giving the long trailing end-points were... morphologically abnormal and mostly dead" and "the few viable abnormal forms... assumed normal morphology when transferred to trimethoprim-free medium." The latter observation supports our view that, in the context of practical chemotherapy of chronic infections, these strains must be regarded as drug-resistant. Promotional literature relating to trimethoprim/sulphamethoxazole mixtures stresses their bactericidal activity. This may be so under controlled conditions *in vitro* but, as pointed out elsewhere^{1,2} the relapse of infection usually suffered by patients with chronic haemophilus respiratory infections after treatment with such mixtures is entirely similar to that following bacteriostatic drugs. The discrepancy can be explained by the poor penetration of sulphamethoxazole into respiratory secretions,³ which has the result that response to treatment is effectively the response to trimethoprim alone—a bacteriostatic drug. In such circumstances Dr. Bushby's demonstration of the ability of even a few organisms exposed to high concentrations of trimethoprim to revert to normal forms in subculture is entirely compatible with our belief that these forms must be regarded as drug-resistant.

(4) Dr. Bushby refers to his experience of the response to trimethoprim of mice infected with our strains. However, little significance can be attached to such studies in the context of chronic human infection, because mice, like other laboratory animals, are susceptible to infection by noncapsulated *H. influenzae* only when injected with enormous numbers of organisms—a situation bearing scant resemblance to the natural situation.

We have no doubt about the value of trimethoprim in the short-term treatment of many patients with acute infections or acute exacerbations of chronic ones, but it has been disappointing in the bactericidal therapy of chronic infections of the respiratory tract. As with other drugs with a similar limitation a natural consequence may be a tendency to prescribe the drug for prolonged periods in individual patients. We feel that a logical conclusion to be drawn from our findings is that trimethoprim should not be used in this way in respiratory disease, or indeed be prescribed at all without a clear indication for it, such as failure of response to ampicillin or tetracyclines. The emergence of resistant organisms provides a warning that indiscriminate prescribing may result in the loss of effectiveness of the drug in situations where at present it is often of great value.—We are, etc.,

J. ROBERT MAY
JUDITH DAVIES

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¹ May, J. R., *Postgraduate Medical Journal*, 1972, 45, November Supplement p. 94.

² May, J. R., *Chemotherapy of Chronic Bronchitis*, 2nd edn., p. 63. London, English Universities Press, 1972.

³ Reeves, D. S., *Journal of Clinical Pathology*, 1971, 24, 430.

Fashions in Infant Feeding

SIR,—Your leading article (30 June, p. 722) drew attention to the lack of published information on the composition of the urine of infants fed high solute loads. The following results were obtained from normal controls during a study of urine composition in infants with heart failure. The infants were

TABLE I—Urinary Solute Concentrations (Mean $\pm$ S.D.) in Infants Fed on Different Feeds

Feed	No. of Infants	Sodium (mEq/l.)	Potassium (mEq/l.)	Urea (mg/100 ml)	Sodium (mEq/l.) Creatinine (mg/100 ml)
S.M.A.	9	20 $\pm$ 11	28 $\pm$ 10	504 $\pm$ 451	1.5 $\pm$ 1.0
Non-humanized milks . .	12	49 $\pm$ 40	80 $\pm$ 45	1,493 $\pm$ 861	1.7 $\pm$ 1.2
Weaning diet	18	57 $\pm$ 48	60 $\pm$ 38	1,117 $\pm$ 642	2.2 $\pm$ 1.6

aged 11-314 days and were receiving Similac (S.M.A.), non-humanized cow's milk, or weaning diets. Results are from random urine specimens collected between 2 and 4 p.m. Urinary sodium, potassium, urea, and creatinine concentrations were measured by AutoAnalyzer and urine osmolality with an Advanced osmometer.

As shown in Tables I and II urinary sodium, potassium, and urea concentrations and urine osmolality were significantly higher ($P < 0.02$) in infants fed non-humanized milk or weaning diet than in those fed on S.M.A.

TABLE II—Urine Osmolality of Infants fed on Different Feeds

Feed	No. of Infants	Urine Osmolality (mosmol/l.)		
		Mean	S.D.	Range
S.M.A.	8	236	121	91-401
Non-humanized milks . .	10	544	285	97-1,000
Weaning diet	9	532	259	167-1,000

The normal urinary concentrating power of infants fed high-protein diets is 1,200-1,400 mosmol/l.¹ Since two infants in this small group were passing almost maximally concentrated urine, it is easy to speculate upon the likely effects of extra water loss.—We are, etc.,

H. RICHARDSON
CYRIL CHANTLER

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Guy's Hospital,
London S.E.1

¹ Edelmann, C. M., *Pediatrics*, 1973, 51, 854.

SIR,—Dr. R. K. Oates (30 June, p. 762) has done a service in pointing out so clearly the faults that commonly occur in preparation of infant feeds. Of particular importance in his series is the fact that of 90 mothers using dried milk formulae nearly one-third prepared milk that was too concentrated or too dilute. Experience has shown that this kind of error is compounded by variations in mixing instructions that occur with different brands of dried milk.¹ Quite apart from mothers, it is clear that doctors, health visitors, students, and nurses become confused by the lack of standardization.

Recently the South African Paediatric Association, in conjunction with the South African Metrickation Board, has managed to persuade all dried milk manufacturers in South Africa to adopt a standardized dried milk dilution as from 1974. For unmodified milk formulae the dilution is to be one 5-ml teaspoon (or scoop) to 25 ml of water, and for modified milk formulae it is to be one scoop (size determined and provided by the manufacturer) to 25 ml of water. All feeding bottles are to be standardized to 250-ml size with 25-ml divisions. Instructions

to mothers can then be simplified to one teaspoon or scoop per division on a bottle whatever milk powder is used. It is hoped that this innovation will reduce errors of excessive concentration and dilution.—I am, etc.,

J. D. L. HANSEN

Department of Paediatrics,
University of the Witwatersrand, Johannesburg

¹ Hansen, J. D. L., Leary, P. M., Anderson, J. H., and Robertson, I., *South African Medical Journal*, 1970, 44, 1312.

Testing for Tay-Sachs Carriers

SIR,—May I ask for the hospitality of your columns to offer a diagnostic service?

Tay-Sachs disease is a fatal condition, death occurring after about three years of distressing illness. It is due to autosomal recessive inheritance, so both parents must be carriers. If we could identify them, amniocentesis could be done between the 14-16th weeks, in time to offer abortion if the fetus had the disease. Carriers can now be detected as their blood contains amounts of the relevant enzyme (hexosaminidase A) which lie between those of affected infants and of non-carriers. The carrier state is 10 times as common in Ashkenazi Jews as in the general population, so there is a good argument for starting a testing programme among Jews. This has been done in Baltimore, Washington, Toronto, Montreal, and other North American cities.

We have set up a laboratory for such testing and we would be glad to visit groups (in synagogues, colleges, etc.) to obtain 5-ml specimens of blood. By appointment individuals can visit this hospital for testing; relatives of affected children will be particularly welcome. The only exception is of women more than 17 weeks pregnant—that is, too late to permit cell culture in time for safe termination, but still with many weeks to worry in if both her and her husband's tests are in the danger zone.—I am, etc.,

PHILIP EVANS

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Great Ormond Street,
London, W.C.1

Laparotomy during Pregnancy

SIR,—While agreeing with Drs. Peter Saunders and P. J. D. Milton (21 July, p. 165) that the risk of precipitating labour following a negative laparotomy during pregnancy is extremely small, it seems illogical that the additional surgical procedure of removal of an easily accessible normal appendix should increase the fetal mortality so dramatically. Furthermore, three fetal deaths in seven patients are small figures on which to base such a

broad conclusion as that "there is no justification for removing a normal appendix in pregnancy". This has certainly not been the recent experience in Oxford.

Of 75 patients operated on for suspected acute appendicitis in pregnancy in Oxford, the diagnosis was found to be correct in 56. In this latter group four fetal deaths occurred and two patients went into premature labour but were delivered of infants who survived. In a further patient the appendix was the site of gross decidual endometriosis undergoing degenerative changes, which was thought to be the cause of the patient's symptoms. This patient also miscarried. However, there were no fetal deaths or cases of premature labour in 18 patients in whom the appendix was normal or fibrotic and was removed.

Most reports in the recent literature¹⁻⁵ dealing with larger series of patients are in agreement with these figures. It would thus seem reasonable for any surgeon or obstetrician faced with negative findings at laparotomy for suspected acute appendicitis in pregnancy to remove the appendix, provided it is readily accessible, as any subsequent attempts at appendicectomy may be rendered more difficult by adhesions resulting from the original laparotomy.—I am, etc.,

D. R. A. FINCH

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¹ Kurtz, G. R., Davies, R. S., and Spoul, J. D., *Obstetrics and Gynecology*, 1964, 23, 528.

² Lee, R. A., Johnson, C. E., and Symmonds, R. E., *Journal of the American Medical Association*, 1965, 193, 966.

³ McCriston, C. C., *American Journal of Obstetrics and Gynecology*, 1963, 86, 593.

⁴ O'Neill, J. P., *Australian and New Zealand Journal of Obstetrics and Gynaecology*, 1969, 9, 94.

⁵ Thomford, N. R., Patti, R. W., and Teteris, N. H., *Surgery, Gynecology, and Obstetrics*, 1969, 129, 489.

Possible Hazard of Polymethyl Methacrylate

SIR,—I was interested to read of the cases of bladder carcinoma and "cystitis of unknown aetiology" reported by Dr. R. Routledge (24 February, p. 487) in workers making polymethyl methacrylate contact lenses.

I have recently seen a patient with a bladder carcinoma adjacent to intrapelvic cement (polymethyl methacrylate) following a Charnley total hip replacement.¹ The patient had developed symptoms of increased frequency and nocturia three months after hip replacement following three years of disabling osteoarthritis. After failure of conservative measures to control his "prostatitis" cystoscopy was performed and a semi-solid tumour (transitional cell carcinoma W.H.O. Grade III) was found at the site of displacement of the bladder by the cement of the arthroplasty (see fig.). The rest of the bladder was normal.

A possible relationship between the intrapelvic cement and the bladder carcinoma could be explained on a physical or chemical basis. However, tumours produced in experimental animals by implantation of methyl methacrylate and other polymers have usually been sarcomatous,¹ while urothelial tumours caused by mechanical irritation have followed intravesical introduction of foreign bodies.²

That no similar cases have been de-

scribed may be correlated with the length of time necessary for polymer breakdown and excretion of breakdown products¹ but the latent period in this patient could have been shortened by heavy smoking and high analgesic intake, both described as having an association with urothelial malignancy.³⁻⁵—I am, etc.,

ROBERT D. WINES

Lane Cove,
New South Wales, Australia

¹ Oppenheimer, B. S., Oppenheimer, E. T., Danishefsky, I., Stout, A. P., and Eirich, F. R., *Cancer Research*, 1955, 15, 333.

² Mobley, T. L., Coyle, J. K., Muayad Al-Husaini, and McDonald, D. F., *Investigative Urology*, 1966, 3, 325.

³ Lea, A. J., *Lancet*, 1966, 1, 590.

⁴ Angervall, L., Bengtsson, U., Zetterlund, C. G., and Zsigmond, M., *British Journal of Urology*, 1969, 41, 401.

⁵ Taylor, J. S., *Medical Journal of Australia*, 1972, 1, 407.

Kidney Donation and the Law

SIR,—Your legal correspondent (11 August, p. 360) certainly does not lack the courage of his convictions. Considering the number of conflicting opinions that he quotes, his final sentence—"It (the Human Tissue Act 1961) is not as difficult as many suppose"—is a statement as remarkable as his certainty regarding its interpretation.

I am not concerned in this letter to argue whether his interpretation is right or wrong, though I certainly question his wisdom in expressing it so emphatically. My purpose is twofold. First, in an area of admitted ambiguity, it is regrettable that your legal correspondent should have thought it relevant, or for that matter desirable, to criticize the Medical Defence Union for passing on to its members the best legal advice it could obtain, even going so far as to attribute to the M.D.U. an opinion which he knew full well was that expressed by leading counsel (in fact two leading counsel on two separate occasions). In parenthesis it is curious that he should have chosen to express this criticism just when that opinion had been supported by Sir Keith Joseph.

Secondly, I do not see what good purpose would be achieved by pursuing his suggestion that "the time has now come for the M.D.U. to reconsider the wording of the Act". In the absence of a judicial decision this could result in nothing more than yet another legal opinion, of which there is already a plethora.

What is necessary is what five members of the MacLennan Group, including its chairman, recommended¹ and what I myself urged in a letter published in the B.M.J.²—namely, an amendment of the Act to remove the ambiguities.—I am, etc.,

PHILIP H. ADDISON
Secretary,

Medical Defence Union,
London, W.1

¹ *Advisory Group on Transplantation Problems*. London, H.M.S.O., 1969, Cmd. 4106.

² Addison, P. H., *British Medical Journal*, 1968, 1, 516.

**Our legal correspondent observed that "The M.D.U. was advised by leading counsel. . ."—Ed., B.M.J.

Kidney Donor Cards

SIR,—In your leading article (28 July, p. 189) you mention kidney donor cards and regret the lack of publicity about them. May I suggest that one of the pages for endorsements in all future driving licences be omitted and replaced by a statement signed by the new licensee and his or her next of kin offering to give his or her kidneys in the event of a fatal accident. This document is generally available on the person of the victim(s) and it could also so easily carry the blood group and the G.P.'s telephone number.—I am, etc.,

R. A. C. MCINTOSH

Sturry, Canterbury

Redesign of Medical Records in General Practice

SIR,—I refer to Dr. L. D. Church's letter (28 July, p. 234). In 1971 I studied records in general practice with the assistance of an Upjohn Travelling Fellowship, and the outcome was an A5 record envelope. A synopsis of this report is to be published in the Journal of the Royal College of General Practitioners shortly. In essence, my findings were that a new record system was essential and any change should adhere to the New International Paper Sizes. In practical terms this meant that the new record would either be A5, the size of the ordinary hospital letter, or A4, the size of a large hospital letter or foolscap size. It was my view that A5 in a double-wallet record form and guesstimated would be ideal for practice. I was dismayed to learn that the A4 size had been accepted in principle by the Department of Health and Social Security as these new records will be large, bulky and unwieldy. Less than 5% of patients have large dossiers.

Now that the profession has had an opportunity of considering these record sizes I hope that the Department will reconsider the situation and choose the smaller A5 record. Doctors using a lateral filing system would find that the A5 record can be filed without any difficulty or any changes being made to existing shelves.—I am, etc.,

A. J. LAIDLAW

Worcester

SIR,—It can be assumed that the advocates of A4 records in general practice write good notes. It is difficult to believe they write bad