We feel that Gram-stain examination and culture of the fluid is the best means of differentiating between infection of the peritoneum and aseptic peritonitis, two conditions that require different management.—We are, etc.,

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Reaction to D-Penicillamine in Rheumatoid Arthritis

SIR,—We read with interest the letter from Drs. J. Shafer and F. D. Hollanders (4 August, p. 289) and would make the following comments. Firstly, like Dr. C. F. Hawkins (18 August, p. 406), we would expect an exacerbation of symptoms in a substantial proportion of patients in whom all therapy had been withdrawn immediately before being offered D-penicillamine. There can be no doubt that a latent period before the drug becomes effective is the norm with this therapy. Moreover, we do not consider withdrawal of concomitant therapy until the patient begins to show laboratory evidence of improvement, often six to eight weeks from the start. This does not apply to previous gold therapy, however, for the more we have used penicillamine during the past six years the more uneasy we feel about the incidence of bone-marrow depression in patients on penicillamine who had had previous cryotherapy. We do not offer penicillamine to these patients any longer.

Secondly, Drs. Shafer and Hollander's reference to "the disorder of taste which is known to occur in one-third of patients on penicillamine therapy" is worthy of comment. In our series of 107 patients (the first of whom started treatment in 1966) we have had to withdraw only one patient because of loss of taste. A further 16 had transient impairment of taste—10 of them while receiving a dose above 1.2 g/day, as reported before.¹ The incidence of impairment of taste has a correlation with high doses of penicillamine, and almost all those unfortunate enough to experience it on a low dose can expect recovery (without interruption of treatment) within two months.—We are, etc.,

ALBERT T. DAY
JOHN R. GOLDING

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¹ Golding, J. R., Wilson, J. V., and Day, A. T., *Postgraduate Medical Journal*, 1970, **46**, 599.

Sex and Social Class

SIR,—Apropos your leading article (21 July, p. 121) on the use of family planning services in England and Wales,¹ clearly much more can and must be done to encourage women of all ages in social classes 4 and 5 to follow the example of their middle-class contemporaries in enjoying the benefits of existing family planning services. But more must be done to improve further and expand services if the needs of many communities and of the nation as a whole are to be fulfilled and these must be suitably publicized via the responsible local press and informative public notices.

My experience in Aberdeen is that women in the late 30s and 40s of all classes are invariably very appreciative personally of

family planning advice. Such women can be encouraged by the doctor to discuss the topic with their teenage daughters and can be provided with booklets² and informative material. In Aberdeen family planning education is included in mothers' and parents' club programmes, and in one working class area mothers bring their teenage daughters to family planning talks and discussions at a clinic on a council housing estate.

Certainly some diffident women may be deterred from attending clinics where they feel a lack of privacy—"everyone knows what you've come for." This can easily be overcome, as in Aberdeen, by provision of multipurpose clinics. One Aberdeen clinic is in a building complex with medical and dental clinics, with a local library adjacent and near to a shopping centre. No embarrassment here! Obviously accessibility is vital, with convenient location and opening times. However, the need is often unvoiced where advice is most necessary. Parents in one Aberdeen urban area requested a clinic after they had organized an opinion poll, yet this was a low-density housing area with a low birth rate and high motivation for family planning advice. No such request was received from the highest-density housing area with the largest number of births, but when facilities were provided the attendance soared so much as to require a busy clinic twice weekly. Clearly the general practitioner has a vital role to play. But G.P.s require to take more interest in follow-up and domiciliary cases. In this the G.P. has an invaluable colleague in the health visitor to co-ordinate efforts.

The numbers of abortions in older women might level off and decrease if there was more discussion of the option of sterilization for women in their 30s whose families are complete. (There were 17,500 terminations in women over 30 years in England and Wales in 1969.³) Such sterilization is often the method of choice for some women who find years of continuous pill taking too much of an organization. Abortion in younger women could also be made safer by greater efforts to encourage advice at earlier stages of gestation. A publicized, confidential pregnancy diagnostic service provided along with family planning services could improve this. The knowledge of facilities available to prevent unwanted pregnancy is, as you state, passed by word of mouth but they can also be effectively publicized, as in Aberdeen, by circulating informative brochures to industry and work places.

Basic to all improvements is the inclusion of sex education in a first-class health education programme. One idea is the benefit of social studies courses in schools, in which studies and visits are undertaken to various places to show what the community has to offer.⁴ The numerous facilities relevant to the well-being of young people should be included in such courses so that information is combined with knowledge of services and facilities and confidence in obtaining professional advice.—I am, etc.,

ELEANOR STEINER

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¹ Bone, Margaret, *Family Planning Services in England and Wales*, London, H.M.S.O., 1973.
² Pilkington, R., *Facts of Life for Children*, Barnes, K., *15+ Facts of Life*, London Family Doctor Publications, 1971.

³ Registrar General's Statistical Review for England and Wales for 1969, Supplement on Abortion. London, H.M.S.O., 1971.

⁴ Dallas, D., *Sex Education—Exploring Education in School and Society*, National Foundation for Educational Research in England and Wales, 1972.

Child Injury Intensive Monitoring System

SIR,—Drs. Geoffrey Diggle and Graham Jackson (11 August, p. 334) raise several important issues. They believe that a "realistic and accurate assessment" of the scale and nature of the battered baby problem can be made by a computer-based system working initially at local authority, regional, and eventually national level. They fail to mention, however, that several local authorities have already established confidential central registers concerning such children. Furthermore, medical personnel now have at their disposal a "request for information form" that provides information similar to the more elaborate scheme they propose.

The use of these forms in Birmingham has been recently assessed during the course of an investigation based on 134 cases admitted to hospital. The relevant form was made available at casualty departments and paediatric wards throughout the region and doctors were instructed that when confronted with a case they should complete the form and forward it to the social service department. Following this they received a prompt reply informing them whether the child had been previously battered or suspected of being battered. A list of the names, telephone numbers, and addresses of those previously involved with the child were also supplied. This procedure was approved by the Medical Defence Union as involving no breach of confidentiality. Nevertheless, of 134 cases, in only one-third was the form completed.

Perhaps, therefore, Drs. Diggle and Jackson's confidential files might be used if they are made available at paediatric hospitals rather than social service departments. However, it may still be erroneous to assume that their scheme will necessarily provide reliable statistics because there are still likely to be a number of doctors who fail to provide their computer with information. To rely on the discretion of casualty officers and others to provide relevant information seems rather optimistic in the light of experience. Even in the U.S.A., where mandatory reporting of battered baby cases has been in existence for a number of years, "conflicting estimates" still occur. Improvement could readily be made if doctors accurately recorded the correct diagnosis on the summary discharge sheet. This still too rarely occurs.

Even if reliable statistics are available, this does not necessarily imply efficient management of the battered baby syndrome. Indeed, management of the problem has in the past too often been plagued by a tendency to rely on the case conference on the assumption that discussion alone is in the best interests of the child. I would suggest, however, that this is extremely inefficient and would instead propose that a regional team, hospital-based, consisting of a paediatrician, psychiatrist, social worker, and psychologist, should be established to tackle the overall problem. This would circumvent the present system of leaving the management of

the battered baby largely in the hands of inexperienced field workers and perhaps go some way towards overcoming the present practice of returning these children home, where re-battering often occurs.—I am, etc.,

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Diseases in the Tropics

SIR,—I would like to question the relevance of Dr. W. Z. Conco's remarks about the diseases of civilization (11 August, p. 331). We are told that Dr. Conco "saw Africans not only with coronary heart disease but also with nearly 58% of the conditions listed by Burkitt and 66% of those listed by Gelfand as rare." This claim cannot be taken seriously until we are given some idea about the clinical criteria for making his diagnosis, the number of patients involved, and an estimate (at least) of the total number of patients seen over the same period. Presumably the age/sex structure of the population in a Zulu rural area was weighted towards women and children with a moderate number of elderly people. If these data are not available perhaps detailed case studies of these patients could be released.

The value of scientific medical terms as a means of communication with patients is doubtful even in the United Kingdom, as accurate medical histories can be obtained only by avoiding medical jargon. If a patient says that he has diverticular disease, toxoplasmosis, scabies, or migraine he should be asked about his symptoms and environment and then examined to permit formulation of a scientific diagnosis. It is irrelevant whether or not the patient understands the scientific medical term. Table I in Dr. Conco's paper hides the "English medical terms" he tested thus they cannot be evaluated, but tables II and IV suggest that the terms chosen were often diagnoses.

This publication reinforces my fear that our profession becomes trapped by a desire to use respectable medical labels. Clinicians must become more honest about the minimal criteria required before making a diagnosis. This approach is being tried in our department and it forces the clinician to use more lay terms, which often describe the clinical situation most accurately. Premature labelling with a pseudo-diagnosis is avoided while our "diagnoses" have accepted minimal confidence limits.

I find table III in Dr. Conco's paper interesting because here are some Zulu lay concepts which need detailed exploration with an open mind free from the shackles of Western medical jargon and dogma. For example, a detailed history of the patient's feelings when he is suffering from *umkhondo* ("trail disease") and study of a series of such patients could prove useful as an exercise in communication and may contribute new medical knowledge to the African scene. Awareness and analysis of lay and witch doctor descriptions of medical problems^{1,2} are surely the first steps towards resolving any communication difficulties of developing countries.—I am, etc.,

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- ¹ Ingle, R. F., *South African Medical Journal*, 1973, 47, 333.
- ² Stott, N. C. H., and Browne, W. M., *South African Medical Journal*, 1973, 47, 334.

Thalassaemia in the British

SIR,—The recent article by Dr. H. H. M. Knox-Macauley and others (21 July, p. 150) and the subsequent letter on this topic from Dr. J. J. Taylor (11 August, p. 353) have prompted me to record my experience on this subject in Canada.

Over the last eight years my laboratory has conducted a search for abnormal haemoglobin variants and for thalassaemias mainly in the provinces of Alberta, Saskatchewan, and Manitoba and has supplied a service to physicians in the last two provinces for the laboratory diagnosis of these conditions. The diagnosis of β -thalassaemia trait was made in 132 individuals in 105 families in Saskatchewan and Manitoba on the basis of of haemoglobin patterns, including determination of the HbA₂ and HbF levels, tests for raised erythrocyte osmotic resistance, and examination of erythrocyte morphology. Thirty-five of these persons (26.5%) were considered to be of British origin with no ascertainable history of recent intermarriage with persons of Mediterranean, African, or Asian origin. Nineteen of the 52 β -thalassaemics found in Saskatchewan and 16 of the 80 found in Manitoba were British. In all of them the HbA₂ levels were raised (mean 5.4%, S.D. $\pm$ 1.13% in the former; mean 5.1%, S.D. $\pm$ 0.73% in the latter; mean 2.2%, S.D. $\pm$ 0.40% in a control group), while HbF levels were normal in about half of these subjects and did not exceed 8% in the rest. These values are in agreement with those of Dr. Knox-Macauley and his collaborators and indicate that the same β -thalassaemia gene is probably involved in both series. The propositi in nine of the 25 British families with β -thalassaemia were detected during population studies. No instances of homozygous β -thalassaemia or of α -thalassaemia were found in British subjects.

These results suggest that at least 20% of all β -thalassaemias in central Canada are of immigrant British stock and that in nearly half of them the condition is not suspected clinically. I agree with Dr. Taylor that one of the commonest difficulties to be experienced in these cases is in convincing some colleagues of the diagnosis.—I am, etc.,

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Hyperamylasaemia in Diabetic Ketosis

SIR,—We were interested to note that Dr. A. H. Knight and his colleagues (21 July, p. 128) have confirmed our original observation¹ that the serum amylase is often considerably elevated in diabetic ketosis and that this phenomenon is not obviously due to pancreatitis.

We did in fact take the matter a little further by studying serial amylase levels during the course of oral glucose tolerance tests in 60 newly diagnosed and untreated diabetics and 52 controls. The curves in each group were summated and mean curves constructed.

The results indicated that the amylase level rose in normals but tended to fall in diabetics, and the difference was statistically significant. The rise in the amylase level following an oral glucose load in normal subjects could be explained in several ways. It could be that the pancreas is stimulated, leading to an overflow of pancreatic amylase into the circulation, or that oral glucose is initially taken into the liver and the glucose appearing in the blood could be due to breakdown of liver glycogen with overflow of liver amylase into the circulation. It is not apparent why the amylase should behave differently in diabetics. The very high amylase levels in diabetic ketosis could lead to a massive breakdown of liver glycogen, and it is even possible that the hyperglycaemia is a secondary phenomenon. In this connexion we have often wondered why the blood sugar should be so high in diabetic ketosis when the patient may have been ill for some days possibly with anorexia and vomiting, which should lower the blood sugar, particularly if the patient has continued to take insulin.

Several workers have doubted whether the serum amylase is simply a passive overflow and have speculated that it may have a primary physiological function. The evidence would now seem to be strong enough to conclude that there are definite abnormalities in amylase metabolism in diabetes, and elucidation of the mechanisms involved may throw some further light on the pathogenesis of diabetes.—We are, etc.,

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- ¹ Finn, R., and Cope, S., *Diabetes*, 1963, 12, 141.

Significance of *Pseudomonas aeruginosa* in Sputum

SIR,—Dr. M. W. Burns's article (18 August, p. 382) draws attention to a problem which is increasing in frequency not only in Australia but also in Britain. I would like to make a few comments.

Though there is a tendency sometimes towards unnecessary antibiotic therapy when *pseudomonas* is isolated from sputum, Dr. Burns seems to over-estimate this danger. The difficulty of assessing the significance of any organism cultured from sputum is well recognized, and *Pseudomonas aeruginosa* is no exception. The present method most commonly used in clinical bacteriology laboratories (in Britain) is an examination of the Gram film in conjunction with the findings on culture. In the absence of pus cells in the film, the isolation of *pseudomonas* from sputum cannot be regarded as a clear indication for antibiotic therapy. (The report from our laboratory always states whether or not pus cells are present in the specimen.) The 34 cases described without clinical infection, in which the organism was considered to be a commensal, would most likely be in the category of non-purulent specimen and the patient would not automatically be put on antibiotics. Dr. Burns does not seem to take the purulence of the specimen into consideration when assessing the importance or otherwise of the isolated *pseudomonas*.

Another point is that no mention is made of the in vitro sensitivities of any of the *pseudomonas* isolated. This would have been