crisis after eating green bananas stewed with the skins on.

Vague admonitions to avoid certain foods or medicines are valueless. The worst the patient expects is indigestion; this is more so since many have already eaten the substances with impunity and so imagine they are immune. We have previously listed the many variables which interact to determine whether or not foodstuffs ingested on a particular occasion cause hypertension.⁶ These include the type, duration, and dosage of inhibitor. In our experiments with the yeast extract Marmite we showed how the same amount of this substance could be harmless on one occasion but produced dangerous hypertension on another, depending on treatment variables.

Patients prescribed monoamine-oxidase inhibitor drugs should be categorically warned that consumption of the known foodstuffs and medicines could possibly induce a fatal episode of high blood pressure, and that the uneventful consumption of these items on one occasion is not a licence for their safety at all times. If the patient is not ill enough to justify such a drastic admonition, another drug should be prescribed.—We are, etc.,

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Disodium Cromoglycate and Induced Asthma

SIR,—The comparison by Dr. H. Herxheimer and Miss Helga Bewersdorff (26 April, p. 220) of their studies with our own¹ on the effects of disodium cromoglycate in induced asthma requires detailed critical examination.

They quote the summary of our report as claiming that "all were fully protected by the inhalation of 20 mg. of cromoglycate powder," and question this by quoting our text as saying that "one of the mite asthma patients was only partially protected." Our summary in fact reads: "inhalation of disodium cromoglycate inhibited all the allergic reactions produced by inhalation of the appropriate allergens." If the inhibition had been complete we would have said so. Indeed, in the one exception this was virtually the case, since the disodium cromoglycate reduced the fall in F.E.V.₁ produced by the allergen challenge to 8%, in comparison with falls in the initial test of 29% and in the final placebo plus allergen test of 24%. A fall of 8% in the F.E.V.₁ is within the limits of variation of the method. It is to be expected, however, that disodium cromoglycate would be less effective in inhibiting more vigorous reactions to challenge than the already appreciable ones on which our study was based;

reactions which were of a comparable order to those listed by Dr. Herxheimer and Miss Bewersdorff.

In their experiments with the aerosol of disodium cromoglycate they report on full protection in two subjects, partial in another, and possible, but not definite, protection in two others. Examination of their figures shows that two of the 13 patients, F and G, should not have been included in their study because their falls in vital capacity and peak flow after challenge were 5% and 20% in one patient and 8% and 10% in the other, and were too small for satisfactory study. These values were, in any case, far lower than the reactions they claim to have established in their patients beforehand—namely a fall in vital capacity of 10% to 20% and in peak flow of 30% to 40%. In the remaining 11 patients there was protection by the disodium cromoglycate in A, B, C, D, and E, and evidence of protection in I and L judged only by the vital capacity, and in J judged only by the peak flow, thus bringing their numbers with evidence of protection up to 8 out of the 11.

They also cast doubts upon our methods, stating that the F.E.V.₁ measurement is not suitable for this purpose because "it impairs lung function," presumably ventilatory function. There must be few reputable pulmonary physiologists who would not accept the F.E.V.₁ measurement as one of the better methods of ventilatory measurement in the present context. The vital capacity measurement used by Dr. Herxheimer and Miss Bewersdorff may be little altered in patients with marked decrease in the F.E.V.₁ and with obvious airways obstruction. The peak flow rate, which they also used, useful though it is, is subject to greater variability than other single breath tests. But overriding all these considerations is the fact that in the patients we studied the F.E.V.₁ gave consistent and reproducible readings in repeated measurements. In the control inhalation tests with carbol-saline, performed before the allergen challenge in order to exclude patients with excessive variability, the F.E.V.₁ and F.V.C. showed little or no change in any of the patients at repeated tests and at any time during the day. Further factual evidence of the overall consistency of the F.E.V.₁ test in our studies is shown by the reproducible reactions produced by the initial allergen test and the final placebo plus allergen test. The falls in F.E.V.₁ measurements in pairs for these two tests were as follows in each patient: -33% and -30%; -27% and -23%; -29% and -24%; -30% and -18%; -45% and -41%; -18% and -13%; -54% and -43%; -23% and -39%; -33% and -24%; and -18% and 0%.

The mode of administration of allergen is also questioned by Dr. Herxheimer and Miss Bewersdorff, who maintain that their closed-circuit method has the advantage of making it possible to estimate the amount of allergen retained. Once again the reproducibility of the reactions we produced shows that our method was eminently satisfactory for the purpose. Indeed, our results were more consistent in all respects than those of Dr. Herxheimer and Miss Bewersdorff. Their use of the closed-circuit method and the termination of reactions by isoprenaline inhalation, usually within 10 minutes, may be part of the explanation for their failure to reproduce our results. It is quite possible that this early termination of the reactions may have masked their true severity, against which the disodium cromoglycate may have been less effective.

An important part of the argument of these workers arises from the assumption that, because of the size of the coarse particles in the powder, it "could therefore not be expected to reach the finer bronchioli" and would therefore be ineffective in this form. They conclude that "we do not believe that the coarse particles can have more than a fraction of the effect of the fine aerosol, such as we used," and that the results obtained by us with the powdered preparation

"must be seriously questioned." We ourselves have no particular brief for the disodium cromoglycate as a powder, but the facts speak for themselves. Used as a powder in the tests as performed, we showed unequivocally that it had a potent inhibitory effect.

Dr. Herxheimer and Miss Bewersdorff have the temerity to comment on the clinical efficacy of disodium cromoglycate on the basis of single inhalations of the aerosol in five asthmatic patients. This part of their work is quite inadequately documented, and it is not clear when it was they made their measurements showing only trivial changes in lung function. Presumably, by analogy, if a single even large dose of corticosteroid did not improve ventilatory function, these workers would accept this as evidence that corticosteroids would not work in asthma.

They suggest that the patients on disodium cromoglycate reported by other workers in whom corticosteroid drugs were stopped or reduced had only very slight mild asthma. In our letter (15 February, p. 443) on steroid-pseudorheumatism in patients under treatment with disodium cromoglycate and in whom the corticosteroids were reduced or stopped, it can be seen that there were three patients on 10 mg. prednisone, one on 7.5 and one on 2.5 mg./day in whom corticosteroids were stopped; one on 12.5 and another on 7.5 mg./day in whom it was reduced to 2.5 mg./day; and two on 10 mg./day in whom it was reduced to 1 mg./day. These patients had been on corticosteroid treatment for at least several years in most cases, and repeated attempts had been made to stop it. They could not be regarded as patients with very mild asthma. We do not know how long such impressive progress will last, but these findings are like those of some of the other workers. The major problem at present is to see why there are wide divergences in opinion about disodium cromoglycate. Close comparison of clinical criteria and methods of assessment may help to explain the differences and to show which patients are most likely to benefit.

The grudging concession by Dr. Herxheimer and Miss Bewersdorff that disodium cromoglycate may protect from induced asthma, and that it seems to help a small minority of patients, would have met more respectful consideration if they had put their "beliefs" to the test and followed the same procedures as we, and others, have employed.—We are, etc.,

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REFERENCE

- Pepys, J., Chan, Moira, Hargreave, F. E., and McCarthy, D. S., *Lancet*, 1968, 2, 134.

Torsion of Testis

SIR,—I was interested to read Mr. K. Mohay-ud-Din's letter (15 February, p. 445) and the subsequent correspondence (15 March, p. 715) on this subject in your columns. I am surprised that no one has mentioned manipulation of the torsed testis and the instant relief of pain that this brings. Since qualifying two years ago I have seen eleven cases of torsion of the testis and in every case have been able to untwist the torsion by rotation of the testis away from the

midline. This included one case seen two hours after attendance at hospital where the patient was given ampicillin and paracetamol.

Detorsion relieves the pain immediately and dramatically and the testis descends in the scrotum with immediate relief to the patient. Analgesics may not be needed to do this.

This does not of course reduce the need for operation on both sides, since recurrence is likely, but it does make operation less urgent, and may be a useful practical procedure while awaiting anaesthesia or make the operation an elective one.—I am, etc.,

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Herpes Hepatitis

SIR,—In the case of herpetic hepatitis in a pregnant woman (26 April, p. 204) products of conception were examined. Unfortunately by the time they were available maceration was so far advanced that no evidence of infection could be established histologically. Attempts to isolate virus from the foetus were unsuccessful.

This was not surprising; the half-life of herpes simplex virus at 37° C. is only three hours.¹—I am, etc.,

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REFERENCE

- 1 Farnham, A. E., and Newton, A. A., *Virology*, 1959, 7, 449.

Tracheostomy

SIR,—The two letters on the subject of tracheostomy (19 April, p. 185) refer to the possible role of cuff pressure in producing tracheal damage. There is an often unrecognized danger in the current method of reinflating tracheostomy tube cuffs with a predetermined volume of air. When a syringe is used for inflating the cuff much of the travel of the plunger is taken up in compression of the contained air. If a Portex polyvinyl tube cuff is inflated with 5 ml. of air from a syringe which contained 20 ml. initially, the pressure in the cuff will be about 100 mm. Hg; if 5 ml. is delivered from a syringe which contained only 5 ml. initially, the pressure is likely to be about 250–300 mm. Hg. If the initial volume in the syringe is not described it is possible for gross over-inflation of the cuff to occur if a smaller initial volume is used during reinflation of the cuff. This is dangerous, as it can be shown that the pressure applied to the tracheal wall increases rapidly as the cuff is inflated beyond the point at which the trachea is first completely sealed.

There is a strong case for inflating cuffs to a predetermined pressure, particularly where deflation and reinflation is part of the routine management of tracheostomies. This can be done simply and cheaply using any aneroid pressure gauge, such as an aneroid sphygmomanometer, connected to the inflating syringe through a Y-piece. The point at which a satisfactory seal occurs can be found by auscultating over the larynx while inflating the lungs.

I would agree that tracheal stenosis following the use of cuffed tracheostomy tubes has become more common. Analysis of the complications occurring in a group of 320 consecutive tracheostomies done during the last five years showed an incidence of tracheal stenosis of 4.3% among the survivors. However cuff pressure is not the only factor, and tissue reaction to the material used must be remembered when looking for alternatives.—I am, etc.,

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SIR,—I was rather surprised to read the letter by Drs. R. G. Bannister and R. A. Beaver (19 April, p. 185) on the use of plastic cuffed tracheostomy tubes.

I wish to clarify that the Portex endotracheal and tracheostomy tubes conform to U.S.P. XVIII page 905 (modified) implantation test. The Portex plastic tracheostomy tube has been available throughout the world for over five years, during which time a considerable amount has been written on the advantages of polyvinyl chloride versus rubber. The incidents of tracheostomy complications at the National Hospital are, of course, disturbing, but many will question the statement that the Portex tube is a hard material. It is important that the polyvinyl chloride cuffs should not be over-inflated, since this will indeed bring about a hardening situation and defeats the object of using a pliable material which will soften at body temperature and mould to the mucosal surfaces.—I am, etc.,

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Portex, Ltd.

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SIR,—The letter from Drs. R. G. Bannister and R. A. Beaver (19 April, p. 185), suggesting that the use of tracheostomy tubes made of rubber may be followed by few of the severe late complications of tracheostomy, agrees with an impression that was gained during a recent survey of more than one thousand tracheostomies in patients admitted to the United Oxford Hospitals.¹ When tracheostomy was done in any unit but one in Oxford, the patients who required a cuffed tube were normally cannulated with modern red rubber tubes of the Radcliffe pattern² (which incidentally are readily available), and so far there have been no tracheal stenoses at the level of the cuff or the tip of the tube in this group. Polyvinyl chloride tracheostomy tubes have been employed routinely for about six years in one unit where the operation is carried out about ten times each year. Among these patients there have been two tracheal stenoses, both involving the stoma and the trachea between the stoma and the carina.

The comparative scarcity of late structural complications which was observed in this series after sometimes very prolonged (several years) intubation with Radcliffe red rubber tubes can perhaps be attributed partly to the inertness of the constituent material, partly to the shape of the tube, which allows it to sit securely in the trachea and stoma, and partly to the long and compliant cuff. This last feature, and the practice of employing a tube

that fits snugly into the trachea, allows the use of minimal cuff inflation pressure to obtain an airtight seal in the trachea.—We are, etc.,

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- 1 Kerr, J. H., Spalding, J. M. K., and Crampton Smith, A., *International Anaesthesiology Clinics*, 1969, in press.
- 2 Salt, R. H., Parkhouse, J., and Simpson, B. R., *Lancet*, 1960, 2, 407.

Anaesthesia for Dental Patients

SIR,—I would like to draw attention to what appears to be an anomaly in the terms of service of the consultant anaesthetist.

A National Health Service patient receiving National Health Service dental treatment from a National Health Service dentist (in his surgery) can receive anaesthesia only from a doctor who is acting in a private capacity, but who is paid a fee indirectly supplied by the National Health Service.

Having regard to the wide range of services available on the Category II list¹ it seems reasonable to suggest that dental anaesthesia of this sort should also be included in Category II. It certainly appears anomalous that the services of a private practitioner must be enlisted for this single small feature of what is otherwise a completely National Health Service provision. Anaesthesia for private dentistry in surgery or nursing-home would, of course, remain the prerogative of the private anaesthetist.—I am, etc.,

Cardiff.

D. WAKELY.

REFERENCE

- 1 *National Health Service. Terms and Conditions of Service of Hospital Medical and Dental Staff (England and Wales) and Headquarters Medical Staff of Regional Hospital Boards (England and Wales)*, 1 February 1967.

Allergic Intolerance to Tetracosactrin

SIR,—With the introduction of tetracosactrin (Synacthen) it was expected that the problem of allergic reaction to administered adrenocorticotrophic hormone was finally overcome.^{1,2} This was anticipated, as it was a synthetic, and therefore pure, preparation, and also through the omission from the polypeptide chain of the terminal 15 amino acids responsible for the immunological and antigenic properties of naturally occurring A.C.T.H. The remaining chain of 24 amino acids was shown to have unimpaired adrenocortical stimulating activity,³ while patients hypersensitive to natural A.C.T.H. were shown to tolerate tetracosactrin in all publications to date.²

We report here a case of allergic intolerance to tetracosactrin believed to be the first encountered.

The patient, a female aged 51, had been attending the department of dermatology for three years for the treatment of a persistent eczema. She had for a period required oral corticosteroids, but these were stopped in September 1968. Her skin deteriorated steadily thereafter and treatment with weekly injections of Synacthen Depot, 1 ml. containing 1 mg. tetracosactrin, was commenced in January 1969.