assess the ability of ambulance staff to achieve successful outcomes using existing training.

All the patients included in the study were emergency cases—that is, either 999 calls or cases classed as an emergency by a general practitioner—and all had “hands on” assessment by ambulance and medical staff. To suggest that this prospectively performed study was a “table top theoretical exercise” is therefore far from the truth.

All of the patients admitted to a resuscitation area were assessed in detail, not merely the 396 who died. The patients in resuscitation areas constituted 46% of all the emergency cases studied in Edinburgh and 45% of the Glasgow cases. These figures are similar to those reported for accident and emergency departments elsewhere in Britain.

Though “abundant evidence in favour of extended ambulance skills” exists in the United States, the situation in Britain is very different. Drs Baskett and Sleet claim that “hard data” are available from centres that have had years of experience with ambulancemen trained to perform cardiopulmonary resuscitation. We do not dispute the role of early defibrillation for ventricular fibrillation, and we agree with Mr William Rutherford (28 February, p 578) that there is a strong case for concentrating resources in this area. In the absence of cardiopulmonary resuscitation by a bystander, however, the limiting factor is the time the ambulance service takes to respond. We refute the comment that “the journey times...leave something to be desired.” The time referred to as “journey time” in fact represents the time from the initial 999 call to arrival in hospital and not the response time of the ambulance service.

We in no way denigrated the fact that “only 54” lives might have been saved; this figure represented the maximum possible benefit, and to suggest that over 2500 lives could be saved in Britain every year is an inappropriate interpretation of our results. As the authors indicate, we took no account of the effect of extended training on mortality. We would be delighted to have a scientific and reproducible assessment for morbidity in patients before they reach hospital.

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Phototherapy and dithranol treatment of psoriasis

SIR,—Dr P M Farr and coworkers (24 January, p 205) attempted to define whether ultraviolet treatment hastened the improvement in psoriasis treated by dithranol. They studied the response to three different types of apparatus: a medium pressure mercury arc lamp, an array of six 60 cm fluorescent sunlamps, and an array of six 30 cm ultraviolet fluorescent sunlamps.

I agree that both ultraviolet radiation and dithranol are effective treatments for plaque psoriasis.1,2 Dr Farr and colleagues’ tables II and III, however, contain clinical details that may lead to a premature conclusion about the efficacy of the different types of ultraviolet equipment studied.

Table II shows that the lesions on the non-irradiated, dithranol treated (control) arms of the patients treated with the mercury arc lamp took 10-75 days to reach half of their initial thickness. This compared with nine days for the two other ultraviolet treatment groups.

Similarly, table III showed that the mean clearance time for the lesions on the non-irradiated arms was 15-19 days, whereas the psoriasis in the patients treated with the heliolum lamps and fluorescent sunlamps was cleared at a slower rate and thus the ultraviolet seemed to produce better results.

One explanation may be that the rate of clearance of psoriasis achieved with dithranol in the patients treated with the mercury arc lamp could not be improved by additional ultraviolet, whereas the psoriasis in the patients treated with the Helarium lamps and fluorescent sunlamps was clearing at a slower rate and thus the ultraviolet seemed to produce better results.

In addition, a more suitable study would measure the effects of treatment with ultraviolet radiation and emollients alone using the different ultraviolet lamps described.1 Dr Farr and colleagues have shown clearly the efficacy of dithranol alone in the treatment of psoriasis.

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AIDS counselling and informed consent

SIR,—There is a very simple cure for many patients with tennis elbow, which is little known and which is only briefly hinted at by Mr Thomas G Wadsworth (7 March, p 621).

Some years ago, when I played a great deal of tennis, I developed the typical lateral elbow in my right arm. This steadily worsened and was relieved for a day or two only after cortisone injections by an orthopaedic colleague. Rest gave no more than temporary relief, and I began to think that I should stop playing tennis. I mentioned this intention when operating at a cottage hospital one day. A general practitioner in the theatre, Dr R M Sheridan, bluntly responded: “Rubbish! Go to Briggs’s in Wood Green high street and get yourself a racquet with a bigger handle.” I took the doctor’s advice and went to Briggs’s shop as soon as I finished operating and bought a new racquet with a handle 3-75 cm larger in circumference than my existing one.

A day or two later I played three sets of tennis with Dr Sheridan. On taking my first service, I said: “I shall never be able to serve with this large handle.” My medicos, I snarled retorting “Shut up! You’ll be alright by the end of the first set.” He was absolutely right. Within half an hour I had adjusted my grip and never had any recurrence of trouble in the next 30 years. Lateral elbow pain is also often caused by gripping too small handles on shears and other garden tools. This can be prevented by building up the handles to the requisite thickness, and I have helped many sufferers to cure themselves in this way.

The mechanism of relief is simply explained if the position of the wrist when the hand is gripping objects of different thicknesses is studied. The smaller the circumference of the object held, the less the degree of dorsiflexion of the wrist. As progressively thicker objects are held, the wrist dorsiflexion increases, reducing the tension on the forearm extensor muscle origins. It seems that with a