Summary of single fibre electromyographic findings in the right extensor digitorum communis muscle of 10 patients with postviral fatigue syndrome

Case No	Age and Sex	% Fibre pairs with abnormal jitter (>55 µs)*	Mean overall jitter (µs)†	Jitter abnormality	Mean fibre density‡	Fibre abnormality
1	51 M	20	40.1	+	1.76	—
2	32 F	20	40.9	+	1.68	—
3	41 F	20	34.8	+	1.5	—
4	35 F	20	38.9	+	1.72	—
5	61 M	60	114.3	+	1.4	—
6	37 M	20	52.3	+	1.4	—
7	60 M	40	94.7	+	1.2	—
8	54 M	20	46.1	+	1.7	—
9	20 F	40	54.9	+	1.3	—
10	18 M	20	36.7	+	1.5	—

*20% or more is abnormal.³
 †Upper limit of normal = 73.4 µs, ‡1.81.

membrane disorder. There has also been some recent evidence of an abnormality of red blood cell membrane in these patients.⁵ These and our findings may suggest that there is a generalised membrane dysfunction, the brunt of which probably falls on muscle fibre membrane and which is likely to be related to the symptoms of fatigue. There is some evidence that an abnormal local lymphokine response might be the basis for this membrane dysfunction.⁶

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SIR,—Dr Anthony David and colleagues call for a new approach in the postviral fatigue syndrome (5 March, p 696). We are unhappy, however, with their emphasis on the manifestation of a precipitating viral illness and hair splitting definitions of fatigue. It is precisely because these aspects have proved to be unsatisfactory specific diagnostic features that controversy remains. Until we have a reliable test which can refute or confirm the diagnosis in the individual case then no amount of fine honing of clinical definition will resolve the difficulties. Against what criteria could such definitions be judged? We cannot assume that patients who do not fit a particular author's definition are not cases, or conversely that those who do are thereby confirmed. We are hampered not by "poor case definition" but by our inability to confirm the suspected diagnosis in any particular case, however defined.

There is no specific treatment at present and a psychological approach is appropriate in certain cases. For the patients seen by Dr David and others, presumably in a psychiatric setting, it makes little practical difference whether they actually have the disease or whether their "illness behaviour" is due partly or entirely to some other cause.

We all know that psychological factors are important in any physical illness from the common cold to carcinomatosis. The implication that some special relation between mental and physical factors applies in this case remains unconvincing.

The authors have misquoted our own work on this topic. The incidence in our study was 140 cases out of a population of 10 000 over six months¹ and not a year as they stated. They went on to suggest that 65% of these patients developed the postviral syndrome within one month of an acute viral illness. In fact these patients presented within one month of the onset of symptoms, suggesting the postviral syndrome rather than any acute episode.

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SIR,—We agree with Dr Anthony S David and colleagues that a multifactorial approach with explicit case definition is the one most likely to make further progress in elucidating and managing the postviral fatigue syndrome.

The informal Epidemic Myalgic Encephalomyelitis Study Group has endeavoured for some years to convince those with the appropriate resources that such a study is needed, but progress has been hampered by a widely held belief that the illness is either imaginary or entirely psychological. The progress that has been made so far is largely due to collaboration between virologists, histopathologists, neurologists, immunologists, and general practitioners. Dr David and his colleagues think it counterproductive for doctors suffering from this condition to research it. Nevertheless, such doctors have contributed much to the existing body of knowledge. Without their commitment to investigation during the past four decades there would have been far less documented material for Dr David and his colleagues to discuss.

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Percutaneous cholecystolithotomy

SIR,—We were impressed by the description of this technique by Dr M J Kellett and colleagues (13 February, p 453) but cannot accept their extraordinary statement, "We believe strongly that it is unethical to treat patients with gall stone lithotripsy without having a *percutaneous* method of drainage and stone retrieval to *avert* severe obstruction or cholangitis" (our italics).

Cholecystectomy is a very well tried and safe treatment for gall stones which the authors themselves used when their technique failed. On the evidence of eight cases (performed, like cholecystectomy, under general anaesthetic) with one failure and one fistula into the colon, no one can deem that the percutaneous technique has superseded cholecystectomy. Endoscopic retrograde cholangiopancreatography is also well established for retrieving stones that have passed down the common bile duct and we have no evidence that percutaneous cholecystolithotomy can deal with this problem. When Dr Kellett and colleagues refer to averting severe obstruction they may be implying that any larger fragments remaining in

the gall bladder should be removed prophylactically. If that is their philosophy they should remove small stones from thousands of asymptomatic patients in the general population. The evidence from Munich is that a very small proportion of patients (2 out of 175) had problems from stone fragments at the lower end of the bile duct.¹ Of course, all the Munich patients were also given bile salts as dissolution therapy (also a well established treatment), and many gastroenterologists see lithotripsy as an aid to dissolution rather than a therapy in its own right.

It seems that Dr Kellett and colleagues have allowed their great enthusiasm for percutaneous treatment to push them into making an ethical statement that is unjustified. We do, however, wholeheartedly accept the ethical principle that "when a new technique is first used its safety and side effects must be carefully assessed and that the protocol must provide for acceptable, alternative treatment should complications arise."

Our own protocol in the assessment of lithotripsy treatment of gall stones states that the patients should be fit for anaesthesia, allowing us to proceed to cholecystectomy or endoscopic retrograde cholangiopancreatography should complications arise. In the treatment of gall stones there is as yet no good reason for a further alternative such as percutaneous puncture of the gall bladder. Is it ethical to use a second new treatment to rescue the complications of the first?

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AUTHORS' REPLY,—At a time when technology is advancing rapidly and minimally invasive surgery is avoiding painful incisions and long postoperative recovery times the response of Dr Johnson and colleagues that the first fallback for failed shock wave lithotripsy of gall stones should be open surgery is exactly the approach we are arguing against.

We have shown that percutaneous nephrolithotomy has a complementary role to extracorporeal shockwave lithotripsy, with retrograde ureterorenoscopy as a back up for stones in the ureter. Open nephrolithotomy is indicated only in a minuscule group of patients suffering from very large staghorn calculi.¹ The importance of having the back up of percutaneous and endoscopic techniques in a centre performing lithotripsy for renal stones is paramount.

We feel that percutaneous cholecystolithotomy should now be a vital addition to the armamentarium of the clinician treating gall stones, complementing endoscopic retrograde cholangiopancreatography and shock wave lithotripsy, cholecystectomy obviously being a fallback when these techniques fail. Complications from extracorporeal shockwave lithotripsy for gall stones appear to be low and the unit in Sheffield has a clear protocol for its assessment of patients before lithotripsy. We fear, however, that many centres may be treating gall stones with extracorporeal shockwave lithotripsy without such assessment and without any endoscopic back up.

The incidence of residual stones after gall stone lithotripsy is depressing even when combined with dissolution therapy. From the Munich experience in 175 patients 55% of those with single stones had