were in the larger hydroceles and I would certainly recommend that this form of treatment be reserved for hydroceles less than 500 ml unless the patient's general condition precludes surgery.

It is clearly important to have long-term follow up in this type of study and I plan to continue reviewing the patients I have treated.

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¹ Moloney GE. *Br Med J* 1975;iii:478-9.

² Nash JR. *Br J Surg* 1979;66:289-90.

Beta-blockade, myopathy, and thyrotoxicosis

SIR,—The report by Dr J C Forfar and others (24 November, p 1331) on proximal myopathy and myositis associated with the use of beta-blocking agents prompts us to call attention to another clinical situation leading to myopathy during beta-blockade.

We have recently treated three male patients, aged 36-57 years, who developed thyrotoxicosis during treatment with the beta-blocking drugs timolol, pindolol, and propranolol. All of them showed chronic thyrotoxic myopathy, and one was in addition suffering from chronic diarrhoea. On examination, two of them were considered clinically euthyroid. There was no nervousness, hyperkinesia, or tremor. The third patient had some thyrotoxic symptoms such as increased sweating and occasional palpitations. The pulse rate of the patients was 80-90 beats/min, and all showed regular sinus rhythm. The most prominent finding in all the patients was wasting of the proximal muscles of the extremities. The muscle power, however, was only slightly impaired.

Apparently, the diagnosis of thyrotoxicosis was delayed in two of the patients. The symptoms had lasted from one to six months, and the patients had lost 10-16 kg before their thyroid function was tested. One of the patients underwent extensive gastroenterological examinations because of chronic diarrhoea of six months' duration. In another patient the diagnosis of thyrotoxicosis first became evident when the drug treatment was interrupted because of a suspicion of an uncommon side effect of beta-blockade.

All the patients showed unequivocal biochemical hyperthyroidism, with serum total thyroxine concentrations of 296-323 nmol/l (23-25 mg/100 ml); the normal range is 60-160 nmol/l (4.7-12.4 mg/100 ml). The myopathy was corrected after the treatment of thyrotoxicosis in each patient despite continued treatment with the beta-blocking agents.

Electromyographic abnormalities of some degree have been found in 90% of thyrotoxic patients, but clinically evident myopathy is infrequent. Beta-blocking agents may improve the muscle power in thyrotoxic myopathy,² and indeed in our patients the muscle power was rather good in relation to the severe degree of muscle atrophy. However, in the report of Dr Forfar and his colleagues the myopathy and myositis were ascribed to treatment with the beta-blocking agents. It remains an assumption that there might be some additive effects of thyrotoxicosis and beta-blockade on the myopathy in our patients.

Little attention has been paid to the development of thyrotoxicosis during beta-blockade.³ This coincidence is probably rather common, because both treatment with beta-blocking agents and thyrotoxicosis are common clinical conditions. Beta-blocking

agents mask many of the most typical clinical signs of thyrotoxicosis, such as tachycardia, tremor, and nervousness, without affecting the underlying metabolic disorder, except the deiodination of thyroxine to triiodo-thyronine.^{4,5} On the other hand, the development of thyrotoxic myopathy is not prevented by beta-blockade. Thus thyrotoxicosis presenting with atypical clinical symptoms and signs is probably the most common aetiological factor to be considered in proximal myopathy developing during beta-blockade.

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² Pimstone N, Marine N, Pimstone B. *Lancet* 1968; ii:1219-20.

³ Shenkman L, Podrid P, Lowenstein J. *JAMA* 1977; 238:237-9.

⁴ McDevitt D. *Postgrad Med J* 1976;52 suppl 4:157-61.

⁵ Anonymous. *Br Med J* 1977;ii:1039-40.

Combined antidepressant treatment

SIR,—Dr J P R Young and his colleagues (24 November, p 1315) are to be congratulated on having carried out the first controlled comparison of combined antidepressant therapy. Their results show clearly that trimipramine was superior to combined antidepressants and monoamine oxidase inhibitors alone.

The authors express surprise at these results because they do not accord with their clinical experience that combined antidepressant therapy is effective in depressive disorders. This might be because the patients in the study differed from those normally treated by combined antidepressant therapy in clinical practice in that they suffered from mild and moderate depression only. Combined antidepressant drugs are normally reserved for patients who have failed to respond to other forms of treatment. While this should not be regarded as a prerequisite for starting combined antidepressant therapy, such patients usually suffer few unwanted effects while on combined therapy because of tolerance to previous and often prolonged single-antidepressant medication. Marked unwanted effects during combined antidepressant therapy was noted in Dr Young's study and this is likely to hinder drug compliance and clinical response.

The tendency for unwanted effects to be experienced more with monoamine oxidase inhibitors and combined antidepressant therapy may have been responsible for the relatively low mean dosage of monoamine oxidase inhibitors taken in the study. There is now good evidence that clinical response to phenelzine is greater in a daily dose of 60-90 mg,¹⁻³ whereas the average dosage in Dr Young's study was 44-45 mg daily.

Although combined antidepressant therapy should not be used for mild and moderate depression as first-choice treatment this does not mean that it has no place in other depressive and associated disorders.

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² Quitkin F, Rifkin A, Klein DF. *Arch Gen Psychiatry* 1979;36:749-60.

³ Tyrer P, et al. *Br J Psychiatry* (in press).

Aggressive patients—what is the answer?

SIR,—I read Dr A Bailey's comments (8 December, p 1509) on Dr K Raghus's letter (3 November, p 1147) with respect to aggressive patients with increasing unease and incredulity.

If I have grasped the meaning of Dr Bailey's comments correctly, do I now have to make meaningful inquiries as to the psychological status of the person demanding a visit for another person, accepting that this is an "indication of the intermediary's agitation"—however rude, objectionable, or threatening that "agitated intermediary" may be?

Let me ask this. If you had a burst pipe, your house was rapidly filling with water, and in your "state of agitation" you asked your neighbour to send for the plumber, whereupon he rang the aforesaid person at, perhaps, 2 am with the request "Send the bloody plumber," would you really expect him to come?

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Mebendazole and hydatid disease

SIR,—I have followed with great interest the recent reports of pyrexia following mebendazole therapy for hydatid disease. We have recently excised a 2-litre hydatid cyst of the liver in a young woman. Full-dose mebendazole therapy was started five days preoperatively and samples of venous blood and intact daughter cysts were collected at operation for mebendazole levels, which we have been unable to measure. In the postoperative period a swinging pyrexia developed, which we attributed to infection in the residual cavity. However, fluid from the closed suction drain was sterile on aerobic and anaerobic culture.

The hypothesis of Mr I M Lyon and Mr K W Reynolds (3 November, p 1111) that the pyrexia is due to drug-induced cyst necrosis is difficult to accept. There is no good evidence in man that mebendazole penetrates intact, established cysts in sufficient levels to produce destruction. Heath¹ had great difficulty in destroying experimental cysts in mice and Reisin's experiments² showing that mebendazole entered cysts by simple perfusion were conducted in vitro.

A full course of mebendazole costs the NHS £185. Unless the drug can be shown to penetrate cyst fluid in therapeutic doses sufficient to destroy viable daughter cysts its use in established hydatid disease must be open to question—especially as Benex³ showed that only a small amount of living germinal tissue is required to produce a new *Echinococcus granulosus* cyst.

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² Reisin IL, et al. *Int J Parasitol* 1977;7:189.

³ Benex J, *Ann Parasitol (Paris)* 1968;43:573-82.

Snail-eating mummy?

SIR,—As a parasitologist I was most interested in the review "Buried treasure" by Dr John Cole (1 December, p 1412), which mentioned the finding of a calcified worm of *Dracunculus* (guinea-worm) in the abdominal wall of a mummy. However, without wishing to appear

trivial, I would like to point out that dracunculiasis is not acquired either by ingesting or by contact with snails but by drinking water containing infected *Cyclops* species, which are small (1-2 mm) freshwater crustaceans. In man the mature female worm discharges first-stage larvae (not eggs) at the surface of the skin, usually when the affected part of the body (commonly the foot and lower leg) comes into contact with water. These first-stage larvae are extremely active and can live for four to seven days; their further development, however, depends on their being eaten by a predatory species of *Cyclops*. The larvae penetrate the gut of the *Cyclops* and after two moults in the haemocoel reach the infective third stage.¹ Dracunculiasis today is mainly a disease of rural communities.

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¹ Muller R. *A manual of medical helminthology*. London, Heinemann, 1975.

St Joseph's Hospice

SIR,—Unbalanced though it was, the programme on St Joseph's Hospice did not justify the strictures of your correspondent AS (15 December, p 1579).

Factual errors show that AS is out of touch with the running of St Joseph's and therefore with its singular lack of religiosity. Ministers, parsons, priests, and rabbis are familiar and welcome visitors, but proselytisation is unknown. If conversions occur, they do so in accordance with Johnson's dictum on imminent death. Certainly, there is nothing to suggest that Catholic or Christian patients are favoured more than those of other (or no) religion.

It would be very sad if AS succeeded in dissuading doctors, nurses, or patients from accepting what St Joseph's offers. I know because I have worked there; I am an Anglican with sceptical tendencies.

B SYMONDS

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Resuscitation of the newborn

SIR,—Few articles and even fewer chapters in books of neonatal medicine give any specific details of the techniques that are necessary to resuscitate newborn babies effectively and safely. Dr H B Valman's article (24 November, p 1343) is exceptional in giving an outline of this subject with which all midwives and paediatricians should be familiar. However, there is one aspect of his instructions on endotracheal intubation which I think needs immediate correction.

The epiglottis is a sensitive and fragile tissue and the blade of the laryngoscope should not be placed over it or be used to press it against the tongue. Instead the tip of the laryngoscope should be advanced slowly over the tongue from the right side of the mouth and positioned in the vallecula, anterior to the epiglottis. In this position the epiglottis swings anteriorly when the laryngoscope is gently raised. There can then be no doubt about the site of the entrance to the larynx even when it is obscured by secretions or meconium. Failure to intubate is frequently due to unnecessary urgency, which leads to an incomplete identification of the anatomy of

the upper airway. This is made more difficult when the laryngoscope is advanced too far. Soft-tissue injury of the upper airway, including trauma to the epiglottis, is a common finding after frequent attempts at intubation by inexperienced personnel. Mechanical stimulation of the epiglottis can also cause reflex laryngeal spasm, making intubation even more difficult.¹

Another common mistake which adds to the difficulties encountered during intubation is the idea that the baby should be placed on a sloping surface with the head much lower than the feet. Dr Valman quite rightly states that the infant should be placed supine on a flat (horizontal) surface. In this position the head should be slightly elevated *without extending* the neck.² Unfortunately, the picture in Dr Valman's article shows a baby on a tilted surface on which some babies will slide, making intubation even more difficult.

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² Klaus NH, Fanaroff AA. *Care of the high-risk neonate*. Philadelphia: WB Saunders Company, 1979:31-3.

Breathing difficulties in the newborn

SIR,—In his article in the *BMJ* of 8 December (p 1483), Dr H B Valman states that the cause of the respiratory distress syndrome is a deficiency of pulmonary surfactant.

While I would not dispute that a deficiency of surfactant *correlates* with the respiratory distress syndrome, precipitating *causes* are observable daily during hospital management of preterm infants. Every intervention, such as heel-pricking with needles, weighing on metal scales, insertion of nasogastric tubes, and especially insertion of rectal thermometers, causes an observable disturbance of respiration, and is frequently followed by a sustained increase in respiratory rate.

The distress which these regular and painful procedures cause the infants can only be expressed physically. This distress may be lessened by nurses who are aware of the babies' feelings, who talk and handle them sympathetically, and who stay a while with the babies when the procedures are over, talking and touching in a non-painful way and giving the babies something to suck—that is, holding the whole baby in mind.

If one's only contacts with other humans are painful, one's hope and one's will to survive are bound to diminish. No one who takes time and trouble to observe these babies will fail to see what I have described above.

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Multiple sclerosis: what can and cannot be done

SIR,—The contribution of Professor E J Field and Greta Joyce (15 December, p 1571) is rightly published under the heading "For debate" and we hope that a lively discussion

will follow. Otherwise readers could get the wrong impression that multiple sclerosis has become a disease easy to diagnose and to prevent. Except for point (1)—that there is no cure for clinically established MS—all statements made by the authors are unproved or misleading.

Work with coded material has shown that the erythrocyte unsaturated fatty acid (E-UFA) test is non-specific¹ and by no means better than other laboratory parameters (immunoglobulin ratio, oligoclonal bands of the cerebrospinal fluid, etc). The results of other authors who could not confirm the test^{2,3} cannot be ignored. Seaman *et al*⁴—given as reference for the test's validity—could reproduce the results for clinically confirmed cases, but not for patients "with minimal neurological findings." That means that even if the test turns out to be reproducible in other laboratories we are still left with the difficulty of early diagnosis and often have to wait for dissemination in space and (indispensable not "wasted") time.

From several well-controlled studies we know that high doses of polyunsaturated fatty acids may mitigate or shorten a bout,^{5,6} but do not influence the overall prognosis.^{7,8} To speak of an "inborn anomaly in the handling of unsaturated fatty acids" as if this were a proved fact rather than a speculative hypothesis is not only a doubtful scientific manoeuvre: it creates a lot of anxiety in patients and their relatives. The pattern of familial multiple sclerosis is hardly compatible with Professor Field's assumption, apart from the fact that the process of myelin building is very little influenced by nutritional factors.

The E-UFA test is under investigation world wide because of its interesting pathogenetic mechanisms, which are far from being understood. The practical implications it might have are still doubtful.

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² Forrester JA, Smith WJ. *Lancet* 1977;ii:453-4.

³ Hawkins SA, Millar JHD. *Lancet* 1979;ii:165-6.

⁴ Seaman GVF, Swank RL, Tambllyn CH, Zukoski CF, IV. *Lancet* 1979;ii:1138-9.

⁵ Millar JHD, Langman MJS, Payling Wright H, *et al*. *Br Med J* 1973;ii:765-8.

⁶ Bates D, Fawcett PRW, Shaw DA, Weightman D. *Br Med J* 1978;ii:1390-1.

⁷ Bates D, Fawcett PRW, Shaw DA, Weightman D. *Br Med J* 1977;ii:932-3.

⁸ Paty DW, Cousin HK, Read S, Adlaka K. *Acta Neurol Scand* 1978;58:53-8.

SIR,—Professor E J Field and Greta Joyce (15 December, p 1571) quoted Seaman and his colleagues,¹ who, using a slightly modified Field's E-UFA (erythrocyte unsaturated fatty acid) test, were correct in all 65 patients with established multiple sclerosis. However, Seaman *et al* found the test negative in all four early cases, which would not support Field's claim of being able to identify latent MS and children at risk. A positive test would seem to be due to a secondary phenomenon rather than an inborn anomaly in the handling of unsaturated fatty acids.

Two trials^{2,3} have shown a marginal benefit for MS patients taking at least 17 g of linoleic acid daily. Eight capsules of Naudicelle daily were of no benefit whatever, possibly owing to the low dose of linoleic acid (3 g daily) and γ -linolenic acid (340 mg daily).³ The theoretical claim that γ -linolenic acid would be more