

established. A computerised system would seem to be an ideal tool to detect this occasional but regularly occurring pattern and could prompt both an early appointment and the appropriate discussion with clinic staff.

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### Diabetic autonomic neuropathy and iritis: an association suggesting an immunological cause

SIR,—I agree with the suggestion of Dr R J C Guy and his colleagues that diabetic autonomic neuropathy is related to antibodies to nerve growth factor and insulin (11 August, p 343). This was suggested by certain similarities in the amino acid sequence<sup>1</sup> and structure of insulin and nerve growth factor,<sup>2,3</sup> as described in the paper. These assumptions would seem to suggest that insulin and nerve growth factor are being produced normally and are then destroyed by an immune process.

It has been suggested that an ancestral proinsulin gene underwent duplication and the two copies subsequently evolved divergently to yield insulin as one product and nerve growth factor as the other.<sup>4</sup> If this hypothesis were true then it would be possible to postulate that the antibodies are directed towards receptor sites with similar structures rather than against polypeptides themselves. The receptors could either be present in the cell membrane activating secondary messengers or on the nuclear membrane, possibly on the DNA itself responding to the secondary messengers. If the antibodies bound irreversibly at a "binding" site of the receptor causing steric hindrance thus inhibiting "activator" site binding this could explain the lack of endogenous insulin and nerve growth factor.<sup>5</sup>

This is one alternative explanation for an immunological cause of diabetes mellitus. Whichever mechanism is correct I would suggest that this paper also adds support to the theory that diabetes mellitus is in fact a group of disorders all presenting with polyuria and polydipsia. This is supported by the fact that not all diabetics develop an autonomic neuropathy and one may suggest that it is only individuals with a specific immunological disorder who do so.

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### Snoring and its treatment

SIR,—The leading article by Dr Sheila Jennett on snoring and its treatment (11 August, p 355) is interesting. She describes nasal

airway pressure, tracheostomy, and posture as possible treatments but I do not agree that they offer an adequate management of this condition.

Snoring is three times more common in the obese population.<sup>1</sup> This is consistent with the finding that obstructive sleep apnoea syndrome is more common in this group. "Heavy snoring" subjects are more likely to hypoventilate during sleep and develop various cardiac dysrhythmias.<sup>2,3</sup> Obese people with obstructive sleep apnoea are found to have nasopharyngeal airway resistance three to four times greater than that of healthy controls. This increased airways resistance is greater in a supine position, and it could well be a critical factor in promoting airway collapse in sleeping obese patients with apnoea.<sup>4</sup> None the less, there is a strong association between obstructive sleep apnoea and obesity<sup>5</sup> and improvement in some cases following weight loss,<sup>2,3,6</sup> suggesting that adipose deposition in the pharynx may be crucial in the development of obstructive sleep apnoea.

Various treatments of sleep apnoea have been tried. In massive obese people I consider that weight reduction is the treatment of choice, although records are not always encouraging.<sup>7</sup> Our patients with sleep apnoea syndrome did respond to weight reduction and the clinical improvement was similar to that achieved by tracheostomy.<sup>2,3</sup> Tracheostomy may be complicated by bleeding, subcutaneous emphysema, low grade infection, and postoperative depression.<sup>8</sup> Mouth breathers may find nasal continuous positive airway pressure uncomfortable.<sup>9</sup> If the problem is explained and the patient is reassured weight reduction may be successful in treating this difficult problem.

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### Urinary tract infection in children

SIR,—Dr W McKerrow and others (4 August, p 299) conclude that the results of their study support the view that all children who have a proved urinary tract infection should undergo radiological investigation. This view originates from the conclusions drawn by Smellie *et al*, who carried out a similar hospital based study of childhood urinary tract infection.<sup>1</sup> I have recently questioned the validity of her conclusions as she had extrapolated from a

highly selected population into the general population.<sup>2</sup> For the same reason I do not feel that Dr McKerrow and others have sufficient evidence to support a policy of radiological and cystoscopic investigation of all children found by their general practitioners to have a urinary tract infection.

Despite the authors' belief that their survey was reasonably unselected, the children in this study were a highly selected group. They were all referred by their general practitioners to a paediatric surgical outpatient department. Unfortunately, the authors give very little clinical data of the children at presentation, so it is impossible to compare adequately this group with groups of children presenting with urinary tract infection in general practice. Even so, 408 children (71%) had had recurrent infection at the time of presentation—a much higher rate of recurrence than that found in general practice studies.<sup>3,4</sup>

The authors further state that investigation after a first infection is advantageous, as of their 164 patients infected only once, 40% had an abnormal intravenous urogram, 54% had an abnormal micturating cystogram, and 35% required surgery. They include among this group, however, 67 children (41%) who had a positive culture of a midstream urine specimen at first consultation: this surely must mean persistent or recurrent infection, assuming the initial urinary tract infection had been treated?

A shift in paediatric teaching to emphasise early referral has been followed by general practitioners being criticised for not referring children with a urinary tract infection.<sup>5,6</sup> It is hardly scientific, however, to extrapolate the results of hospital studies based on selected high risk children to the general population. Surely the management of childhood urinary tract infection in general practice should be determined by studies of the condition in general practice? A large prospective study of childhood urinary tract infection in general practice is clearly needed or many children are going to continue to be exposed perhaps unnecessarily to the uncomfortable and (perhaps) hazardous process of full urological examination.

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### Graves' disease and atrial fibrillation

SIR,—I was pleased to see the paper by Dr G R Scott and others (18 August, p 399) recommending a high dose of iodine-131 in the treatment of Graves' disease complicated by atrial fibrillation. The same treatment is also the most suitable treatment for toxic nodular goitres complicated by atrial fibrillation. There is no mention of the role of antithyroid drugs in these patients and yet

unfortunately many physicians use them definitively when atrial fibrillation is present. Many patients respond satisfactorily and sinus rhythm is often restored. This is a most unwise definitive form of treatment, however, for atrial fibrillation nearly always recurs if there is a relapse, which is unfortunately so common. Occasionally, complications of atrial fibrillation may then occur, and these may often herald a relapse, as we have clearly shown.<sup>1,2</sup> I feel strongly, therefore, that all patients with thyrotoxicosis and atrial fibrillation should have definitive treatment with iodine-131 and I agree fully with the dosage recommended by the authors.

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### Use of biothesiometer to measure vibration thresholds

SIR,—Bloom *et al* state in their report on the use of biothesiometry: "No other study of biothesiometry has investigated how vibration threshold varies with age" (16 June, p 1793). I would like to draw the authors' attention to the papers listed below.<sup>1-6</sup> The papers deal with the variations in vibratory threshold with age, and the results obtained agree well with those reported by Bloom *et al*.

Studying the centile charts in the paper by Bloom *et al*, I get the impression that subjects with threshold levels above the meter range—namely, >50 volts—have been included in calculating the regression lines. In doing so the results are bound to be erroneous, since the calculations require that the two parameters (or a transformation of these) can vary linearly within the studied range. Even though centile charts cannot be constructed for subjects aged over 60 biothesiometry can still be of use in these older age groups, since a finding of a threshold level within the meter range would exclude the presence of peripheral neuropathy.

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### Doctors and the Third World

SIR,—Recent correspondence in the *Lancet* parallels that of your own on surgical services for developing countries.<sup>1</sup> Common themes are the paucity of adequately trained doctors in the Third World; overproduction and unemployment of doctors in the West; and the difficulty that junior doctors face on their return from

assignments abroad, both in obtaining recognition of their overseas experience for the higher professional training requirements and in finding subsequent employment in the National Health Service.

I have suggested a possible solution to these problems.<sup>2,3</sup> It entails the establishment of links between the postgraduate tutors in the various specialties, the Overseas Development Administration, and the voluntary agencies. The tutors would be responsible for advising junior staff before their departure, maintaining contacts during the period abroad (which could include research projects if required), and helping with re-employment and accreditation on return. The Overseas Development Administration should extend its role to cover superannuation and pension rights, and should maintain the salary offered by the voluntary or governmental agencies at NHS levels. The voluntary agencies could cooperate to produce detailed descriptions of jobs available (the Bureau for Overseas Medical Service could help with this—Africa Centre, 38 King Street, London WC2), with a basic training programme before departure to cover aspects of preventive medicine and teaching.

It is time that the royal colleges concerned themselves more seriously with the concept of Third World experience; it should be seen as an acceptable, even desirable, part of the higher professional training period in a junior doctor's education. In anaesthesia, for example, many seniors take an active interest in experience derived from developing countries, and such experience is often accepted for accreditation by the faculty's joint committee on higher training. Indeed, an association of anaesthetists with a specific interest in the Third World is now being formed.<sup>4</sup> I hope that the other specialties will follow suit so that more formal links may eventually be established with developing countries, to our mutual benefit.

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### Risks of intrauterine contraceptive devices

SIR,—Dr Pat Thompson doubts our findings of decreased prevalence of actinomycetes in cervical smears of patients using copper bearing rather than inert plastic intrauterine contraceptive devices (4 August, p 323).

There is no evidence that *Actinomyces israelii* can be cultured or identified by immunofluorescence techniques in cervical and vaginal samples taken from women who are not using an intrauterine contraceptive device.<sup>1,2</sup> The most probable source in these cases is the gut via the perineum. These organisms, however, do not proliferate to form recognisable colonies in the absence of a foreign body (most commonly an intrauterine contraceptive device) and in our experience the identification of actinomycetes is a "marker" of a polymicrobial anaerobic infestation of the cervix. *Actinomyces israelii* only rarely spread to include the upper genital tract, but when

this does occur the resulting indolent infection may cause serious morbidity.

During the period of our study (1978-81) inert intrauterine contraceptive devices were still being used in the Dundee area and we found that the presence of actinomycetes was almost invariably associated with long term (more than two years) use of these devices. Conversely, patients with copper containing devices had a low prevalence of actinomycetes and we considered that this was related either to the known bacteriostatic action of copper<sup>3</sup> or to the practice of replacing devices containing copper at regular (usually two yearly) intervals.<sup>4</sup>

In this area devices containing copper are now used almost exclusively and we rarely identify actinomycetes. It would be interesting to know the exact proportions of inert and copper containing intrauterine devices used in the North Kensington family planning clinic during 1981 for this, in our view, has a major effect on any study of actinomycetes infestation.

With reference to our paper the percentage of copper and plastic device users harbouring actinomycetes is wrongly quoted by Dr Thompson.<sup>5</sup> Of 45 cases where *Actinomyces israelii* was cultured only two were copper device users.

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### Doses of aminophylline given intravenously in casualty

SIR,—We have carried out a similar study to that of Drs C S Munro and K Prowse (11 August, p 354) and endorse their findings.<sup>1</sup> With a loading dose of 5 mg/kg body weight in acutely ill asthmatics, some of whom were already on oral maintenance theophylline, none of the patients developed toxic levels. We also found that an infusion of 0.7 mg/kg body weight after the above loading dose produced therapeutic levels in 14 out of 20 patients. Eight hours after the infusion was discontinued, however, only seven patients had adequate theophylline levels, a finding which suggests that if theophylline is to be used in the convalescent period it should be started orally immediately after the infusion is discontinued.

We believe that our study and that of Munro and Prowse show another important feature—the difficulty of using oral theophyllines. The pharmacokinetic characteristics of the drugs are difficult enough, but it is then essential