—for example, skimmed milk powder—may be necessary within or in excess of the stated calorie value of the diet.—I am, etc.,

HELEN S. BUTLER

Singleton Hospital, Swansea Senior Dietician

¹ *Wheat Chemistry and Technology*, ed. I. Hlynka, p. 81. St. Paul, Minn., American Association of Cereal Chemists, 1964.

² Department of Health and Social Security, *Recommended Intakes of Nutrients for the United Kingdom*, Reports on Social Health and Medical Subjects N. 120. London, H.M.S.O., 1969.

³ Ministry of Agriculture, Fisheries and Food, *Household Food Consumption and Expenditure 1969*. London, H.M.S.O., 1971.

Undergraduate Curriculum

SIR,—Dr. G. E. Crawford's letter (14 October, p. 116) indicates that some people may be under the misapprehension that a main objective of departments of general practice is to influence undergraduate students to become general practitioners.

The principal objective is to broaden an undergraduate's experience of medicine and to let him see at first hand clinical situations which cannot be presented to him in hospital. It is true that in gathering that experience the student may come to believe that general practice is an attractive career. Such a judgement would surely be fortuitous. Training for general practice per se is, as Dr. Crawford's letter states, a postgraduate subject.

I know of no department of general practice that is not concerned with research in and for general practice.—I am, etc.,

H. W. K. ACHESON

Manchester

The Porn Industry

SIR,—Congratulations on your leading article, "The Porn Industry" (30 September, p. 779). The kind of common sense (and sometimes the humour) that is expressed in your leading articles is something for which I envy British doctors.—I am, etc.,

R. WITZMANN

Munich, Germany

Heart Failure and Neonatal Hypocalcaemia

SIR,—The interesting association of heart failure and neonatal hypocalcaemia described by Drs. O. Troughton and S. P. Singh (14 October, p. 76) raises some important points. Some of the earlier reports^{1,2} of oedema in association with neonatal tetany described its occurrence in the first few days of life. Many of the babies were very ill and diverse factors probably accounted for both the hypocalcaemia and the oedema.

In a recent study³ of hypocalcaemic tetany in bottle-fed babies occurring 5-11 days after birth 6 out of 18 babies had bilateral pitting oedema of the feet in the absence of other features of heart failure. The oedema was associated with dilutional hyponatraemia and there was probably an increase in total body water. It was suggested that the high sodium load of the cow's milk diet together with a transient functional immaturity of the kidneys were aetiological factors. Though Drs. Troughton and Singh do not state the type of milk their babies were receiving it

is unlikely that the diet was of aetiological significance in the development of oedema, as this sign developed within 48 hours of birth in several of the babies.

Unless it is appreciated that oedema in association with neonatal hypocalcaemia is not uncommon there is the danger that heart failure will be over-diagnosed. Many of these babies will also have hypomagnesaemia. Under these circumstances I would question the safety of digoxin therapy. Though the role of potassium deficiency in producing digitalis toxicity has been appreciated for many years, comparatively little is known about the effects of magnesium deficiency on the tolerance to digitalis in man. It has been shown in animals that acute depletion of serum magnesium decreases the dose of acetylthioflavine needed to produce toxic arrhythmias,⁴ and chronic depletion of magnesium prolongs the duration of digitalis toxicity.⁵ Magnesium-deficient mice given ³H-digoxin by the intraperitoneal route have higher myocardial and plasma levels of ³H-digoxin after one hour than control mice.⁶

Until more is known about digitalis therapy in the face of magnesium deficiency I would suggest that when the diagnosis of heart failure is open to doubt digoxin should be withheld and a short-acting diuretic be given in addition to calcium and magnesium supplements. In those babies who are obviously in heart failure parenteral magnesium sulphate should be given prior to digitalization.—I am, etc.,

MALCOLM L. CHISWICK

St. Mary's Hospital, Manchester

¹ Kehrler, E., *Archiv für Gynäkologie*, 1913, 99, 372.

² Shannon, W. R., *Archives of Paediatrics*, 1929, 46, 549.

³ Chiswick, M. L., *British Medical Journal*, 1971, 3, 15.

⁴ Sellar, R. H., et al., *American Heart Journal*, 1970, 79, 57.

⁵ Kleiger, R. E., Seta, K., Vitale, J. J., and Lown, B., *American Journal of Cardiology*, 1966, 17, 520.

⁶ Goldman, R. H., Kleiger, R. E., Schweizer, E., and Harrison, D. C., *Proceedings of the Society for Experimental Biology and Medicine*, 1971, 136, 747.

Dentist's Libel Action

SIR,—I am most grateful to you for reporting (4 November, p. 308) my evidence in this regrettable libel action so fully and so accurately, making my position clear. Your report, however, contains one small error. When I referred to the Birmingham team's measuring device as being so sensitive that it might easily have shown exactly the same changes with a person snoring in bed at night, I was referring not to the E.E.G., as your report seems to suggest, but to the device used for detecting respiratory obstruction.

Another tiny slip, perhaps due to an error in the transcript of my evidence, is to be found near the foot of your first column, where you quote me as having said that there was rapid recovery, but that the patient had to be kept hot for an hour or more. What actually I said I cannot remember, but it was certainly not hot.—I am, etc.,

J. G. BOURNE

Salisbury, Wilts

*The phrase "the patient has to be kept hot" was indeed in the official transcript of Dr. Bourne's evidence.—Ed., *B.M.J.*

Leucocytes in the Faeces

SIR,—I feel that your leading article on leucocytes in the faeces (14 October, p. 62) should not pass without comment. Reference was made to the investigations of J. C. Harris and others, who administered intestinal pathogens (*Salmonella typhi* and *Shigella* species) to volunteers.

From their article one finds that the volunteers were "informed adult inmates of the Maryland House of Correction." I expected that a journal of the *B.M.J.*'s reputation would discuss the ethics of this study as well as applaud the results.—I am, etc.,

ARTHUR RUSHTON

Aberdeen Royal Infirmary, Aberdeen

¹ Harris, J. C., DuPont, H. L., and Hornick, R. B., *Annals of Internal Medicine*, 1972, 76, 697.

Multiple Factors in Leukaemogenesis

SIR,—The work of I. D. Bross and N. Natarajan,¹ quoted in your leading article (21 October, p. 128), raises some disturbing thoughts in the preventive medicine sector. Could it be that the leukaemogenic potential of the attenuated (vaccine) measles virus is in the same class as that of its rogue (wild) cousin? Is there any likelihood that early immunization against measles in close temporal association with x-irradiation would increase the risks of leukaemogenesis?

Perhaps these are unworthy thoughts. Perhaps someone has already studied and dismissed these possibilities. If such is the case I would like to be reassured. Otherwise it may be desirable to withhold immunization against measles, for example, from children who received radiation antenatally and to be even more reluctant to x-ray children who have been immunized against measles in the recent past.—I am, etc.,

J. K. ANAND

Ilford, Essex

¹ Bross, I. D., and Natarajan, N., *New England Journal of Medicine*, 1972, 287, 107.

Gastric Acid Secretion in Chronic Renal Failure

SIR,—I was interested to read the paper by Dr. P. S. Ventkateswaran and others on gastric acid secretion in chronic renal failure (7 October, p. 22), and am grateful to them for pointing out the distinction in the paper of Fillastre *et al.*¹ between volume of secretion and hydrogen ion concentration. However, some of the statements Dr. Ventkateswaran and his colleagues make need clarification.

They say that there is an increased incidence of peptic ulceration in patients with chronic renal failure. Certainly the proportion seems to be high, but in order to say it is increased the incidence must be related either to a random sample of the population of equivalent age and sex distribution or to a similar group of patients with chronic severe illness but not renal disease. In addition, having stressed that gastric secretion is different in males and females, they do not say whether the control group and renal failure group were matched for age and sex.

The third point is the interesting effect of dialysis. Again we may ask whether the

dialysis and non-dialysis group were comparable for age and sex and whether the apparent differences in small numbers of patients was statistically significant. We studied the immediate effect of dialysis and found no significant difference in the peak acid output measured in the same 14 patients before and after dialysis,² but on reviewing our data we find that the basal acid output was significantly reduced after dialysis ($P < 0.05$). The peak acid output measures the parietal cell mass and perhaps its sensitivity to pentagastrin, but the changes in basal acid output, though less reproducible, may reflect the changes in urea and electrolytes produced by dialysis.—I am, etc.,

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¹ Fillastre, M. P., Blaise, P., Ardaillou, R., and Richet, G., *Revue Française d'Etudes Cliniques et Biologiques*, 1965, 10, 180.
² Gordon, E. M., Johnson, A. G., and Williams, G., *Lancet*, 1972, 1, 226.

Late Onset Psychosis

SIR,—On reading Dr. I. Kolvin's article (30 September, p. 816) about emotional problems in childhood and adolescence I was puzzled by an apparent lack of logic in the fourth of his five points of difference between infantile psychosis and the psychosis of later childhood. Dr. Kolvin states that "the low rate of schizophrenia in the parents of infantile psychotics and the significantly high rate in the psychosis of later onset suggests a genetic connection between late onset psychosis and adult schizophrenia." This statement is misleadingly structured.

Presumably the first part of the statement means (though this is not entirely clear from the text) that the parents of infantile psychotics have been found to have a low rate of schizophrenia whereas the parents of children with psychosis of later onset have been found to have a high rate of schizophrenia, the difference between the two groups being large enough to be significant. The inference of a connexion between psychosis of later onset in the child and adult schizophrenia in the parent follows from the high rate observed in this group. On the evidence only of the different rates in the two groups there is, however, no logical sequence in the suggestion that the connexion is "genetic." It might equally be an "environmental" link (exposure to a common factor or the effect of the adult's disorder on the child).

The structure of the statement was probably produced in an attempt to summarize the findings of a number of studies in one sentence, but the result is both confusing and misleading.—I am, etc.,

ELIZABETH WARREN

Edinburgh

Fungal Contamination of Bacteriological Swabs

SIR,—Drs. A. D. Bremner and Milcent H. S. Bell (21 October, p. 175) draw attention to the presence of fungal mycelia in polyvinyl sponge employed in cervical smears. I draw attention to another potential source of confusion during microscopy. In my experience some batches of commercially

available bacteriological swabs contain yeasts without hyphal forms, despite their sterility. These yeasts may be overlooked during casual microscopic inspection of purulent material, but caused us confusion in eye and vaginal swabs.

If yeasts are seen in stained films but cannot be cultured the confusion is invariably resolved, in my experience, by examining stained films of a small quantity of saline in which an unused swab has been vigorously stirred.—I am, etc.,

R. H. GEORGE

Birmingham Maternity Hospital,
Edgbaston, Birmingham

Urinary Incontinence in Women

SIR,—Your leading article on incontinence in women (23 September, p. 717) confirms that this is a difficult and confused subject. For those not actively engaged in its investigation we would like to offer further news.

Firstly, the good news. The fundamental abnormality underlying the symptom is surely the result of bladder neck incompetence. The normal bladder neck mechanism is securely stress competent. If it becomes incompetent stress continence depends on the efficiency of the distal mechanism, and, unlike that of the male, this cannot be relied on—partly, at least, because the bulk of the voluntary extrinsic sphincter, once known as the "compressor urethrae," is pathetically small in the female. The concept of the "posterior angle" has been escalated to mythology and must surely be laid to rest. Its goniometric absence may indicate that the bladder neck is unlikely to be competent but its presence, particularly when surgically reproduced, is by no means evidence that it is functionally occlusive, let alone stress competent. Fortunately there are better ways of assessing bladder neck function.

The bad news is that the problem of stress incontinence is often much more complex than we have been taught and perhaps thought. Incompetence of the bladder neck may result from its weakness, but an entirely normal mechanism may be rendered incompetent as a result of the "open sesame" detrusor contraction associated with detrusor instability and provoked by simple cough or posture change. The clinical potential of the specific instability is evident but its precise importance both in relation to diagnosis and to treatment remains to be defined and detailed prospective studies are under way.

Significant detrusor activity associated with the trigonal/bladder-neck opening mechanism in the female is not easily measured. Diagnostic detrusor pressure changes are often minimal and often completely masked on a total-bladder-pressure record. They currently necessitate measurement by electronic subtraction (total bladder pressure minus intra-abdominal pressure). Smooth muscle myography presents problems. The new electronic nappy for objective evaluation of the timing and extent of leakage, may prove important in assessment. It is to be hoped that it will be possible to simplify these relatively sophisticated investigations for wider clinical use as their importance becomes more clearly defined.

In the meantime we agree that the actual

symptoms of urgency and incontinence do not always correlate accurately with detrusor instability and, furthermore, that a clinical history and examination can no longer be regarded as sufficiently objective for the critical evaluation of various forms of treatment. The management of overt detrusor instability remains a major problem. We have not achieved the excellent results of drug treatment, reported by Moolgaoker and others,¹ in which 15 out of 17 patients seem to have been cured or improved with anti-spasmodics, sedatives, or ephedrine. The results of surgery may also be somewhat disappointing, for though some patients with detrusor instability may be helped by operations which improve urethral function the longer term results may not be so encouraging.—We are, etc.,

E. P. ARNOLD

A. D. G. BROWN

J. R. WEBSTER

P. H. L. WORTH

ANNE M. JEQUIER

C. P. BATES

C. G. WHITESIDE

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¹ Moolgaoker, A. S., Ardran, G. M., Smith, J. C., and Stattworthy, J. A., *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1972, 79, 481.

Future of Child Guidance Clinics

SIR,—Despite the fact that the reorganization of the National Health Service is imminent it seems that the future of the child guidance clinics is still uncertain. Though child guidance clinics have practised community and family therapy by casework long before its importance was recognized by other medical and social agencies, and child psychiatrists created well over 50 years ago the concept of team-work which is now the basis of the new social services, little attention to the future of this service and the status of its staff has been given in the Seebohm Committee's recommendations or in the Green Paper and Consultative Document.

It seems that health, education, and social services are competing for this valuable prize, but history clearly connects child guidance and psychiatric services for children with the health and education departments. It was my privilege as a representative at the Special Representative Meeting of the B.M.A. in Leicester 1971 to put an amendment on behalf of the Hendon Division which emphasized the essentially medical nature of child guidance, which was unanimously accepted.¹ It has also been suggested that diagnostic and therapeutic clinics should be separated and organized by the education and health services respectively, but this would, in my view, be a mistake, since diagnoses and treatment form integral parts of the same process of helping disturbed children. However, I am sure that despite their very close co-operation the separate areas of competence and organization of child guidance and school psychological services will have to be maintained.—I am, etc.,

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¹ *British Medical Journal Supplement*, 1971, 3, 105.