butazone and other antirheumatic drugs and perhaps indicate that new drugs should be used with caution.—I am, etc.,

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Antibiotic Discs Active against Resistant Organisms

SIR,—Antibiotic discs that give no zone or a smaller zone than expected with sensitive organisms have been encountered in antibiotic-sensitivity testing by many laboratories. If the reduced activity is common to all discs in a vial or to a complete batch of discs the error should be detected by the finding of unusually small zones with control organisms or of unusual resistance patterns with some organisms. Even if errors due to reduced disc potency are undetected they are safe in that sensitive strains are reported resistant. The error is unwelcome in that the choice of antibiotics suitable for therapy is reduced. Either such discs were impregnated with less than the correct amount of antibiotic or the antibiotic has deteriorated under adverse storage conditions. The latter is probably the reason in most cases. A more significant type of abnormal disc is that which gives a zone typical of a sensitive strain with a resistant organism. Without making any effort we have found three examples of this during the past 18 months.

(1) A strain of *Staphylococcus aureus* with a minimum inhibitory concentration of tetracycline of 128 μg/ml gave a zone of 15 mm with 10-μg tetracycline discs, while there was no trace of inhibition with 30-μg tetracycline discs. Five discs from one vial were found to give a similar effect. The inhibitory substance was not identified. The Oxford *Staphylococcus aureus* NCTC 6571 gave zones equivalent to a diameter of 36 mm with these discs in tests by the Stokes method.

(2) A strain of *Staph. aureus* with an erythromycin M.I.C. of >128 μg/ml gave a zone of 27 mm with 5-μg erythromycin discs. No trace of inhibition was found with 15-μg erythromycin discs or with 5-μg erythromycin discs from vials from the same or different batches as the faulty discs. All the discs from two vials gave this effect. The inhibitory substance was not identified. The Oxford control gave a zone diameter of 27 mm with these discs.

(3) With two strains of *Staph. aureus* with sulphamethoxazole M.I.C.s of 128 μg/ml and 256 μg/ml respectively zones of up to 18 mm diameter were recorded by several laboratories using one batch of 25-μg sulphamethoxazole discs. No trace of inhibition was observed when the same strains were tested with other batches of 25-μg sulphamethoxazole or with 250-μg sulphasomidine discs. The Oxford control gave zones of around 25 mm with these and other batches of 25-μg sulphamethoxazole discs. Differences in the antibiotic sensitivity patterns of the sulphonamide-resistant organisms that gave zones with these discs suggested that the inhibitory substance might be a penicillin. When tests were repeated adding a drop of Burroughs Wellcome penicillinase diluted 1/20 to the area of the disc the inhibition was eliminated. It therefore seems likely that the contaminating substance was a penicillin. To obtain an estimate of the amount of penicillin contaminating the discs different amounts of benzylpenicillin were added to discs from a normal batch of 25-μg sulphamethoxazole discs and the zones compared with the zones given by the contaminated discs in tests with the sulphonamide-resistant organisms. Zones similar to those obtained with the contaminated discs were given when 0·05 μg of benzylpenicillin was added to normal discs.

All of these unusual discs appeared to contain the antibiotics with which they were labelled but were contaminated by another antibiotic, or perhaps some other chemical substance. Over-impregnation with a particular antibiotic is unlikely as this would be reflected in increased control zone sizes. The incorrect sensitivity will be seen only when an organism is sensitive to the contaminating agent but resistant to the drug labelled on the disc. Sensitive organisms, including the usual control organisms, will give zone sizes typical of sensitive organisms, as did the Oxford control in the examples given above. Thus in routine sensitivity testing detection of false sensitivity caused by discs containing a contaminating antibacterial agent is unlikely.

The frequency with which such discs reach routine laboratories cannot be estimated. The onus lies on manufacturers to act on reports of faulty discs; but it is unlikely that such discs will be detected by their quality control procedures unless, by chance, one of their control organisms has the appropriate sensitivity pattern. However, it is worth while knowing that faulty discs of this type can find their way into circulation.—We are, etc.,

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Coping with Minor Casualties

SIR,—It is a pleasure to see a leading article (2 March, p. 339) dealing with basic aspects of this major problem. Effective methods of managing minor casualties unrelated to injury, and injuries not requiring hospital treatment, are absolutely essential to improvement in the hospital treatment of all the injured. An apt definition of a "minor" emergency is something that happens to other people.

In spite of the emphasis placed on it by the Platt Committee[1] and in other reports since, insufficient attention has been paid to solving this part of the problem. To relate this to shorter working hours for doctors is irrelevant, like saying that shorter hours for firemen would mean no night fire service. It is a matter of priorities and organization. Your reference to casualty attenders as "the rag-tag-and-bobtail of the medical case load" is a clear indication that attitudes which have plagued attempts to find solutions to this complicated problem have not changed.

Regional boards and others are currently authorizing the expenditure of sizeable amounts of money in an attempt to provide roadside medical care of the injured so that they can be brought more efficiently into many ill-equipped, understaffed "casualty" departments where, but for the lack of funds, the standard of treatment could be greatly improved.— I am, etc.,

J. C. SCOTT

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1 Central Health Services Council, Standing Medical Advisory Committee, *Accident and Emergency Services: Report of the Sub-committee*. London, H.M.S.O., 1962.

SIR,—Rightly your leading article (2 March, p. 339) emphasizes that relatively minor complaints, to the extent of some 50% of the case load, serve to overburden hospital casualty departments. With these attenders I have considerable sympathy in the light of the prevailing difficulty for such sufferers to make immediate contact with their general practitioner; a rigid appointment system for surgery attendance promotes all the greater inducement to visit the casualty department. A remark once made to me by the secretary of the Casualty Surgeons' Association was illuminating—"It is our privilege to see the people who come here, rightly or wrongly, for they are all in need of help." On many occasions inquirers have been advised by me that, in their dilemma, the hospital is the best venue for them.

In essence, the over-crowding mainly befalls the casualty officer on duty and his staff. Again correctly, your article stresses that he should be untrammeled on the employment of his special skills in the care of serious accidents and other emergencies. To liberate him from the excessive demands made on his time by trivialities I have found that the solution lies in the employment of the most junior medical member of the staff as eliminator. In the hospital service this duty cannot be delegated to a nurse or receptionist; some members of the public are too litigious for this risk to be taken. In the industrial scene the attitude of attenders is different and the medical centre of a factory, staffed by trained nursing personnel, can efficiently discharge this duty. My scheme is essentially geared to peripheral general hospitals.

The young man is positioned, during hours of peak demand, in an apartment adjacent to the main examination room. His duty is to appraise all ambulant attenders who are not transparently in a serious state. He gives such attentions as are necessary—for example, simple dressing, antitetanic measures in vogue, simple removal of eye foreign bodies, etc. He has been well briefed in human relations and he is now required to explain in kindly and courteous terms the further course to be followed, be it referral to G.P., to the casualty officer-in-chief, or elsewhere. This junior is of course available for any other duties required by his seniors. Now that we have established the propriety of consultant status for the head of this department such a young man is assured of his future promotional opportunity and will readily agree to this ostensibly rather lowly function.

Television and newspaper exhortation to trivial attenders will have little or no effect in reducing their numbers at the accident centre, justifiably regarded by the public as the place from which it is their right to seek advice at any time.—I am, etc.,

DANIEL LAMONT

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Corneal Sign in Neonates

SIR,—In resuscitating neonates we have noticed in a few a haziness of the cornea immediately at delivery which has cleared within a few hours. Most of these babies have required intubating and ventilating, often for more than 15 minutes, before establishing spontaneous respirations. The subsequent course of these babies has been one of hypotonicity for a few hours followed by hypertonicity, irritability, and in some cases convulsions. In retrospect there has been evidence of marked intrapartum hypoxia.

We have not seen haziness of the cornea

immediately after an uncomplicated delivery, and we think its presence is an indication of prolonged intrapartum hypoxia. We wonder if others concerned with the care of the newborn have had similar experience.—We are, etc.,

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Milk Powder Malnutrition

SIR,—I share the doubts which Dr. A. E. Ifekwunigew and Professor D. B. Jelliffe (9 February, p. 246) express about the wisdom of "breast milk substitute." I wish to place the issue in its wider context.

There is growing recognition of a considerable potential hazard to child health in developing countries. An increasing number of mothers are being persuaded by baby food advertisements to feed their infants artificially. In rural areas particularly, mothers cannot afford to buy sufficient to nourish their babies adequately,[1 2] they are unable to understand instructions on the tin about preparation of the feed,[3] and they lack adequate equipment for sterilization of feed and utensils.[4 5] The result is an increase in infant mortality and morbidity from malnutrition and infection[2] which is made immeasurably more disturbing because it is wholly avoidable.

The misleading effects of sales promotion activities by milk companies[1] can only be condemned in the strongest possible terms. In Ibadan a study[6] showed that mothers believed milk powder to be superior to breast milk. Milk company representatives (who include trained nurses) promote their products in maternity and postnatal clinics.

Given the political will-power, this hazard to infant health can be avoided by two simple steps: (1) government action to ban advertisement and limit sales of milk powder to prescription alone; and (2) education as to the superiority of breast-feeding.—I am, etc.,

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1 *New Internationalist*, 1973, Nos. 6 and 8.
2 Plank, S. J., and Milanesi, M. L., *Bulletin of the World Health Organization*, 1973, **48**, 203.
3 Goldschmidt, B., *South African Medical Journal*, 1966, **40**, 71.
4 King, M., Morley, D. C., Burgess, H. J. L., and Burgess, A., *Nutrition for Developing Countries*. London, Oxford University Press, 1973.
5 Morley, D., *Paediatric Priorities in the Developing World*. London, Butterworths, 1973.
6 Social Policy Research, Ltd., *Infant Feeding and Health in Ibadan*. London, S.P.R., 1973.

Bibliography on Breast-feeding

SIR,—For the past few months we have been trying to draw up a recent bibliography on breast-feeding. This will be used in planning strategies intended to try to prevent the decline in breast-feeding, especially in developing countries.

As part of this bibliography we have been particularly trying to include any documented examples of actual programmes which have been initiated in any part of the world, on a small or larger scale, in an attempt to increase the prevalence of breast-feeding in a community or to prevent its decline. This seems to us to be a very difficult matter to document and I am therefore writing to you to ask if any readers can give references to such programmes available in the literature or information based on personal experience.

I would very much appreciate any assistance in this regard and look forward to receiving information.—I am, etc.,

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Diagnostic Test for Multiple Sclerosis

SIR,—As the worker in the Medical Research Council Demyelinating Diseases Unit who made the actual cytopherometric measurements which underlie both the work reported by Professor E. J. Field and others (9 March, p. 412) and the letter from Dr. J. B. Foster and others (p. 452), may I be permitted to point out that all measurements in both sets of experiments were done "blind" on scrambled and numbered bottles as described in detail by Caspary and Field.[1] The differences are therefore attributable to differences in the totality of experimental conditions. I have complete confidence in the figures obtained during the strict observance of technical protocol which I knew to be in force while Professor Field was in charge of the organization of the work. The major differences at the time of the "blind" trial referred to by Dr. Foster and his colleagues ("blind" in so far as scrambled specimens were scrambled once again) would appear to be as follows.

(1) Failure to observe full animal-house precautions against exposure of normal animals to droplet immunization with common viral antigens especially during an "influenzal" period. The basic importance of this has been repeatedly stressed[2-5] and indeed Field[3] pointed out the probability of such conditions interferring with the linoleic acid test. Even the observance of such a simple precaution as obligatory wearing of a mask in the animal house was in abeyance during the "blind" trial. On the principle of Occam's razor one need go no further in seeking an explanation of the alleged discrepancy. However, there are certain other sources of error which may have been operating.

(2) During the period of the "blind" trial it proved difficult to obtain lymphocytes from several blood specimens brought in, sedimentation with our long-established technique being poor. The cause of this was never established (and I am not aware that any note of such aberrant specimens was made). It may, however, have been related to the fact that it is not clear that all detergents were strictly excluded from glass washing—something which used to be enforced against a certain opposition.

(3) During this period, too, some "normal" guinea-pigs in the colony yielded peritoneal exudates which showed "spontaneous" sensitization to PPD by the macrophage migration inhibition test being carried out by another worker (something which would have been anticipated if (1) had been operative).

(4) Animals which had been injected with PPD in Freund's adjuvant were kept immediately adjacent to normal guinea-pigs being used for harvesting peritoneal exudate—something absolutely forbidden under the previous regime. Because of reflux of inoculum there was every opportunity for PPD powder insufflation into the normal animals to occur

(5) No record appears to have been kept as to whether patients were or had recently been suffering from "influenza" or "colds."[2 5]

I would add that (like Professor Field) I have never had an opportunity of seeing the actual experimental records (though shown the worked-out results). It would have been helpful if, when the protocol for the "blind" trial was drawn up, known cases of multiple sclerosis had been included as "positive markers" as is standard practice, for example, in carrying out Wassermann tests. This would have enabled a discrepancy to be spotted at an early and perhaps correctable stage.

Finally, Dr. Foster and his colleagues have departed from the customary practice of presenting results with complete experimental detail in full. This is all the more regrettable since differences of this sort more commonly than not stem from variation in experimental technique (sometimes quite subtle) and are resolved when full technical details become available, as happened, for example, only recently with the Bendixen method.—I am, etc.,

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1 Caspary, E. A., and Field, E. J., *British Medical Journal*, 1971, **2**, 613.
2 Field, E. J., and Caspary, E. A., *Lancet*, 1972, **1**, 963.
3 Field, E. J., *British Medical Journal*, 1974, **1**, 245.
4 Field, E. J., Caspary, E. A., Shenton, B. K., and Madgwick, H., *Journal of the Neurological Sciences*, 1973, **19**, 179.
5 Thompson, E., Lewis, C. M., and Pegrum, G. D., *British Medical Journal*, 1973, **4**, 709.

Epidemic Neuromyasthenia

SIR,—The account by Dr. M. J. Dillon and others of an epidemic of neuromyasthenia among the staff of the Hospital for Sick Children, Great Ormand Street (23 February, p. 301) disturbs me. The authors say that they considered the question whether the disorder was "functional," by which they presumably mean psychogenic, and that they limited the number of investigations in order to avoid creating an atmosphere of fear in a vulnerable population. Did these scruples prevent them from seeking psychiatric opinion on diagnosis and treatment and making any psychiatric inquiries other than noting the number of patients who had previously had psychiatric disorder, the number who appeared depressed, and the response to antidepressants? Or did they, in the physically mechanistic style of the last century, deliberately turn away from the holistic approach to concentrate on a minute search for an infective agent?

At the simplest one would like to know of the morale of the student nurse population, who contributed 71% of the patients. How long had they been at the hospital, were they homesick, and how old were they? Apart from the patients who were considered to be depressed, what was their mood and particularly was there "belle indifférence"? There is no mention in the list of symptoms and signs of any alteration of consciousness, and the authors' lack of comment on this point is remarkable because if the illness had an infective aetiology one would have expected disturbance of consciousness in some of the 145 patients. What little psychiatric information there was would have been made more meaningful by a few details, especially about the failure of antidepressives. Which ones did the authors use, in what dosage, and for how long?