Correspondence

Letters to the Editor should not exceed 500 words.

A Surgeon's Duty

SIR,—Sooner or later many families are faced with the alarming and disquieting question: Has the operating surgeon told the facts about the operation, either to the patient or to his nearest relatives?

This is at present entirely a matter for the surgeon's judgment. I submit that there should be a policy laid down which would encourage the surgeon to tell intelligent and well-balanced patients whether, for example, their probable chances of life should be estimated in months or years.

My brother, Professor Haldane, has recently died of a secondary cancer. When he left hospital he was under the impression that he had at least another two or three years in which to initiate and complete various pieces of scientific work, deal with the affairs of his institute, etc. The lives and plans of a number of other people were involved; some of them feel bitterly about this. The surgeon writes that, though he made it perfectly clear to both my brother and his wife that he was available during my brother's convalescence, to discuss his future, they never broached the subject. In fact, his wife left England two or three months before my brother himself

died, clearly thinking that the operation had been successful. The operation, certainly, had been successful; what she did not know was that cancer had spread to the liver.

Now I am sure that the surgeon behaved absolutely correctly. But I must add that before my brother went into hospital it was particularly brought to the notice of the hospital authorities that he was the kind of person who ought to be told what was happening to him. Several responsible people tried to find out, but the pattern of medical hierarchy prevented this.

I would have supposed that there might have been some positive public duty on the part of the hospital or the surgeon to warn my brother, who had often faced death before. The time was bound to come when he felt he had been cheated and treated like a child. Doubtless it is wrong to tell unstable people that they are going to die within a few months; there may be a case for withholding such facts from the young. But after 60 we should all be prepared to hear such things and to face them with courage and equanimity. A new code is needed.—I am, etc.,

Campbeltown, Argyll. NAOMI MITCHISON.

The Scientific Method

SIR,—Sir George Pickering's brilliant Harveian oration on Medicine and Science (26 December, p. 1615) was evidently intended to provoke discussion, and it has accordingly provoked me to question his teaching that experiment is an integral part of the scientific method. This is the more strange as he says that science arose from medicine and astronomy; and in astronomy experiment is very little used. Its method, as recently illustrated by the checking of Einstein's hypotheses during an eclipse, is to compare the results of imagination with the results of observation, with no experiment in the sense of altering the course of events.

This distinction is of importance to me since I hold that the experimental method is inapplicable to the processes of human gestation, yet that by imagination and observation we can establish the nature of many important deformities with benefit to their treatment. However, when I try to argue on these lines it is obvious that most of my colleagues simply do not recognize the method I am using, and I am pretty plainly told that if I want to be taken seriously I should get busy with some rabbits.—I am, etc.,

London W.1.

DENIS BROWNE.

SIR,—Sir George Pickering deserves our thanks for his most stimulating paper "Physician and Scientist" (26 December, p. 1615). The points he made cannot be stressed too often. It is sad but true that failure to

apply the scientific method to medicine has resulted in the loss of considerable knowledge.

However, some comment is necessary regarding his discussion of the method of science. The latter indeed does consist of four stages, which are as follows: observation, hypothesis, experimentation, and induction (drawing conclusions).

The final and, in some ways, perhaps the most important stage of induction was given scant mention in his paper. He rightly emphasized that experiment is no more than planned observation; but the essential basis of the experiment is to provide a secure basis for making inferences afterwards. If I may quote R. A. Fisher: "New knowledge comes from experience by inductive inference and is the only way from which it can come."—I am, etc.,

Walton Hospital,
Liverpool 9.

JOHN T. ROSE.

Help for Old People in the Cold

SIR,—Following an article in the *Guardian* on Friday, 4 December 1964, on the value of storage heaters for protecting old people from hypothermia, an imaginative and generous offer has been made by the London Electricity Board.

A letter has been sent to the town clerk of every borough council in the Board's area offering to supply and install storage heaters at a special price in cases of hardship. In appropriate cases vouchers from the National

Assistance Board are accepted in payment for the electricity consumed. The Westminster Old People's Welfare Association is already experimenting with the use of at least one storage heater. The advantage of this form of warmth is that bedrooms are automatically kept warm at night, often the most difficult time for frail elderly people. The heaters are fitted with an automatic time-switch so that it is impossible for the consumer to switch off in a fit of economy. They always have a separate meter. As for the heater, it need never be the property of the old person concerned. It is virtually indestructible and can be moved to another home when no longer required.

There must be many of your members who know of cases which might be suitable for this kind of help. I hope that the London Electricity Board will receive many inquiries about their offer. I am sure that there will be a minimum of red tape on the part of local councils in arriving at a definition of "hardship."

In areas outside the London Electricity Board it might be useful if London's example were brought to public attention.—I am, etc.,

House of Commons,
London S.W.1.

LENA M. JEGE.

Renal-artery Stenosis

SIR,—The recent paper by Dr. C. J. Schwartz and Dr. T. A. White (5 December, p. 1415) was most welcome and confirmed the view of some with special interest that the relationship between renal-artery stenosis and hypertension is complicated and that renal arterial stenosis alone must be a rare cause of hypertension. Their report was based entirely on post-mortem material, examined from an unselected series of 154 necropsies. They found "no clear or consistent relation between the degree of stenosis and the diastolic blood-pressure levels in either males or females." They also noted "a not insignificant number of kidneys maintain weights within the normal range for each age group, in spite of severe multiple renal arterial stenoses."

In many papers on the subject of hypertension renal arterial stenosis has been illustrated in renal arteries leading to a small or scarred kidney, or in one artery of multiple arterial supply. It is our view that in many cases renal arterial stenosis is present coincidentally or additionally to other aetiological factors. Occasionally it may perhaps develop as a result of hypertension. The presence of multiple arteries alone is statistically associated with hypertension.¹⁻³ A kidney much smaller than normal must raise doubts about a developmental cause rather than an acquired renal arterial stenosis, and dysplasia or hypoplasia must be taken into account. Gross renal scarring noted in some material examined by Schwartz and White and thought to have a vascular basis but no clear relationship to renal arterial stenosis showed "considerable distortion of the normal pattern of cortical vessels"—surely similar to

the scars regarded by Marshall as developmental?⁴

We have always suspected and increasingly believe that renal arterial stenosis is rarely a primary cause of hypertension, though it may well aggravate existing hypertension. Although we have studied in detail over 400 patients in our hypertension unit and have completed angiograms for almost as many patients again from other hospitals, we have rarely considered that any arterial narrowing we noted was the principal cause of hypertension. In our own series we have found few patients in whom radiological evidence of arterial narrowing was associated with typical features of renal arterial stenosis—that is, with characteristic divided renal studies and convincing pressure drop across the stenosis. Even if such findings are present it is still necessary to exclude other factors such as chronic pyelonephritis.

We have actually only completed four renal-artery operations for renal arterial stenosis, which gives some idea of the infrequency with which clear-cut findings emerge. Of course, in many instances nephrectomy has been thought the more logical procedure because of coexistence of unilateral renal disease. We exclude from these observations those patients with some cause other than atheroma for the syndrome of renal arterial stenosis—such as fibromuscular dysplasia or extra-arterial compression of the renal artery. In such patients the local arterial lesion may well be the primary factor in the hypertension.

The clinical assessment of the significance of radiological renal arterial stenosis is most difficult, and in our view few patients have isolated renal-artery lesions wholly responsible for hypertension. It follows that local surgical measures may well be unsuccessful in relieving hypertension if there is some additional aetiological factor present such as a multiple arterial supply to the kidney. This is not to deny that occasionally there may be very successful cases in which hypertension is relieved by treatment of renal arterial stenosis, but we believe this to be a very small proportion of all patients with hypertension.

The recent paper has usefully outlined the lack of a clear-cut relationship between renal arterial stenosis and hypertension when approached from the pathologist's viewpoint: we find this view consistent with our clinical impressions.—We are, etc.,

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A. KLIDJIAN.
D. H. HULL.
M. L. DYSON.

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Plantar Warts and Athlete's Foot

SIR,—Some years ago at the time when my partner and I were treating all plantar warts by diathermy under a general anaesthetic, which was a time-consuming process, I decided to try and find out if plantar warts had anything to do with barefoot dancing and physical education, which was then coming

into vogue. After the first few weeks to my amazement I found the majority of children with plantar warts were from those schools that wore gym shoes for physical education but all the children were keen swimmers. Although I have no statistics to back up my opinion, I believe that barefoot dancing and physical education is not a cause of plantar warts. The virus I feel needs a water surface and a sodden foot in order to penetrate the plantar skin.

I was rather surprised at Dr. J. Reid (2 January, p. 59) writing that this was one of the commonest conditions seen at his skin clinic, as we very rarely refer any of these cases. This must I feel show the differing standard of general practice throughout the country, and the stimulus that a general-practitioner hospital (which we have) gives.—I am, etc.,

Tonbridge, Kent.

JOHN BARLAS.

What Should I Do?

SIR,—In the correspondence column (2 January, p. 62) Dr. M. J. Ball asks whether it is his first duty, when such a case comes his way, to protect a pregnant woman who is not being adequately cared for in accordance with current orthodox practice, or whether it is his first duty to be loyal to the responsible general practitioner, whom he describes as "a fellow practitioner."

We all meet this sort of problem sooner or later, and some of us meet it not infrequently. Those of us who have been worried by this will sympathize with Dr. Ball in his dilemma. Dr. Ball's uncertainty is due to the effect upon his clarity of thought of a shameful philosophy that is all too common in the medical (and nursing) professions: the philosophy that the first rule of medical ethics is loyalty to one's colleagues, one's firm, one's chief, or one's hospital, above all other considerations.

Since medical student days he, and I, and most of us have learned "medical ethics" primarily as a code of mutual protection of doctors against the lay world. We are now in the habit, quite inaccurately, of using the terms "ethical" and "unethical" in description of relationships between doctors when we are merely describing codes of professional etiquette. No doubt we all remember being taught that it is not done to steal each other's patients, or to put up one's plate for private practice opposite another doctor's surgery, but how many of us remember being taught anything of the moral and legal rights of patients? How many doctors qualified since 1947, for example, remember being taught anything about the Nuremberg Code?

Thus, the first real principle of medical ethics, that it is the patient who matters most, is forgotten, to the present ridiculous extent that, out of a misguided sense of loyalty, most doctors and nurses would shrink from disclosing malpractice, negligence, low standards of care, unethical experimentation, or various other abuses which they might observe practised by a colleague, or even make the slightest attempt to prevent repeated recurrence.

I am sure that Dr. Ball needs no advice as to where his moral duty lies—to the neglected patient or to the incompetent "fellow-practitioner." Yet in the same

situation many midwives and clinic doctors would consider it proper (and easier) to do and to say nothing, leaving the general practitioner to be answerable only to his own conscience and the patient's medical knowledge, limited though they both may be.

How can what is immoral be medically ethical in their interpretation?—because of this widespread misinterpretation of a doctor's ethical obligation to his profession, itself due to the common dangerous misuse of the word "ethics," which is still perpetrated in textbooks and at teaching hospitals.

If we were to restrict the term "medical ethics" to the description of a doctor's obligations to a patient, and use only the term "medical etiquette" to describe day-to-day relationships between doctors, then that which is ethical would be that which is moral. In such circumstances many doctors and nurses who in the past, seeing abuses committed by colleagues, have saved themselves trouble, effort, and conscience by pleading "medical ethics" as a barrier to action would now have to face moral issues.

Dr. Ball, however, having already tried to face the moral issue, would now have no problem of decision on a point of ethics; he would, of course, still have the problem of discreet execution of his decision—but on a point of etiquette only.—I am, etc.,

Rugby, Warwickshire. MICHAEL EDMONDS.

SIR,—Dr. M. J. Ball's dilemma (2 January, p. 62) could be solved by sending a copy of the haemoglobin result to the unbelievers together with a copy of the *Report into Maternal Deaths 1958-1960* and one of the *Perinatal Mortality Survey*.—I am, etc.,

Barnstaple, N. Devon. A. J. B. EDWARDS.

SIR,—It seems that Dr. M. J. Ball's problem and his *cri de cœur* (2 January, p. 62) would not have arisen had he confined himself to the work for which he attended the clinic or, indeed, had the patients' own doctors done the simple work of blood-sampling for which, presumably, they had contracted and were being paid as part of normal antenatal care. The short answer to the question is that the first duty in this case is to stop interfering in colleagues' work.—I am, etc.,

Uarán Mór,
Ireland.

S. UA CONCHUBHAIR.

Snuff

SIR,—As a heavy snuff-taker (more than an ounce a week) for more than 25 years I should like to take issue on one point in Professor D. F. N. Harrison's interesting paper (26 December, p. 1649), and also raise a query.

In my experience snuff-takers do not sneeze—at least, not as the result of taking snuff. This tolerance is acquired very quickly. So far from the beginner reaching this stage when "hypertrophy and then atrophy of the nasal mucous membrane" has developed, the habituation takes something more like two or three days. Should the snuff-taker try a snuff which is more peppery than those to which he is accustomed he will sneeze like anyone else, but, once again, a new tolerance is very rapidly acquired.