Letters

efficacy and low incidence of side effects from this treatment.1–5 Two further papers appeared in the November issue of the Annals.6 7 The paper by Walters and Cawley showed the value of combining pulse methylprednisolone with sodium aurothiomalate treatment,6 results similar to our findings in a previous publication, of which the authors appeared not to be aware.8 The paper by Williams et al again showed the low incidence of adverse effects from this treatment and, more specifically, failed to find an increased incidence of avascular necrosis of the femoral head, which theoretically would be an expected adverse effect in view of the large doses of corticosteroid used in pulse methylprednisolone therapy.7 Though this retrospective cohort study needs to be confirmed by a prospective long-term study with large patient numbers as well as matched controls, it confirms the favourable risk/benefit ratio of this treatment. It is therefore surprising that our papers on this subject in another journal3–5 led to an editorial comment6 and correspondence9 10 describing this treatment as ‘ineffective’ and ‘hardly justifiable’ in the treatment of rheumatoid arthritis. Perhaps this simply reflects a conservative attitude of North American clinicians, yet it is surprising that pulse therapy, with its favourable risk/benefit ratio, considerable efficacy in short term control of inflammation, and efficacy as oral treatment,11 should not be at least as attractive a therapeutic option as the alternatives—namely, non-steroidal anti-inflammatory drugs (low efficacy, high incidence of adverse effects) and disease-modifying antirheumatic drugs (slow onset of action, high incidence of adverse effects, high drop-out rate). None of the recent publications has advocated pulse therapy as an alternative to remittive agents, but rather as adjunctive treatment to achieve rapid control of inflammation while awaiting control with a remittive agent, or in the event of failed treatment with several disease-modifying antirheumatic drugs.

Perhaps it is time for a Viewpoint article in the Annals on the place of pulse methylprednisolone therapy in the treatment of rheumatoid arthritis, lest a potentially useful treatment should be neglected as a result of therapeutic conservatism.

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Evaluating drugs in rheumatoid arthritis

Sir, In their recent viewpoint Scott et al discussed the problems of assessing rheumatoid arthritis in clinical trials.1 Some of these prominent workers have apparently changed their minds lately. Some standard variables were not discussed.

The authors did not comment on the use of visual analogue scales, McConkey's clinical score, Ritchie's index, and patient opinion. Patient opinion is certainly one of the best methods of assessing the value of disease-modifying drugs in rheumatoid arthritis. As long as the patient is taking a medicine he or she has accepted it, and the length of the treatment period is a measure of patient opinion of the drug. The time has passed when the doctor's opinion was the deciding factor for the patient. The reasons for withdrawal are often multifactorial, and factors other than those reported may be concealed. If the drug is good for the patient he or she will stick to it and accept a certain degree of inconvenience. Thus in my opinion treatment survival as a measure of patient opinion is the most important variable in an evaluation of any anti-rheumatic drug.

The authors stated that 'the debate about the true place of x-rays using current technology has subsided; there is now a relatively negative view of x-rays and less value is placed on them'. This view was not held in recent trials on methotrexate2 and auranofin,3 where x-ray evaluation was of basic importance. Recently, Symmons and Dawes stated that radiological assessment is useful both in serial studies to assess the severity of disease and as an outcome measure.4 Previously, McConkey had stated that in his opinion the best way of studying drugs in rheumatoid arthritis is to carry out serial measurements of the acute phase proteins, relating these measurements to changes in function and to changes in the radiographic appearance.5

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Hepatic veno-occlusive disease and herbal remedies

SIR, The recent report by Lemley et al of a patient who developed hepatic veno-occlusive disease while taking azathioprine is of interest to us. We have recently seen a patient with veno-occlusive disease and another chronic disease—namely, multiple sclerosis. This patient was taking no drugs but was discovered to be an avid taker of herbal remedies, including comfrey tea.

It has been established that this herbal medicine can result in veno-occlusive disease. We note the comment of Lemley et al that ‘despite years of experience with azathioprine in RA, veno-occlusive disease has never been previously reported in this population’. Although the orthodox drugs taken by this patient are listed, no mention is made of whether he took any herbal remedies or other alternative medicine. Although azathioprine may have caused veno-occlusive disease in the patient described, we feel it is important to remind practitioners treating people with chronic diseases that not all alternative treatments are harmless and also that the side effects of such treatments may be mistakenly assigned to orthodox drugs.

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References