

Correspondence

Hyperbaric Oxygen in Myocardial Infarction.

SIR—I find myself most unhappy about the article on 'Severe Acute Myocardial Infarction Treated with Hyperbaric Oxygen. Report on Forty Patients' by Ashfield & Gavey (*Postgrad. med. J.*, 1969, 45, 648). An attempt is made to present a series of uncontrolled clinical observations on a notably variable condition as a critical evaluation. The subjective nature of the whole argument is scarcely concealed. The whole article is perhaps best described by the curiously opalescent fifth paragraph of their summary.

However, myocardial infarction is a common and serious illness and a new form of therapy which might be valuable must be considered carefully. The paper appears to establish symptomatic relief of pain and dyspnoea though the evidence even on that is thin enough. Twenty-three patients had pain—and we are not even told how many were relieved, only 'the majority'. Nor is clear evidence adduced as to the role of hyperbaria. The dyspnoea with pulmonary oedema (the predominant symptom in 35% of the cases) was apparently always relieved. Evidence as to clinical and radiological signs suggests that this was at best a symptomatic effect, though an attempt is made to argue otherwise. Apparently no other therapy was given. It is difficult to know what the severity of the illness in their patients was but, if there was no change in signs and a low mortality, one suspects they cannot have been severe.

The authors' subjective approach throughout gives rise to misgiving. No evidence is given to enable one to penetrate beyond their clinical impressions. Criteria for LVF, shock, are not given; biochemical evidence of shock is lacking though arterial blood samples were apparently taken. ECG observations are exclusively anecdotal—what credence can we give to a report which merely states: 'restoration of sinus rhythm in many arrhythmias' as one of the results and: 'reduced incidence of ventricular ectopic beats' as another? Evidence on changes in the chest X-ray is equally lacking. The lack of information makes it impossible to assess the patients or the treatment.

It would be possible to continue with this destructive criticism indefinitely. Having read this paper I share the authors' impression that this might be a useful form of therapy, but I would like to have some evidence for this.

A proper clinical and scientific assessment is necessary—the painstaking work of Cameron and his colleagues would have formed a good model. Can we now ask that Dr Ashfield and Dr Gavey, having recovered from their initial enthusiasm, assess this treatment properly? It may be of little value—symptomatic therapy at best—or it may have a real contribution to make. The most unfortunate result of this article could be that the uncritical acceptance by the authors of this form of treatment could prejudice many people against hyperbaric oxygen when it may be of value.

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We have shown this letter to Dr Ashfield who replies as follows:

SIR—Naturally we are also unhappy that Dr McNicol is unhappy with our account of this work, but we are not surprised. He is criticizing a pilot study which was initiated only to see if there were grounds for organizing a controlled trial. No attempt is made to 'prove' anything at this stage, and the techniques and measurements were not intended to stand comparison with the Olympian studies described in the papers of Dr McNicol and his excellent, and large, teams.

A controlled trial is well under way at the Westminster Hospital, because of the observations that were made in the present study. With respect to the admittedly painstaking Dr Cameron, his original controlled trial was unfortunately invalidated by his later discovery that the deficiencies of the BLB mask prevented his patients receiving oxygen at 2 atmospheres anyway! His 1966 paper, while very valuable from the physiological point of view, involved patients in a single hyperbaric session as an investigative procedure, and was not an attempt to give hyperbaric oxygen as a treatment.

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