drum or in some way inferior in its thinking to that of the 'hard science' of hospital-based medicine, often seems to lead to attempts to erect an alternative edifice of research emphasizing, say, epidemiology or statistics. This talks the same language and carries some academic respectability. There is, of course, need for such work, and indeed for continuing expansion in the other discernible directions of modern general practice, training and postgraduate education. But we now have glimpses of a new sort of medicine, and if we have the vision to begin weaving together those threads that are appearing, they will provide the material for a new tapestry of medicine which will illuminate our practice in ways that we can, at present, only dimly, but with mounting optimism, anticipate.

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Reference

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Sir,

Having been slightly dismayed by Dr A. S. Dixon's recent article (June *Journal*, p.358) I feel I must make the following four points in reply:

1. Dr Dixon describes as a 'moot point' the degree of acceptance among linguists of Edward Sapir's hypothesis concerning the influence exerted on a society or culture by its language. I think he would find, in fact, that the vast majority of linguists are rather less favourably inclined towards this view than he suggests.
2. The main argument against Sapir's theory of language providing a grid through which its speakers see the world is that while it is indeed the case that different cultures make different distinctions in their languages, according to what is considered important in their respective societies, such differences in categorization do not prevent the implicit recognition of distinctions which are *not* made. The fact that some languages do not distinguish between blue and green in their colour terminology does not mean that their speakers see the sky and grass as being the same colour. The influence of society on language, however—the converse relationship—is much more widely accepted; it is surely as a result of his environment, rather than his state of mind, that the well-quoted Eskimo has so many words for snow.
3. I would also dispute rather strongly Dr Dixon's description of language as 'conservative'. There are innumerable examples of language changing quite readily to fit the changing needs of its speakers: the kinship terminology of Russian, for instance, changed quite drastically after the revolution to reflect the development of a different kind of family unit, and certain terms quite quickly became obsolete as new ones were found to be necessary.
4. Dr Dixon's view, then, that it is language which 'encourages the depersonalization of illness and the placing of disease beyond the patient's control' is surely rather a cop-out. He does, however, admit that it might be necessary, when trying to improve the current situation, to break through 'social and ideological rather than (the) linguistic or conceptual barriers'. Would it not therefore be more beneficial to that society to spend time trying to change its views of control of illness and shared responsibility between doctor and patient, rather than attempting to provide the medical profession with a language which will undoubtedly develop anyway in response to the changing needs of its speakers?

JUDITH HALLIWELL
Secretary to Stuart Fellow

Royal College of
General Practitioners.

Sir,

'Is the underlying grammatical construction of our language the reason . . . why we seem to have trouble with apparently vague processes and influences as represented by the psychological and social factors in our lives?'

The question certainly needed to be asked. We are surely indebted to Dr A. S. Dixon for tackling the subject with such erudition and scholarship (June *Journal*, p. 358).

Even the answer is provided. 'In natural languages . . . vocabulary, inflection and modes of sentence structure . . . do not make it impossible to express certain things, they (may) merely make it more difficult to express them.' Why then does Dr Dixon in his conclusion state three difficulties indicating that his answer to the initial question is an unqualified yes? His article itself contains two pointers to why this may be.

We in the medical profession do not use words fundamental to the philosophy of medicine with thought and consistency. For example in the first enumerated difficulty in his conclusion Dr Dixon implies that we might be more successful if we did not automatically consider diseases as 'its'. But surely the whole advantage of inventing (not discovering) any disease is that we then have a hard mental tool to use in healing people who are ill. Diseases are similar to Newton's Laws of Motion. As such they are as concrete as any abstract notions can be. It would appear diseases are invented specifically to be 'its'. I believe it would be clearer to argue that we should not consider all illnesses to be 'its'.

Let me return to a phrase from the article's fundamental question, namely 'apparently vague processes and influences'. Consider Dr Dixon's example of 'it rains'. This phrase did not prevent meteorologists from making great strides in the understanding of weather systems. Apparently vague meteorological processes and influences of 10 years ago are now elegant hypothesis shaped by Occam's razor.

In short, that our patients' illnesses seem vague is not due to any inherent defect in the English language but to our lack of knowledge. We in primary care would wait futilely for a grammatical revolution. What we need are our equivalents of the weathermen's sputniks and Kray II computers.

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Treatment of Heroin Addiction

Sir,

I read with interest the account of clonidine withdrawal in general practice by Joliffe and Melville (June *Journal*, p. 368). I agree that the prevalence of the problem, the inadequacy of the present clinic system and the imperative to encourage a sense of self achievement and responsibility by the patient, all lend support to a community primary care approach to the problem.

I have been treating heroin addicts with clonidine withdrawal for the past nine months. My treatment practice differs from that of Joliffe and Melville in that I do not insist on a fixed dose of clonidine. I explain the action of clonidine in as technically complete terms as is possible and allow the patient to titrate symptoms of withdrawal against over-sedation and drowsiness. Ideally, I ask the patient to keep a record of all