

fatigue syndrome and somatisation needs discussion. Ten years ago, Hickie and several New South Wales physicians were dismissive of an article linking chronic fatigue syndrome and neurasthenia: "We have demonstrated immunological abnormalities in patients with chronic fatigue syndrome as compared with both normal controls and patients with major depression. Further, the demonstration of abnormal cytokine production in patients with chronic fatigue syndrome may underpin 'acquired neurasthenia'."³ All the SOMA-6 items are key symptoms of chronic fatigue syndrome. With the overwhelming amount of biological data now available, purely psychological theories about chronic fatigue syndrome (as opposed to chronic fatigue) are totally untenable, as is the use of the term neurasthenia, introduced into medicine in 1869 and discarded by the American Psychiatric Association's *Diagnostic and statistical manual of mental disorders*⁴ as invalid.⁵ A recent study from the Fatigue Clinic, King's College Hospital, UK,⁶ found that an astonishing 68% of patients had been inappropriately misdiagnosed with a psychiatric illness. This is surely a warning for overzealous psychiatrists.

Thirdly, treatment implications are a concern. The authors comment that "only 27% of patients with *Level 1* disorders received pharmacological interventions" (*Level 1* implying positivity for PSYCH and SOMA items). They say that general practitioners mostly used "relatively ineffective non-pharmacological strategies", and that they had responded to missing all this "unmet need" by "criticising the oversensitivity of the screening instrument and inappropriateness of diagnostic systems used", implying an underprescribing of antidepressants. The recent National Survey of Mental Health and Well-being⁷ showed that somewhere between two-thirds and a half of the 23% of the population diagnosed with psychiatric disorders did not visit their general practitioner. How does this fit in with the 49% found by Hickie et al?

In summary, the authors' broad and idiosyncratic conceptualisation of "mental disorder" and their use of a screening tool which labels many physically ill people with or without concurrent distress as cases of "mental disorder" implies that general practitioners need to prescribe more antidepressants — at what cost and for whose benefit?

For a longer version of this letter, contact the first author.

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3. Hickie I, Lloyd A, Wilson A, Wakefield D. Taking chronic fatigue syndrome seriously. *Am J Psychiatry* 1992; 149: 1755-1756.
4. American Psychiatric Association. *Diagnostic and statistical manual of mental disorders*, 4th ed (DSM-IV). Washington, DC: APA, 1994.
5. Phillips N. Response to prolonged fatigue, anxiety and depression: exploring relationships in a primary care sample [letter]. *Aust N Z J Psychiatry* 2000; 34: 692-694.
6. Deale A, Wessely S. Diagnosis of psychiatric disorder in clinical evaluation of chronic fatigue syndrome. *J Roy Soc Med* 2000; 93: 310-312.
7. Andrews G, Henderson S, Hall W. Prevalence, comorbidity, disability and service utilisation: an overview of the Australian National Mental Health Survey. *Br J Psychiatry* 2001; 178: 145-153. □

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IN REPLY: It is with great pleasure that we resume our ongoing correspondence with Phillips concerning the medical and psychological status of patients who present with non-specific somatic complaints such as chronic fatigue.¹ As we have reported previously,² we have been strong advocates of both the need to develop appropriate instruments for measuring neuropsychiatric states characterised by non-specific somatic symptoms and to promote effective medical and psychological management of patients with these disabling conditions.³

In their letter, Phillips and colleagues fail to grasp the essential issue. To describe a condition as a neuropsychiatric state (or mental disorder) does not necessarily lead to simplistic and entirely unhelpful assumptions about "medical" versus "psychological" causes or treatments. Phillips et al attempt to promote once again the notion of "biological" (ie, acceptable) versus "psychological" (ie, unacceptable) theories of the causation of chronic fatigue syndrome. Such an approach is not only intellectually sterile and inconsistent with the past decade of intensive research by a wide range of medical and psychological research teams,⁴ but also profoundly unhelpful to people affected by these disabling disorders.⁵

In recent years, the very significant health burden of common mental disorders such as depression, anxiety, alcohol or other substance misuse, and neurasthenia (prolonged fatigue states lasting longer than three months) has been well documented in the Australian community⁵ and in the primary care setting.⁶ Phillips and colleagues appear to have no knowledge of the basic epidemiological fact that mental

disorders are two to three times more common in primary and other medical care settings than in community studies (hence the total rate of disorder in our study is about twice that detected in the Australian National Survey of Mental Health and Well-being). Contrary to their implications, the total rates reported in our general practice study are entirely consistent with the largest multinational study of primary care ever conducted.⁷ That study indicated that a third of all primary care patients have mental disorders and another third have mental health difficulties (with or without concurrent medical disorders) requiring specific psychological assessment.

The significance of our study is that it has brought the extent of common mental health needs (including depression, anxiety, alcohol or other substance-misuse and somatoform disorders) to the attention of the Australian medical profession. What is now required is a concerted and integrated response — not a return to dualistic notions of illness that have for so long hampered the provision of effective pharmacological and non-pharmacological treatments to patients with mental disorders who present for medical care.

1. Phillips N. Response to "Prolonged fatigue, anxiety and depression: exploring relationships in a primary care sample" [letter]. *Aust N Z J Psychiatry* 2000; 34: 692-694.
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EBM in action: Is laser treatment effective and safe for musculoskeletal pain?

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TO THE EDITOR: The EBM in Action article on laser treatment by Del Mar et al¹ raises my anxiety about the reliability of evidence-based medicine (EBM) in general and, at the very least, the authors' assessment of the question they set out to answer. One is seldom, if ever, in a position

to understand the breadth and depth of an issue unless it is the subject of particular study. Most of us can not challenge statements made in such articles without an intimate knowledge of the literature. As laser therapy is the topic of my PhD thesis, I am in a unique position to have much of the literature on the subject at my fingertips.

The authors state in their conclusions that "low power laser therapy appears to be no more efficacious than placebo in relieving musculoskeletal pain". Several aspects of the analysis on which they base this conclusion cause me great concern. Firstly, they state that "The search report highlighted the high quality of evidence supporting the refined question". Quite the contrary. One of the two systematic reviews they cite² has been criticised in the literature for its many inadequacies, not the least being that laser acupuncture and laser therapy are included in that review as if they were the same, which they are certainly not.³ In addition, the review by Beckerman et al concludes, with regard to musculoskeletal pain, that "the efficacy of laser therapy for musculoskeletal disorders seems, on average, to be larger than the efficacy of placebo treatment. More specifically, for rheumatoid arthritis, post-traumatic joint disorders and myofascial pain, laser therapy seems to have a substantial specific therapeutic effect".⁴ How can this fit with the conclusion of Del Mar et al? Furthermore, Gross et al state in their review, which consisted of three trials of laser therapy, that "In general, all therapies have not been studied in enough detail to adequately assess either efficacy or effectiveness".⁵ The choice of other articles by Del Mar et al is also somewhat mystifying in that, out of the nine references cited, one is in Russian and two in Danish. There are many other relevant articles in English that they have not cited.^{6,7}

There is no doubt, as I myself have found, that it is difficult to search for this topic in the literature, as there are many terms used for laser therapy and the information comes from a broad range of sources. However, this is no excuse for a group that holds itself out to be "expert" in a field.

It is very disappointing to see such an incomplete review with such an inappropriate conclusion based on such a poor sample of the literature. If this is an example of EBM in action, then I think we should be very concerned.

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IN REPLY: Chow criticises us for not identifying all relevant trials. But is she willing to help others in the process of reviewing? We could not identify a systematic review of the area with her involvement, nor does she appear to have registered an appropriate protocol with the Cochrane Collaboration (instructions for which are available at <<http://www.cochrane.de>>). Systematic reviews are important to help clinicians make sense of a diversity of trials. If experts such as she, devoting years to the area, do not help us, who should?

We were not attempting such a systematic review (which would probably take several months of work). Rather, we were trying to provide clinicians with the best obtainable answer in a 24-48-hour turnaround time.² Given that clinicians might have one question per patient,³ attempting a systematic review with each question would give us a lifetime of work after only a fortnight of clinical work! We needed to balance the timely requirements of clinicians with the quality of the evidence obtained. As Chow points out, tracking down every last trial is difficult. Therefore, we first aimed to identify systematic reviews rather than attempting to find all trials ourselves. These reviews missed some relevant material — as indeed did Chow, who did not cite several recent studies,^{4,4} which makes us wonder if she has a strong prior belief that may bias her views. Would these missed trials have made any difference to the conclusions we reached previously? A systematic review published since our original literature report (January 2000) suggests not, although there may be some specific subgroups of patients and conditions for which laser therapy is effective.⁵

All of this highlights the need for a collective effort to sort out the mess of

medical information. In seven years the Cochrane Collaboration has systematically reviewed less than 5% of more than 300 000 trials on the clinical trials registry. They are difficult to perform and maintain. Millions of dollars continue to pour into primary research that still remains inaccessible to us at the clinical frontline, the "great criticism" with which Archie Cochrane pricked the profession into action.⁶ Please, Dr Chow, abandon throwing bricks from the sidelines, and join us in trying to help clinicians access research evidence in the timely fashion needed for day-to-day practice!

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1. Del Mar CB, Glasziou PP, Spinks AB, Sanders SL. Is laser treatment effective and safe for musculoskeletal pain? *Med J Aust* 2001; 175: 169.