SIR,—I was distressed to read in Sheila Hancock's contribution to the symposium on "Care of the Dying" that she had been told that her mother had about nine months and, later, that her husband had about one year to live. Surely no one can predict the length of time even a terminally ill patient has to live. Such a prediction could and indeed has, in my experience, caused great distress to patient, relatives, and friends, not to mention nursing staff and other members of the caring team.

I was just a little surprised, too, that no one spoke of the need for doctors and nurses to prepare themselves, get their thoughts in order, and sort out their feelings as regards their own demise because, in my opinion, unless they do so they cannot have the understanding and insight required in the treatment and nursing care of their dying patients.

Dr. Elisabeth Kübler Ross in her book *On Death and Dying*¹ has said that she helps people to live until they die, and this attitude was obviously subscribed to by all those taking part in the symposium, but here again one cannot do so effectively if one's thoughts, beliefs, and feelings are in a turmoil.—I am, etc.,

M. E. F. ALEXANDER

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¹ Kübler-Ross, E., *On Death and Dying*. London, Macmillan, 1970.

Predicting Survival in Terminal Cancer

SIR,—Dr. C. Murray Parkes recently reported (1 April 1972, p. 29) that in predicting the survival time of dying cancer patients before their admission for terminal care to an institution, doctors (and other hospital staff members) were typically over-optimistic. Their predictions, which were usually in the 0-12-week range, generally exceeded actual survival times. Errors of prediction were frequently in excess of 100% (actual survival time either more than double or less than half that predicted) and when such excessive errors occurred they were in large part positive errors. In contrast, Mr. J. Scotto and Dr. M. A. Schneiderman (7 October, p. 50) reported that in a controlled clinical trial involving less-terminal patients (estimated survival was supposed to be at least two months, but a few shorter predictions were made) physicians' predictions of survival time were reasonable and gave no indication of being either typically over-optimistic or typically over-pessimistic.

Both reports in fact indicate that doctors do make reasonable and useful predictions of survival time. But a distinction must be made between survival predictions in the terminal situation and those made when death is not yet imminent. For a progressive disease a physician might in one instance reasonably estimate that it would likely take two years for death to occur. With passing time and as the physician can view what is happening to the patient he can make more accurate estimates. But these more accurate estimates will display greater relative inaccuracy; an error of a week when the prediction is two weeks may seem large, but it would be unusually small when the prediction is two years.

The true difficulty in predicting terminal

survival time may be that when death does finally occur it is due to some singular and essentially unpredictable event. (Evidence for this essential unpredictability in the report by Dr. Parkes is the number of patients dying within one day even when predictions were as long as five weeks. It may be that movement to the hospital was contributory.) Perhaps the best we can expect the doctor to do is to say, from his past experience, what he would judge to be the average time remaining to the terminal patient. If the physician can do a perfect job in making such average predictions he will nevertheless seem to be unduly optimistic by the standards Dr. Parkes has used. For constant risk of death the probability of living beyond the average is only 36.8%, while that of dying sooner would be 63.2%—thus seemingly optimistic predictions would outnumber seemingly pessimistic predictions by 1.7 to 1. Errors of prediction in excess of 100% would be commonplace, with actual survival times more than twice average occurring with 13.5% probability and survival times less than half of average occurring with 39.3% probability—here seemingly extreme optimistic predictions would outnumber seemingly extreme pessimistic ones by 2.9 to 1. The physician could perhaps have avoided this appearance of over-optimism by giving as his prediction the time to which he thought the patient had a 50-50 chance of surviving; but then he would seem over-pessimistic as on the average he would be underestimating.

What is not clear from Dr. Parkes's report is exactly what physicians thought they were supposed to predict. For hospital planning purposes it would be desirable that the prediction be of an average survival time. Conceivably the doctor's prediction may be in some instances only an estimate of how long the patient might well live, while in other instances it may be only a warning of how soon the patient might well die. It is unreasonable to ask the doctor to provide a single number which will serve all possible purposes. Perhaps the doctor could specifically be asked to make an average prediction together with a likely upper limit. A likely lower limit may also be useful, but would probably be close to zero for most patients. Thus a prediction could take the form—"average of three weeks, but death may occur any time between now and nine weeks."—We are, etc.,

DAVID P. BYAR

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Organization of Conferences

SIR,—In "Personal View" (24 February, p. 480) Dr. Clifford Hawkins gives good reasons for having "a new look at organizing conferences." As he says, there is seldom good discussion; and yet "audience participation in a specialized society is surely the best way to encourage cross-fertilization of ideas."

I recall a conference, over 30 years ago, at which time was obviously running out. Accordingly, the last three speakers were given two minutes each (instead of 15 as

they had expected) and were invited to state their conclusions without presenting their evidence. The resulting discussions were the liveliest, the most informative, and the best sustained of the whole conference.

I am sure that the reason why many conferences fail to produce good discussion is that the audience have been offered far more than they can take. The essence of art is selection.—I am, etc.,

JAMES HOWIE

London W.1

SIR,—The "Personal View" of Dr. Clifford Hawkins (24 February, p. 480) presented food for thought on how medical conferences ought to be organized. Fortunately, many of his ideas have already been accepted by at least some organizers. A case in point was the Second International Meeting on Aerospace Medicine in Melbourne last year.

Each speaker had, in principle, 20 minutes for his presentation and 10 minutes for discussion. However, speakers were grouped (two or three together) according to topic, the discussion period then being cumulative, with the speakers acting as a panel for the discussion. The discussion was opened by someone who had previously received the full texts of the papers and was thus able to tie them together and present well-prepared comments. All participants had question cards, and the written questions were collected after each paper and given to the chairman. There were two roving microphones, and before each discussion period the first two questioners were asked to identify themselves and received each a microphone. After the first questioner had finished the third was identified and received a microphone, and so on. In this way each questioner was always ready to present his question or comment without the often tedious delay in receiving a microphone.

The frustration of simultaneous sessions was avoided, there being only one lecture hall. As the presentations were grouped according to topic, it was often possible to engage in personal discussions during those sessions not of immediate interest; this was greatly facilitated by the very strict adherence to the printed time-table, which was kept practically to the minute during the whole meeting. The coffee breaks and social functions were so organized that there was adequate (and frequently used) possibility for many useful personal discussions.

All in all, this meeting was practically a textbook example of how a medical conference ought to be run, and the organizers received much well-deserved praise from all practitioners.—I am, etc.,

R. G. CAMERON

Basle, Switzerland

Treatment of Status Asthmaticus

SIR,—In your leading article (9 December, p. 563) the use of heavy sedation with opiates is recommended to abolish the patient's respiratory drive during mechanical ventilation. We feel that the use of opiates in patients with asthma is unwise and potentially dangerous.

As early as 1915 the possible bronchoconstrictive effects of morphine and heroin

were demonstrated in normal subjects.¹ Sir Thomas Lewis showed in 1927² that intracutaneous morphine elicited a triple response similar to that induced by histamine. Histamine release by opium alkaloids has been demonstrated in isolated cat skin and muscle preparations.³ Elevated plasma levels of histamine have been measured in intact dogs following the administration of morphine and pethidine.⁴ A significant decrease in the lung capacity of intact dogs has been observed following the administration of morphine and pethidine—an effect most likely to be due to bronchoconstriction.⁵ The mechanism of opiate-induced bronchoconstriction may include central vagal stimulation, histamine release leading to enhanced bronchomotor tone and pulmonary vascular congestion, and histamine-induced reflex enhancement of vagal tone.⁵

In view of this evidence and the availability of alternative sedatives, we feel that the use of opium alkaloids in asthma is unjustified and should be avoided.—We are, etc.,

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- Higgins, H. L., and Means, J. H., *Journal of Pharmacology and Experimental Therapeutics*, 1915, 7, 1.
- Lewis, T., *The Blood Vessels of the Human Skin and Their Responses*. London, Shaw and Sons, 1927.
- Feldberg, W., and Paton, W. D. M., *Journal of Physiology*, 1951, 114, 490.
- Thompson, W. L., and Walton, R. P., *Journal of Pharmacology and Experimental Therapeutics*, 1964, 143, 131.
- Shemano, I., and Wendel, H. J., *Journal of Pharmacology and Experimental Therapeutics*, 1965, 149, 379.

Treatment of Typhoid

SIR,—We read with interest the article on chloramphenicol combined with ampicillin in the treatment of typhoid by Professor F. De Ritis and others (7 October, p. 17). We conducted a similar trial in Madras.¹ The patients were studied in three treatment groups: group 1 was given chloramphenicol alone (30 mg/kg/day); group 2 ampicillin alone (125 mg/kg/day); and group 3 a combination of chloramphenicol (20 mg/kg/day) and ampicillin (80 mg/kg/day). The mean duration of pyrexia after starting the treatment in the three groups was 5.6, 4.8, and 5.4 days respectively. Though the difference between the treatment groups was not significant the average dose of chloramphenicol and ampicillin was smaller in group 3. If the patients had been given full doses of chloramphenicol and ampicillin, as in the study of Professor De Ritis and his colleagues, the pyrexial period might have been shortened significantly. Furthermore, the need for steroid supplementation to control toxæmia after antibacterial treatment only for the first 72 hours was less in those on combined therapy than in those on chloramphenicol alone or ampicillin alone. One patient in each of the first two groups relapsed and none in the third group.

We also studied the use of furazolidone in enteric fever in a similar way.² Patients in group 2 were given chloramphenicol (43.5 mg/kg/day); those in group 2 were given furazolidone alone (17 mg/kg/day); and those in group 3 chloramphenicol (39 mg/kg/day) and furazolidone (12.5 mg/kg/day).

day). The mean duration of pyrexia after starting treatment was 4.6, 5.9, and 3.5 days respectively, the difference being statistically significant in favour of the combined therapy. No patients in group 3 relapsed, but one did so in each of the other groups.

Combined therapy seems therefore to be superior to single drug therapy. The possible advantage of a reduced dosage of the individual drugs in the combination, especially of chloramphenicol, is also to be considered. With an increasing number of reports of resistance of *Salmonella typhi* to chloramphenicol it will probably be wiser to start patients, at least those who are very ill, on all three drugs, as it is done in some other infections. When economy is also a consideration chloramphenicol and furazolidone, being cheaper, can be given in preference to the combination containing ampicillin.—We are, etc.,

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- Thiruvengadam, K. V., Subramaniam, S., Krishnaswamy, S., Subramanian, N., and Sharma, A. V. S., *Journal of the Association of Physicians of India*, 1970, 18, 1019.
- Thiruvengadam, K. V., Subramanian, N., and Sharma, A. V. S., *Journal of the Association of Physicians of India*, 1971, 19, 855.

Constraints on Consultants

SIR,—I disagree with the first sentence of your leading article (3 March, p. 501), in which you say that the consultant alone has the right to decide on the line of treatment for his patients.

In my opinion the patient has the right to accept or reject a particular line of treatment even when it has been explained to and understood by him. If he is not capable of accepting or rejecting treatment by virtue of his understanding or disposition such decision should be made by his next of kin. This is the procedure adopted in surgical and in medical wards but does not dispose entirely of the ethical and legal hazards.

The only exceptions to such procedure are urgent necessity of saving life or mitigation of disruptive social behaviour.—I am, etc.,

HAROLD PALMER

Harpenden, Herts

SIR,—If the concept that schizophrenia is caused by the family environment is indeed correct, then surely the appropriate solution should be group family therapy. It is therefore astonishing that you should be able in your criticism (3 March, p. 501) of the Napsbury experiment to state that the purpose of treatment was not even adequately explained to patients' relatives. There seems to have been a clear disparity between theory and practice.—I am, etc.,

I. ATKIN

Weybridge, Surrey

Graphic Records in Labour

SIR,—I welcome your publication of the Addington Labour Observation Chart and would like to present another example (see

fig.). This chart has been successfully used at Charing Cross Hospital for the past four years in Professor N. F. Morris's unit. It is part of the notes, which are in book form, and can be extended indefinitely with identical adhesive sheets (The gum is behind the right hand edge.)

LABOUR CHART

Name :

Contractions began at hours on

The main advantage is compactness, which has been achieved by including fetal heart, maternal pulse and blood pressure, and cervical dilation on the same graph. Clarity is retained by using red or black symbols and, rather surprisingly, the graphs do not often conflict. The time scale at the top is infinitely variable, permitting flexible spacing of observations, which is particularly useful during crises.

Considerations of space preclude the reproduction of the whole chart here, though I would be pleased to supply copies of this. The lower half includes space for recording the following: contractions (duration, interval, and strength); oxytocin (concentration and rate); drugs given (dosage); fluid intake (oral and intravenous); urine output (volume and abnormalities); and station of fetal head. The chart was designed to measure 8½ in x 11 in (21.6 cm x 28 cm) but, if applied to the new standard A4 paper, an extra ¾ in (1.9 cm) becomes available.