article (29 September, p 751)—never “give a moment’s thought to its precise definition” are perhaps wiser than they know. Disease is one of those words best not defined. The same is true of health. What could be more fatuous than the famous definition of health made years ago by the World Health Organisation: “Health is a state of complete physical, mental, and social wellbeing, and not merely the absence of disease and infirmity”?

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SIR,—Like Dr S Renfrew (20 October, p 1000), I think the word disease is one to avoid when possible. There is no such entity as a disease. The word disease is an abstract noun. In the medical context, we are really speaking of people who are diseased. A medical diagnosis is in fact incomplete unless it includes a description of both the figure to which our attention is drawn (the pattern of signs and symptoms, possibly embellished with aetiological assumptions) and of the background or context in which the figure is set (the person who has the disease). In psychiatry the figure is often the person himself (or his personality) and the background is his social context (the family or small group of which he is part). Professor Gerald Caplin has long taught the importance in psychiatry of the “environmental diagnosis.”

Of course, it is true that in practice we can often get by without making the environmental or background diagnosis explicit, but it is necessarily the case that what we call disease results from the interaction between figure and ground: there can be no disease without a person to manifest it. Discussion of the meaning of the reified concept of “a disease” is likely to be futile unless its abstract nature is recognised.

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SIR,—It is interesting to examine the basic concepts of medicine from a philosophical point of view. I fear, however, that the recent paper by Dr E J M Campbell and others (29 September, p 757) may have blurred some issues by unnecessarily complicating the definition of the term disease.

The distinction between a nominalist and essentialist approach to concepts is not necessary or relevant to the problem facing medical science. Some people may think of disease as something which can be caught, as a fisherman catches fish, because some illnesses are caused by infectious organisms, which do have an existence quite independent of their victims. On consideration, however, it is clear that diseases are phenomena that happen to living creatures. Any disease is a set of abnormalities. That is to say it is a set of properties of an individual. To try to distinguish between the existence of an individual and the existence of its properties, abnormal or otherwise, is unnecessarily complicated. It is the problem of the existence of functions or predicates and can be left to the philosophers.

The example of confusion in defining chronic respiratory disease demonstrates the need for agreement on the use of the term disease. The problem is not what is the nature of the concept disease but what sort of

abnormalities make up a disease. In terms of Campbell and others’ definition, what abnormal phenomena and common characteristics make up a disease? It is to this problem that we should turn our attention.

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On-demand analgesia equipment

SIR,—The recent letter by Mr Tom Lind and others (1 September, p 548) concerning the Cardiff Palliator we feel needs some further explanation.

Firstly, the Cardiff Palliator was designed in collaboration with the anaesthetics and medical physics department at the Welsh National School of Medicine and fully meets the safety regulations for medical equipment.

Secondly, with regard to thumbwheel switches, (a) the switch is in a relatively central and well-protected position of the front panel; (b) the setting of the thumbwheel switches is clearly displayed on a digital display immediately above the switch panel, and any change in switch setting when the machine is not infusing drug is immediately shown on the digital display; (c) any change in setting of thumbwheel switches when the machine is infusing drug has no effect on the size of increment given on that occasion; (d) the group of thumbwheel switches at the rear of the machine has been given a secure cover to guard against possible inadvertent setting.

Thirdly, the maximum total dose of drug the patient may receive is limited by the amount placed in the syringe. This is infallible.

Finally, we think that point three in the letter is fair comment. However, it would be fair to point out that this is the first commercially available machine of its type and must necessarily reveal ways in which the design could be improved. To this end provision has been made by providing electronic outputs at the rear of the machine for additional data collection, analysis, and display. I think additionally it would be worth mentioning that this is the only commercially available system purpose designed to be used with any analgesic regimen, in contrast to the Janssen machine, which will take fentanyl only.

Pye Dynamics, in conjunction with the Welsh National School of Medicine, is continually looking at ways of improving the Cardiff Palliator and the next generation could incorporate modifications, such as those mentioned, which can be discovered only by users in the market place.

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Hodgkin’s disease and occupational exposure to chemicals

SIR,—We note with interest the finding of Drs H Olsson and L Brandt (8 September, p 580) of a large proportion (19%) of men with Hodgkin’s disease who had been in employments exposed to chemicals. This has prompted us to examine the recorded occupations of cases in the North-western Regional Cancer Registry.

In the period 1962–1976 inclusive there were reported 558 cases of Hodgkin’s disease in males aged 16 and over resident in the south-east Lancashire and north-east Cheshire

conurbation (corresponding roughly to the present county of Greater Manchester). We have compared their occupations with those of 290 men with chronic leukaemia and 33,097 men with other neoplasms reported for the period 1972–5. Among the cases of Hodgkin’s disease, 21 (3.8%) were in occupational categories liable to chemical exposure, compared with 6.6% of the cases of chronic leukaemia and 3.9% of the group with other neoplasms. Moreover, the proportion employed in the chemically exposed occupations in the working male population of Lancashire,² an area that included a large part of the conurbation, was 8.6%. As in the Swedish findings, we have a bimodal age distribution, but we do not find any suggestion of a more marked association with occupation in those aged over 30.

Thus our data fail to confirm the Swedish report of a marked excess of occupational exposure to chemicals among men with Hodgkin’s disease. We hope that other population-based cancer registries will examine their data and report their findings.

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¹ Office of Population Censuses and Surveys, *Classification of Occupations 1970*. London, HMSO, 1971.

² Office of Population Censuses and Surveys, *Census 1971: Economic Activity, Lancashire*. London, HMSO, 1972.

ABC of blood pressure measurements

SIR,—The excellent article by Dr Eoin T O’Brien and Professor Kevin O’Malley contains one minor but important error (6 October, p 851).

Blockage of the air filter makes it hard to blow up the sphygmomanometer but it does not result in difficulty in controlling the rate of fall of the mercury. The latter is due to stickiness of the control valve. Most control valves are designed so that they cannot be dismantled and cleaned—if they could the top would always be falling on the floor during the performance of the measurement. Consequently the only cure for this disease is to throw away the control valve and buy a new one. Since they cost only about £1 this is unlikely to cause a financial crisis even in such over-RAWPed hospitals as Guy’s.

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SIR,—It is unfortunate that Dr Eoin O’Brien and Professor Kevin O’Malley in their article (20 October, p 982) on the technique of indirect blood pressure measurement should perpetuate the rheological absurdity of accepting the disappearance of sounds as the level of diastolic pressure.

The audible events occurring during the release of extramural compressions of the brachial artery are simple to explain on Reynoldian flow theory of critical velocity and the theoretical and logical reasons for the muffled end-point are overwhelming.

As long as the extramural pressure imparted by the cuff is above systolic pressure, the artery remains closed and no sound will be heard. At a pressure just below systolic there will be a jet of blood at systolic zenith creating the first

staccato sound. This point, taken as systolic blood pressure, is slightly lower than that by direct measurement, but is acceptably close, and there are no other sound criteria to confuse the issue.

As the extramural pressure is lowered further the proportion of the cardiac cycle during which the artery is patent progressively increases, causing the decrease in pitch and increase in length of the sound as it approaches diastolic pressure, but as long as this extramural pressure is above diastolic there will be a period, however brief, when the artery is closed and the sound will be staccato. As soon as the sound appears muffled this criterion no longer holds and this must be the closest approximation to the diastolic pressure, and direct measurement confirms this.

The disappearance of sound is in many cases close to the muffled point and can give a reasonable approximation of the diastolic blood pressure, but it is clearly illogical to suggest a correspondence of the two, for the following reasons. Firstly, in a hyperkinetic circulation, as the authors point out (for example, after exercise or the apprehensive patient in the examining room), sounds may be heard over the brachial artery even when the cuff pressure reads zero. Secondly, if the stethoscope bell is not at the point of maximum pickup over the artery, or in a fat arm, the sound will disappear at higher pressures and the same occurs if the stethoscope transmission is poor or the examiner's hearing impaired.

The continued recommendation of phase 5 as the diastolic pressure is, in my opinion, a grosser error than that of the almost universal practice of starting a pulse count on the unit as opposed to zero, thus negating the basic principle of interval timing.

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Neurological manifestations and *Mycoplasma pneumoniae* infection

SIR,—I would like to add a further case report to those given by Drs J Aidan Twomey and M L E Espir in their short report "Neurological manifestations and *Mycoplasma pneumoniae* infection" (6 October, p 832).

The patient was a 54-year-old woman who presented to the department of neurosurgery with a five-day history of atypical pneumonia and gradually progressive focal neurological signs suggestive of a left temporal lesion with a dense right hemiparesis. Investigations revealed a white blood count of $22.5 \times 10^9/l$ (87% neutrophils, 7% monocytes) associated with strongly positive cold agglutinins and a mycoplasma titre rising to 1/512. The patient had been started on antibiotics prior to admission and mycoplasma was not cultured from blood, sputum, or cerebrospinal fluid. Analysis of the cerebrospinal fluid showed normal protein, glucose, and cell counts and CSF IgG and IgA within the normal range. Computed tomography (CT) confirmed a large left temporal lesion of intermediate uptake but the brain biopsy specimen was histologically normal. The patient responded well to erythromycin, with complete recovery at her six months' follow-up, and a later CT scan showed resolution of the abnormal area.

I report the case as a neurological manifestation of mycoplasma pneumoniae presenting as a focal lesion resembling an acute glioma. I suggest that the underlying pathology is direct infection of the nervous system rather than a secondary immunological reaction. While agreeing with the authors that mycoplasma

involvement should be excluded in otherwise unexplained acute neurological symptoms I do not agree that its incidence is underestimated.

I am grateful to Mr K Frazer of the department of neurosurgery, University Hospital of Wales, Cardiff, for permission to report this case.

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Mycosis fungoides—unsolved problems

SIR,—Your leading article "mycosis fungoides—unsolved problems" (18 August, p 409) is an excellent review of the diagnostic and therapeutic problems of this intricate disorder. We would, however, dispute the conclusion: "Which of all of these treatments is used must depend partly on the individual case, but even more on the techniques and experience available in any one centre."

We feel that this attitude suggests a lack of collaboration and exchange of information between centres, which contrasts sharply with the activities of those involved in research in mycosis fungoides. The establishment of co-operative study groups in Scandinavia, the Netherlands, Scotland, and North America is a result of this interest. The international meetings on mycosis fungoides in Paris in 1977 and Washington in 1978 formed valuable platforms for the dissemination of new knowledge between these study groups and emphasised the value of our continuing collaboration in increasing knowledge and understanding of this rare disease.

OVE GROTH
LARS MOLIN

KRISTIAN THOMSEN

GUNNAR VOLDEN

Scandinavian Mycosis Fungoides Study Group
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RONA MACKIE

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Possible cancer hazard associated with 5-methoxypsoralen in suntan preparations

SIR,—In 1976 it was suggested¹ that some degree of caution should be exercised in the use of psoralens in the phototherapy of psoriasis "as somatic chromosome damage of this magnitude may result, after clonal selection, in later malignancy." Stern *et al*² has reported that the risk of cutaneous carcinoma in patients treated with oral 8-methoxypsoralen phototherapy for psoriasis was 2.63 times higher than expected for a matched control group. An accompanying editorial³ comments on the risks and benefits of phototherapy.

It is with some disquiet that I would inform your readers of what may be a serious problem to a very large number of young people using certain suntan preparations widely advertised and sold by pharmacies, without prescription,

in several European countries, including France. French men and women as well as tourists flocking to the warm beaches and to the ski resorts are, in my judgment, at risk. The suntan preparations sold contain, in many formulations, 5-methoxypsoralen as well as the usual ultraviolet chemical filters. This furocoumarin has, as one of its effects, the ability to induce tanning in the presence of light (320-380 nm). This is, of course, the major desired effect of 5-methoxypsoralen and thus such preparations will help tanning. The melanin pigments will thus decrease the harmful effects of prolonged exposure to solar radiation.

The other biological effects of 5-methoxypsoralen, however, are similar to those of psoralen (the parent chemical) and 8-methoxypsoralen. There is an abundance of data⁴ that psoralens, although differing quantitatively in their biochemical and biological effects, (a) form monoadducts and cross-links to DNA in the presence of light; (b) produce point mutations or chromosomal mutations (or both) in all cellular systems treated, from viruses to human cells; (c) produce skin cancer, in the presence of light, in mice; and (d) produce skin cancer (and possibly other cancers) in the presence of light in man. There is admittedly no direct evidence as yet that 5-methoxypsoralen is carcinogenic or mutagenic. There is nothing, however, in the chemical structure to indicate that it will behave in a qualitatively different manner from the other psoralens. Its reactions with DNA are discussed by Scott *et al*.⁴

It is with considerable uneasiness that I suspect an increase in skin and other cancers to be probable in future years from the widespread and, in my opinion, ill-advised use of preparations containing 5-methoxypsoralen. My concern is not helped by the knowledge that DNA repair mechanisms may be less than optimal in, perhaps, one person in 100.

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¹ Ashwood-Smith, M J, and Grant, E, *British Medical Journal*, 1976, **1**, 342.

² Stern, R S, *et al*, *New England Journal of Medicine*, 1979, **300**, 809.

³ Epstein, J H, *New England Journal of Medicine*, 1979, **300**, 852.

⁴ Scott, B R, Pathak, M A, and Mohn, G R, *Mutation Research*, 1976, **39**, 29-74.

Clinical practice and community medicine

SIR,—Your leading article (6 October, p 817) and Professor E D Acheson's article (p 880) on this subject raise issues which have quite often been discussed in the past. There appears to be nothing in principle against the idea of a community physician having a part-time contract with his employing authority, the remainder of his time being spent in a sub-consultant capacity in a clinical specialty. Many community physicians, though they may hanker after clinical work, find that their regular employment is sufficiently demanding. There may be occasions when an employing authority, in order to fill a vacancy, will be glad to agree to such an arrangement. There are, however, practical and formal difficulties to be overcome in order to provide work which is suited to a particular individual's skills and interests. For example, there may