentary (18). For instance, he claimed that “supporters” of the illness had manipulated the World Health Organization (WHO) and persuaded them to “include myalgic encephalomyelitis under diseases of the nervous system” in the latest revision of the International Classification of Diseases (ICD-10). This is simply untrue, as a call to the WHO would have confirmed. He was also incorrect when he claimed that the “supporters” had “landed a Myalgic Encephalomyelitis Act on the British statute books, requiring an annual report to be made to parliament on its causes, effects and treatment.” There is no ME Act in the existing British statute books.

In his response to our analysis, which was rejected for publication, the editor suggested that we found what we expected to find. However, anyone replicating the analysis will obtain a list of the same studies, and there is little interpretative flexibility. A trial on cognitive-behaviour therapy is not a study on the role of enteroviruses, and conclusions that chronic fatigue may be perpetuated by inactivity cannot be interpreted as a suggestion that the illness is complicated by an abnormal immune response. We therefore reject the argument that the bias reflects our expectations. Moreover, we are not the first to have documented a far from balanced editorial policy in the *BMJ*. An independent report published ten years ago

also identified a tendency to favour psychological explanations (17). For details of additional correspondence, see Appendix 1.

Finally, it may be argued that the *BMJ* did not receive any original papers on bio-medical subjects during the period in question and that this explains the 'bias.' We do not have any information on the total number of submissions on CFS or the topics covered, but we are aware of a number of relevant papers submitted by colleagues which were rejected. They include a study which found hypoperfusion in the brainstem of patients with ME/CFS but not in those with concurrent depression, in patients with major depressive disorder or in healthy controls (19). Similarly, Lane et al.'s findings of enteroviral sequences in a subgroup of patients were also subsequently published elsewhere (20).

Since the analysis and correspondence with the editor, there has been no apparent change in editorial policy. Thus there were no original reports on neurological, immunological or virological aspects of the illness, and only one item alluding to such research published elsewhere (*Minerva* August 21st 2004).

Given the findings, our knowledge of papers which have been rejected, plus the fact that between 1995-2000, only one study was published clearly linking CFS with a non-psychological aetiology, there remains little doubt that the editorial policy of the *BMJ* is uncritically supportive of the psychiatric view of CFS. This has seriously compromised the quality of information provided on CFS to the readers of the *BMJ*. Moreover, our analysis shows that the journal has consistently ignored non-psychiatric professional views on CFS. Based on our knowledge of this illness, we are unable to find sufficient scientific reason to justify this stance.

Flawed scientific research is harmful since the principle of self-correction by science cannot be applied. In addition, the editor's decision on the selection of scientific communications and choice of authors for review articles and editorials on CFS should not only be fair, but must be seen to be fair. We were unable to see this transparency or balance of opinion on CFS in this search of papers published during the five years in the *BMJ*. Website if required: <http://freespace.virgin.net/david.axford/me/me.htm>

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APPENDIX 1

Following the Analysis: Challenging the Bias

The request for a more evidence-based, as opposed to theory-led editorial policy was rejected by the editor. One argument was that the choice of material was no different to that in similar journals (letter January 2001).

To see if the choice of papers for publication in the *BMJ* differed from those of comparable high impact medical journals, we conducted the following analysis, using data from the same time period. All journals were listed in the medicine, general and internal section of the Journal Citation Reports for 2000 alongside the *BMJ*, and all had published a number of papers on CFS (21). The highest ranked journal, the *New England Journal of Medicine*, did not publish any articles on CFS during this period so could not be included in the analysis. This was also the case for two other journals in the top ten: *Medicine* and the *Proceedings of the Association of American Physicians*. *The Annual Review of Medicine* published a review but no original research. A separate analysis assessed papers published by the *BMJ* in the five years prior to the period in question. Details of the publications in the *Lancet* will be discussed elsewhere.

Analysis of Papers in the Journal of the American Medical Association (JAMA)

The MEDLINE search identified 14 items. Of these, two were original papers, 2 were case histories (same case over one year) and 10 were letters. The

subjects of the research papers were treatment (hydrocortisone) and the relationship between CFS and neurally mediated hypotension. With only two studies, it is not possible to discern a clear bias towards any one theory. However, there was no obvious policy favouring the CBT model.

Analysis of Papers in the American Journal of Medicine (Am J Med)

The MEDLINE search identified 42 items including 21 articles in a supplement of conference proceedings of which 9 were original papers and the rest were reviews. In addition, there were 10 original reports, 2 editorials and 9 letters or replies.

The supplement covered a broad range of subjects and views. Of the original papers not in the supplement, one was on treatment, three covered biochemistry and physiology, one focused on the validity of the diagnostic criteria, and two assessed risk factors. De Meirleir et al.'s study concerned immunological aspects of the illness (22), with one editorial discussing the findings (23) and another directing readers' attention to the evidence linking CFS with cognitive-behavioural factors (24). Two of the papers implicated psychological factors as playing a significant role in aetiology (25, 26), one reported on an intervention which included cognitive-behavioural therapy (27) and one focused on seasonal affective disorder (28).

Readers of the *American Journal of Medicine* were well informed about the latest research. No study focused solely on one psychiatric theory, but there was an editorial and a number of letters which discussed CBT. Although there were six papers dealing with psychological aspects of the illness, there was no evidence of any bias or preference.

Analysis of Papers from the Annals of Internal Medicine (Ann Int Med)

The MEDLINE search identified 12 items, with one original paper (on prevalence), one review (on functional somatic syndromes), and ten letters.

There are too few papers to assess bias, but the review on somatisation (30) was balanced by the publication of six critical letters. We therefore cannot conclude that there was evidence of a preference towards one school of thought.

Analysis of Papers from the Archives of Internal Medicine (Arch Int Med)

The MEDLINE search identified 14 items, consisting of 6 original papers and 8 letters.

The subject matter of the studies included treatment (fludrocortisone), prevalence, overlap with other conditions, a ten year follow-up with recovery rates and a similar paper on outcome, and the results of laboratory tests. There was no study focusing exclusively on a psychiatric aspect of the illness, though one of

the letters discussed psycho-social factors. There was no study on the virology or immunology.

Again, we found a wide range of articles and there was no evidence of a preference towards one theory as opposed to another.

Comparison of the BMJ with the Lancet and American Journals

Using original reports, reviews and editorials only, we counted the number of articles which were focused predominantly on psychiatric factors or which implicated psychiatric factors as playing a significant role in aetiology and recovery. The results were compared using chi-square tests for independent samples and exact P-values were calculated because of the occurrence of low cell counts in some of the analyses. Exact P-values are based on the exact distribution of a test statistic and are appropriate where the data set is small.

As Table A1 shows, the *BMJ* and *Lancet* published 13 psychiatric articles while there were only seven such papers in the American journals (24-30).

The difference in the number of papers on non-psychiatric and psychiatric factors published by the various journals was highly significant ($\chi^2 = 23.39$, $df = 5$, $P < .0001$). This remained the case when either *BMJ* or *Lancet* was excluded from the analysis (*BMJ* excluded: $\chi^2 = 13.48$, $df = 4$, $P = .0091$; *Lancet* excluded: $\chi^2 = 19.66$, $df = 4$, $P = .0004$). However, when both *BMJ* and *Lancet* were excluded, the difference between the journals was no longer significant ($\chi^2 = 3.822$, $df = 3$, $P = .2558$). This supports the view that the British journals published a greater proportion of psychiatric articles than the comparable American journals.

TABLE A1. Non-psychiatric and psychiatric articles published in *BMJ*, *Lancet*, *Am J Med*, *Ann Int Med*, *Arch Int Med* and *JAMA* between 1 Jan. 1995-31 Aug. 2000.

Journal (rank)	Articles				Total
	Non-Psychiatric		Psychiatric [#]		
	N	(%)	N	(%)	
<i>BMJ</i> (7)	2	(20)	8	(80)	10
<i>Lancet</i> (3)	2	(29)	5	(71)	7
<i>Am J Med</i> (8)	27	(82)	6	(18)	33
<i>Ann Int Med</i> (4)	1	(50)	1	(50)	2
<i>Arch Int Med</i> (6)	6	(100)	0	(0)	6
<i>JAMA</i> (2)	4	(100)	0	(0)	4
Total	42		20		62

[#]Broadly-defined, see text

Analysis of Papers in the BMJ 1990-1995

A search using MEDLINE for papers in the *BMJ* published between the first of January 1990 and 1995 identified 43 items. Of these, 31 were letters, one was an editorial, one was a review of sleep disorders citing CFS, and ten papers reported original research. The studies covered a range of topics including epidemiology, virology (2 papers) and physiology. There were also two longitudinal studies. Overall, there was a subtle bias towards psychiatric aspects of the illness, particularly notable in the editorial, but there was also information on virological and other non-psychiatric factors which provided some degree of balance.

The findings indicate that general medical journals published outside the UK do not have the same editorial bias in relation to CFS as the *BMJ* does at the moment. It is also noteworthy that the *BMJ* published a wider range of articles prior to the period in question.

Contact with the Journal Committee of the BMJ and Other Organisations

Following our correspondence with the editor of the *BMJ*, we contacted the Chairman of the Journal Committee (September 2001). He also rejected our concerns, noting that the choice of articles reflected "editorial freedom."

We accept the principle of editorial freedom. However, there are two issues which deserve consideration.

1. The *BMJ* claims to choose papers on best evidence. Indeed, this is what readers expect from a medical journal. It differentiates scientific publications from non-scientific ones. One might therefore ask whether any mainstream medical journal should have a policy which promotes one theory at the expense of another. Moreover, should a scientific publication censor *all* the research supporting alternative theories? And should the editor overlook major flaws and disinformation as part of the policy? Does this not undermine the scientific process?
2. If a journal has the 'freedom' to be partisan and promote one approach at the expense of others, should it not *declare* its policy so readers are aware of it and realise that they need to seek information on the immunological and virological aspects elsewhere? And should that journal continue to claim to support evidence-based medicine when it selects articles on one illness largely on the basis of personal preferences? If one accepts that medical journals may be partisan, then there must be honesty and transparency. At the moment, this is not the case.

The BMA Ethics Committee was unwilling to consider a complaint regarding the journal's editorial policy and the Press Complaints Commission refused to investigate the matter as it did not fall under their Code of Practice (letter, July

2003). The COPE council discussed the evidence of bias but felt that this was a matter of 'editorial independence' and "discretion about what papers should be published," and that this is outside the remit of the COPE Code of Conduct for editors (e-mail 24th February 2005).