

hilar lymph nodes were involved. The histology of the main tumour showed oat cell carcinoma, while the removed subcarinal lymph node contained deposits of poorly differentiated squamous cell carcinoma. The patient's progress was uneventful, though he was left hoarse following damage to the left recurrent laryngeal nerve. He did not receive radiotherapy or cytotoxic drugs. He has been carefully followed up for 14 years, and despite the bad prognosis initially given, he has remained quite fit, continuing at work, and clinically and radiologically he has shown no signs of recurrence of the lung cancer.

Mr. Abbey Smith lays down the criteria for acceptance of the existence of this phenomenon of cure following incomplete surgical resection of cancer as biopsy confirmation, record of incomplete surgery, and necropsy confirmation of absence of cancer throughout the body. In the case described above, the first two criteria are fulfilled, but the third cannot be met since the patient remains alive after 14 years. It looks therefore as though the policy of Mr. Abbey Smith's unit of removal of the bulk of the tumour where complete removal is not possible, may at times—if but rarely—produce an unexpected successful result.—I am, etc.,

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Acid-base Balance and Bleeding

SIR,—The criticisms made by Mr. M. H. Irving (29 May, p. 529) of our paper on acid-base balance in acute gastrointestinal bleeding (1 May, p. 242) prompt us to clarify certain points raised in his letter.

We reported that transfusion with stored bank blood, which is known to have a high lactate concentration¹ and a low pH,² resulted in a rise in arterial blood lactate concentration in seven patients who were not clinically shocked either before or after blood transfusion. Mr. Irving suggests that this was because the patients remained in a "state of compensated shock", and points out that "a normal arterial blood pressure is no guarantee that hypovolaemia has been corrected". These patients were all transfused to a normal central venous pressure³ prior to the second blood sample, with the exception of one patient who still had a low central venous pressure at this time. Blood volume measurements were carried out in five of these patients, including the patient with the low central venous pressure, using ⁵¹Cr labelled red cells. All five patients had a blood volume well within the normal range,⁴ according to weight and sex, at the time of the second blood sample.

We also reported that patients who are clinically shocked may have a severe metabolic acidosis on admission. In one such patient we measured the blood pH again after transfusion of one litre of blood had reversed the state of clinical shock and raised the blood pressure to normal. There had been only a very slight increase in pH from 7.25 to 7.26, but a further increase to 7.43 occurred following infusion of 200 mEq sodium bicarbonate. After this further blood transfusion was continued. Mr. Irving suggests that "the actual volume of the 200 ml of sodium bicarbonate solution was possibly as significant in correcting the acidosis as the buffer itself." It seems unlikely that such a small volume would produce such a marked effect when no significant effect had

been obtained with one litre of blood. A further point against this explanation is that the blood lactate concentration remained very high at 128 mg/100 ml following the infusion of bicarbonate, as mentioned in our paper.

Mr. Irving complains that the authors "do not make it clear at what point they believe post-transfusion metabolic acidosis should be corrected. The implication, however, from the interval between their pre- and post-transfusion acid/base measurements is that it may be several hours after the cessation of blood transfusion." The measurements he is referring to here are those obtained in the patients who were not clinically shocked before or after blood transfusion, and we specifically point out that monitoring of acid-base balance "is probably unnecessary in patients who are not clinically shocked." We suggest, however, that it "is advisable in patients with acute gastrointestinal bleeding who are clinically shocked, especially if rapid blood transfusion is contemplated." We intended this to imply that these measurements should be carried out before or during the early stages of blood transfusion, while the patients are still clinically shocked. If a severe acidosis is detected, we would suggest correcting it immediately. We certainly did not imply that it should be corrected "several hours after the cessation of blood transfusion." Thus, in saying that his "main criticism of the paper is the suggestion that a *persistent* metabolic acidosis in shocked patients should be treated by the infusion of sodium bicarbonate" (our italics), Mr. Irving is misinterpreting our suggestion. We accept that eventually "the metabolic acidosis of hypovolaemic shock is self-correcting if adequate volume replacement is provided." Our point is that, in the acute situation of a patient in clinical shock with a severe lactic acidosis, the rapid transfusion of stored bank blood, with a high lactic acid concentration and a low pH, may

be dangerous unless the patient's blood pH is monitored.

In support of his opposition to the use of sodium bicarbonate, Mr. Irving quotes Schweizer and Howland's⁵ (1962) paper saying that the metabolic acidosis of shocked patients undergoing major surgery responds promptly to treatment of the hypovolaemia with acid bank blood. This paper has been superseded by their more recent publication on this subject,⁶ which is the one quoted in our paper. In this, they report an improved mortality rate with simultaneous infusion of alkali, and a similar improvement has been reported under more carefully controlled conditions in experimental animals with haemorrhagic shock.²

Mr. Irving further points out that sodium bicarbonate is "ineffective in treating the shock state." We did not at any stage suggest that it will correct the shock state, only the accompanying acidosis. We share his belief that correction of a low blood pH should not be allowed to divert attention from the need to correct hypovolaemia by blood transfusion.—We are, etc.,

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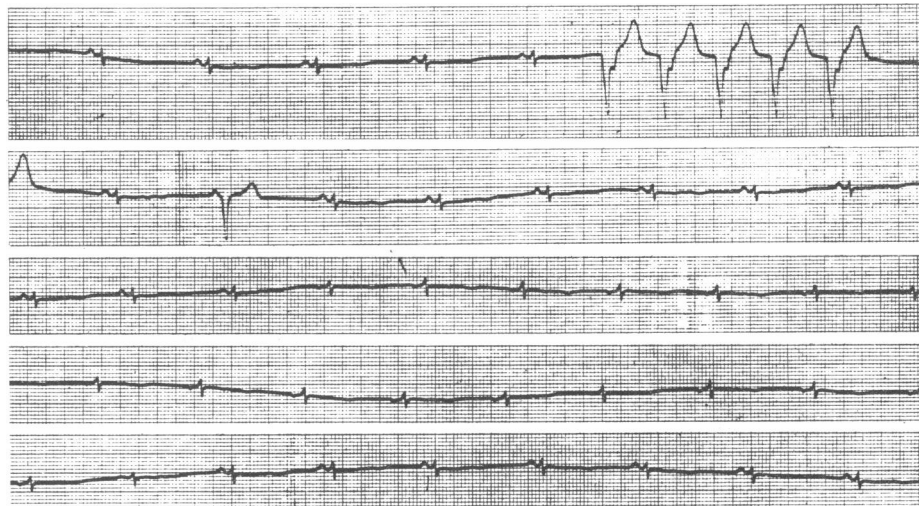
Myocardial Infarction and the G.P.

SIR,—Dr. L. Adamson (27 February, p. 505) warns against the use of prophylactic procainamide in early myocardial infarction in the absence of E.C.G. control.

In a recent report¹ on experience in a coronary care unit an E.C.G. record was presented which illustrates one of the hazards to which he alludes (Fig.). It demonstrates sinus bradycardia, a run of ventricular premature beats, reversion to

sinus bradycardia, and then alteration to nodal bradycardia—all occurring in a period of less than one minute in a patient with fresh infarction. The rapid transition from tachy- to brady-arrhythmia is not uncommon in the early stages of infarction and is impossible to detect clinically.

In this case atropine could have precipitated ventricular fibrillation or tachycardia, and procainamide block-syndrome or asy-



stole. Prophylaxis in the absence of E.C.G. control may well produce unnecessary deaths.—I am, etc.,

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¹ Horgan, J. H., *Journal of the Irish Medical Association*, 1971, **64**, 163.

Suicide and Euthanasia

SIR,—I am concerned that the opinions of Dr. S. L. Henderson Smith (10 April, p. 111 and 5 June, p. 591) appear to be passing unopposed although I am encouraged by Dr. D. Hooker's letter (5 June, p. 585). With some re-wording, the latter could equally well apply to the euthanasia problem.

How can we accept the view that euthanasia would not be immoral? If Dr. Henderson Smith can define and authorize in this way, why should not Tom, Dick, and Harry also do so? Who has authority to decide what is moral and what is not? Which religion condones this taking of life and calls it moral?

Dr. Henderson Smith is rightly concerned about the problem of terminal suffering, although I feel his language is exaggerated, and his "daily . . . appalling torture" is in sharp contrast with the view of your leading article (23 January, p. 187). The answer does not lie in euthanasia (in its current meaning) but is a challenge to our profession to ease these sufferings, without "striving officiously to keep alive." With all the resources of modern medicine, combined with a real concern and compassion, we must be in a better position than ever to do this. This is not a question of confusion of semantics; whether we kill or whether we encourage or assist another to take his own life, this is immoral.

It has been my purpose in this letter to emphasize an underlying principle rather than the many practical problems such as uncertainty of prognosis and selection of patients, or the possible late results and abuses which would inevitably follow. We can now look back on the abortion law reform, and I sense that most of us do so with some regret. Are we going to stand by while yet another immoral and degrading Bill is slipped past us?

While I respect Dr. Henderson Smith's worthy intentions and concern about terminal suffering, I fear lest we may deviate from what is right by confused thinking.—I am, etc.,

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X-ray Equipment

SIR,—At the meeting of the Radiologists' Group of the B.M.A. on 20 May one of the speakers put forward a suggestion of a panel of radiologists to advise the x-ray supply industry on the type of equipment required. One of the Department of Health medical advisory staff who was present promised to look into this and to consider setting up such a panel under the aegis of the Department.

The more I think about this proposal, the less I like it. It is obvious that the advice of such a panel would have to be available to

all companies interested in supplying x-ray equipment. I wonder if the proposer of this panel had such a wide dissemination in mind? With the "benefit" of support from the Department, I fear that the advice of this panel would be regarded as binding. This could lead to over-standardization of equipment and to delays in introducing new ideas; few committees are sufficiently "light on their feet" for such a changing field as x-ray equipment.

For it to be at all effective the panel would have to be small and it would have to meet fairly frequently. Could such a panel be at all representative? The size and the facts of geography would be against it. Further, most x-ray apparatus is used by radiographers, not radiologists; giving them adequate representation would create further problems over committee size.

To sum up, I think such a panel would probably be too small to be truly representative, but too large to be effective. It would lead to centralization and standardization and thus have a stultifying effect on the design of x-ray equipment.

Far from centralization in this field I think we need to bring the decision making and financial responsibility down to local level. Standardization has many benefits and we should try to evolve a system of purchasing x-ray equipment which encourages radiologists to buy standard equipment, but leaves latitude for purchase of modified or non-standard items where the local need demands it. I suggest that a hospital group should be allocated a number of "capital units" each year depending on the work done. Teaching and other special hospitals would have a loading factor depending on their particular requirements. Groups should be able to bank their units and earn interest or they could borrow on their "future earnings" and pay interest. Each year the Department should assess the cash value of a "capital unit." This calculation should be based on Hospital Equipment Note No. 6. *Diagnostic X-ray Department* (H.M.S.O. 1962), a reasonable work load per room, and an average expectation of life of apparatus. The radiologists could then plan their purchase of x-ray equipment over a period of several years in the light of the needs of the group.

The mechanics of the system would be quite simple. The unit system is already in use in x-ray departments for assessing work load; 100 notional units might well equal one "capital unit." This x-ray unit system is now in need of updating and it requires some standardization of application, but would nevertheless be a practical basis of assessing work load. The figures are already returned at group, board, and Department levels, so that little work would be involved in allocating "capital units."

Such a system would put the decision making and the financial responsibility at the same level, that of the user of the equipment; this would fit in with modern ideas of management. It would give manufacturers the chance to plan their production; the inability to do such planning was one of the complaints made by the speaker mentioned in my first paragraph; he hoped his panel would, among other things, help him to do this forward planning.—I am, etc.,

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Logical Foundations of Medicine

SIR,—Like most general physicians I feel too out of my depth to engage in a technical argument with the computer experts. Nevertheless, because I feel that the medical man in the street must remain in contact with the computer men, I wish to discuss certain aspects of the paper by Professor W. I. Card and Professor I. J. Good (27 March, p. 718).

I concede that computers have a logic of their own, but surely it is a rigid logic compared with the flexibility of the logical processes of the human mind influenced by experience. The computer can adapt to changing situations but it must be programmed to adapt. The human mind is adapting as it goes along. Further, in relation to clinical facts, the computer only performs with information ascertained by clinicians, as distinct from its capacity to perform directly in relation to response to laboratory data. Skill in obtaining pertinent clinical facts is often what makes the difference between the good and the poor diagnostician. The computer can take a structured history, but can it take a good situational history, one in which skilled questioning flows from the subtleties of interpretive listening to patients' answers? There is an assumption that computer use is exclusively scientific. I suspect that a great deal of it is as much an art as is the practice of medicine.

The computer is one of many medical helps which are being promoted as ways of saving the doctor's time, and much of this development is legitimate and desirable. But we are saving the doctor's time for what purpose? Surely so that he can spend more of it in the business of clinical communication with the patient. A clinical history is not only an instrument of diagnosis, it is often an instrument of therapy. Talking to someone is more personal and satisfying than talking to something. Even if a computer takes a history, no doctor will believe it in his heart until he has checked at least some aspects of it with the patient, and no patient will believe he has been properly listened to unless he talks to a person.

How can a dermatologist use a computer? What rigmarole of documentation can supplant the instant view of a skin lesion? Who needs a computer to diagnose chicken pox and measles? The problems in acute alimentary bleeding have to do with detecting the bleeding lesion, with the techniques of investigation and not with the niceties of differential diagnosis. It does not need two hundred questions on a piece of paper to raise the suspicion of a duodenal ulcer. It needs five or six questions and clinical common sense. A computer can be encyclopaedic but it cannot be sensible. It can be systematic but not sensitive. The fact that common things commonly occur does not mean that the uncommon is not what my patient has. Unfortunately doctors do miss the diagnosis of myxoedema, but can myxoedematous patients fill in forms correctly?

Computers may have improved weather forecasting but they have not perfected it. Low pressure areas shift unexpectedly, other weather systems appear from nowhere. And there is only one weather forecast a day. How do you organize two or three million health forecasts a day? This must need a lot of time, energy, machinery, and people. And apart from the question whether clinical