warning: dispose of her with a dab of benzyl benzoate—in the excitement of her translation she may have laid an egg.—I am, etc.,

Winchester.

LIONEL BACON.

Accident Services

SIR,—It is heartening to read the view of an orthopaedic surgeon of Mr. Noel Smith's distinction that accident incidence has now reached such vast proportions that the time has come for resuscitation to be made a speciality in itself (15 April, p. 176). Hitherto the mass of his orthopaedic colleagues have seemed to be averse to this. Few of us who are engaged in accident surgery would disagree that casualty departments are bedevilled by the demands for minor surgical affairs, more appropriately dealt with in surgical outpatient departments; this role is particularly embarrassing when an accident unit is constantly being called upon to reanimate gravely injured persons. It is true, also, that any major accident unit is incomplete without a comprehensively equipped resuscitation room, as has been declared by the Ministry itself. Further, it should be there in addition to any contiguous intensive therapy department.

The smaller hospitals not classified as accident centres could clearly be brought into an imaginative and larger accident service than now exists. Mr. Smith advocates the employment of a "flying squad," to be expeditiously dispatched to the peripheral hospital from the major one. Up to a point I agree, but I see a more effectual measure in another direction, which could combine with or do without the lesser hospital in varying circumstances. In company with others, here and abroad, whose professional lives have been concerned with accident service I have always deplored the commonly accepted tenet, "rush the patient to hospital," in the presence of near-irreversible shock. "The mortality of the ambulance ride," in the experience of all of us, is amply confirmed by the frequency of the frustrating "brought in dead." I disagree with the negative injunction, contained in the Ministry's "Report by the Working Party on Ambulance Training and Equipment,"¹ to the effect that the prime task of the ambulance staff is "to transport patients to treatment centres without avoidable deterioration or unnecessary delay." But early on I learned the lesson: "Let not our haste lead us into harmful hurry." I contend that the duty is to transport the patient in an improved condition; too often "unnecessary delay" connotes undue rush, with flashing lights and clanging bells—all factors contributing to the unhappy termination, brought in dead.

The action should be in reverse: rush the hospital to the patient; resuscitative measures of whatever extent can then be effected at the locus of the accident. A rejuvenated injured one can thereafter be quietly and smoothly conveyed to his destination, be it the intermediate peripheral hospital or, more generally, the major accident centre—in either case, in safety. Definitive surgery can then follow with minimum delay.

The foresight shown by the International Grand Prix Motoring Organization in establishing their mobile hospital—a pantechicon van-like vehicle housing x-ray unit, drug refrigerator, blood bank, and other equip-

ment—for attendance at racing circuits provides the key. I know that less ambitious arrangements have been achieved along these lines in other quarters, but I feel that this latest racing service ought to inspire the Ministry to adopt just such a one as their own. The mobile hospital idea could well be, as it were, a pseudopodial activity of existing intensive therapy units.—I am, etc.,

DANIEL LAMONT.

Grimsby General Hospital,
Lincs.

REFERENCE

- ¹ Report by the Working Party on Ambulance Training and Equipment, Part I—Training, 1966. London.

Crush Injuries of the Chest

SIR,—I read with interest the paper "Crush Injuries of the Chest" by Dr. W. Bargh and others (15 April, p. 131). The authors comment that they institute intermittent positive-pressure respiration (I.P.P.V.) on clinical grounds even if blood-gas analysis shows a low PaCO_2 . They do not say whether their blood-gas analyses included the measurement of PaO_2 .

It is almost certain that when the PaCO_2 was lowered in the absence of a metabolic acidosis the hyperventilation thus indicated was due to hypoxaemia. The latter condition, in association with chest injuries, where lung contusion and atelectasis can be considerable, may be due to increased shunting of blood in the lungs. Other factors could be anaemia, in which the oxygen-carrying capacity of the blood is reduced, and hypovolaemia or hypotension. The latter cause poor perfusion, resulting in increased oxygen extraction, and thus a lowering of the mixed venous oxygen content (CvO_2). The increased respiratory effort induced under these circumstances by the arterial hypoxaemia raises the patient's oxygen requirement (VO_2). A vicious cycle results, in which the cardinal symptoms may be restlessness and difficulty in achieving control with a ventilator.

Treatment under these circumstances must be aimed at improving oxygenation or, more specifically, raising the CvO_2 . As the authors point out, this may be difficult even when pure oxygen is used, because blood passing through large right to left shunts in the lungs is not available for oxygenation.

Transfusion of blood and restoration of haemoglobin content improves perfusion and oxygen carriage. Any beneficial effects of I.P.P.V. in such a patient are due to a lowering of VO_2 as a result of the abolition of respiratory effort and sedation produced by the drugs given, and also the alleviation, to a certain extent, of atelectasis by the positive pressure. Raising the inspired oxygen in association with the use of a ventilator may be of but little benefit. However, the authors do not comment on their usual practice.

Bearing in mind the complication of intubation and tracheotomy, plus the deleterious effect of I.P.P.V., if injudiciously employed, on both cardiac output and cerebral blood flow, this procedure may still tip the scales in a favourable direction. This use of I.P.P.V. is distinct from the use of the procedure where there is a failure of alveolar ventilation manifested by a raised

PaCO_2 (e.g. in the management of paradox produced by a large labile segment of chest wall), although of course both sets of circumstances may coexist.—I am, etc.,

Department of Clinical Measurement,
Westminster Hospital,
London S.W.1. JULIAN M. LEIGH.

Oral Contraceptives

SIR,—The article by Dr. E. R. Bickerstaff and Dr. J. MacDonald Holmes on cerebral arterial insufficiency and oral contraceptives (25 March, p. 726) is most interesting and valuable in drawing attention to aspects of a problem which is as yet poorly understood. It is certainly of the greatest importance that such observations should be made.

I should like, however, in no spirit of carping criticism, to make a comment about the somewhat unscientific statement that these patients had been taking oral contraceptives. No doubt they had, but if we are ever to be able to disentangle the confused aetiology of thrombosis we must think not of "oral contraceptives" but of synthetic oestrogens and progestational compounds. The authors do state, "There was no predominance of any one variety of oral contraceptive preparation." This, however, hardly goes far enough. It would be interesting to know whether the preparations were sequential or combined (in which case the patients would have received heavy doses of progestins over long periods—which could well be relevant). Furthermore, it would be of interest to know how often 19-nortestosterone derivatives were used as progestins, for these are modified male hormones and may in some way destroy the vascular advantage premenopausal women have over men.

Could I therefore urge that we no longer think of oral contraceptives as if they were a single entity, for if this practice is not changed interesting and potentially valuable observations in the future will be invalidated because of ignorance of just what factors are involved?—I am, etc.,

A. ARNAUD REID.

Seaforth, New South Wales,
Australia.

Treatment of Spasticity

SIR,—Unfortunately your expert contributors in the article Today's Drugs (1 April, p. 36) dealing with the benzodiazepines omitted to mention the value of chlorthalidopoxide in the treatment of spasticity other than that associated with paraplegia or with cerebral palsy in children.

Six years ago Livingston¹ reported excellent results in a 26-year-old man suffering from athetoid paralysis. In the same year Sylvester² reported that in 10 spastic patients improvement in knee and elbow extension was significant. The following year Carter³ reported on 94 spastic patients, 19 being over the age of 21 years. Marked improvement took place in 17 and moderate in 64. Treatment was prolonged in some cases, a 9-year-old girl suffering from cerebral palsy requiring two months in which to learn to walk and a 22-year-old male epileptic patient requiring five months. All of 14 severely mentally subnormal patients up to 60 years of age, nine being over 21 years, showed improvement with treatment. They had been under ob-

servation as inpatients for periods varying from 14 weeks to 38 years (mean 12.6 years) before treatment. One improved in gait and arm after seven days of treatment, but another showed no improvement until after two months, when walking improved, but not until four and a half months later was improvement seen in the spasticity of the arm.⁴

Dosage reported by the authors varied from 2.5 mg. daily to 105 mg. in divided doses. Single massive doses of from 750 mg. to 1.8 g. were given by Livingston¹ and produced recovery. Dosage must be adjusted carefully for each patient, for if too great a dose is given the muscles of a sound leg may lose some of the normal tension and falling may occur.

The degree of spasticity may vary slightly from time to time, showing that a psychological element may be added to the organic basis. This is specially the case in children. Crosland⁵ describes children with arms apparently almost useless and in the usual hemiplegic state, who, when believing they are unobserved, can handle objects in which they are specially interested "with quite reasonable dexterity." Observation before treatment is necessary to determine any emotional overlay.

This letter is written to draw attention to the fact that not only may chlorthalidose improve spasticity at any age but also in muscles other than those involved in paraplegia. Improvement may persist after cessation of treatment, particularly in patients receiving high doses and long treatments.—I am, etc.,

G. R. A. DE M. RUDOLF.
Clevedon, Somerset.

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Diplopia and Oral Anticoagulants

SIR,—The following brief case summary may be of interest in that it illustrates how the use of an oral anticoagulant may produce an unexpected clinical response.

The patient is a male now aged 48 years. In 1959 he complained of attacks of diplopia occurring off and on for the preceding nine months, and lasting approximately five minutes each. He also had two attacks of blindness affecting the right eye for a few minutes, with full recovery subsequently. Both these symptoms occurred spontaneously with no precipitating causes. Full investigations, including clinical examination, electrocardiogram, cerebrospinal-fluid examination, bilateral carotid angiograms, and air encephalograms, were all normal. It was therefore decided to keep him under observation without any definite diagnosis being made.

In 1964 he suffered an anterior myocardial infarction. Diplopia, which had continued to occur off and on, now disappeared as soon as therapy with anticoagulants in the form of phenindione was instituted for the infarction. Since then diplopia has recurred on several occasions, on either reducing the dose of phenindione or stopping it, indicating a definite correlation. There has been no ocular weakness, and tests for myasthenia gravis are negative.

It would seem in retrospect that this patient is suffering from vertebrobasilar artery insufficiency and has benefited from taking oral anticoagulants in an unexpected way. If indeed this is so, several interesting

questions arise. What is the mode of action of the anticoagulant in this patient? Should he be subjected to another series of investigations? What does the future hold for him?—We are, etc.,

Scunthorpe,
Lincs.

J. P. M. PENNEY.
V. K. JAIN.

Amphetamine Prescribing

SIR,—Dr. F. S. Reed rightly questions (8 April, p. 108) the therapeutic value of the amphetamines in current medical practice. That a variety of firms should make a multiplicity of products, all but minor variations of each other, and all containing a potent habit-forming drug, is astonishing enough, but that these products should presumably still be prescribed at a time when their potential dangers have been amply demonstrated and publicized, and when increasing amphetamine misuse causes public concern, is incredible.

The therapeutic value of any drug must ultimately depend upon its uniqueness (or relative efficacy) and its safety, and, except for those instances quoted by Dr. Reed, the amphetamines, I would suggest, have long been supplanted.—I am, etc.,

B. W. DURRANT.

St. Augustine's Hospital,
Near Canterbury, Kent.

Narcosis for Chronic Tension

SIR,—In a paper published last year, Dr. W. Sargent and others (5 February 1966, p. 322) observed that some chronologically tense patients of good premorbid personality, referred for consideration of a leucotomy operation, made very encouraging improvement when given modified narcosis combined with either electroconvulsive therapy or antidepressants. The authors write that their treatment "... seems to constitute, if confirmed, a real advance in the treatment of a group of often seemingly hopeless illnesses," and later, "It seems certain from the figures that patients having narcosis with E.C.T. and combined antidepressive drugs do much better than those having E.C.T. and drugs alone."

However, examining the Table quoted, the number of patients who received narcosis and who became symptom-free, or had only minimal symptoms (26 out of 36), is not statistically significantly greater than those in the same categories who did not receive narcosis (23 out of 37). It can also be seen that the material is further classified in terms of the length of follow-up. Significantly more of the cases showing a poor response to treatment are in the long follow-up group, and this group is under-represented in those who received the treatment which Dr. Sargent and his colleagues feel is superior. The trend towards a better response to treatment in the narcosis group could therefore be due to the significantly shorter follow-up in these patients. While not wishing to dispute the good results reported by Dr. Sargent and his colleagues with these generally intractable patients, it would be invaluable to know whether the trend they noted in respect of modified narcosis has been confirmed by further studies. Since over a year has passed since the paper was published, it may be pos-

sible for the authors to add further cases to the otherwise small number (eight) who received narcosis and who were followed up for more than 18 months.

Sleep may knit up "the ravell'd sleeve of care," but before psychiatrists round the world begin subjecting these chronically sick individuals to the potential hazards of four weeks' obfuscation such additional data would be desirable.—I am, etc.,

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Brisbane, Australia.

Pressurized Aerosols in Asthma

SIR,—I was most interested to read the correspondence in the *British Medical Journal* (4 March, p. 563; 25 March, p. 756) concerning the possible harmful effects of the frequent use of pressurized aerosol sprays in asthma. Some of us in Australia have expressed similar views in letters published in the *Medical Journal of Australia*.¹⁻⁵ In addition, it appears that in some States in this country the death rate from asthma is rather dramatically increasing. Both of these subjects are being investigated and the findings both here and in the United Kingdom should prove most interesting. In the meanwhile I would like to add a further plea for great care in the prescribing of "do-it-yourself" therapy such as aerosol sprays.—I am, etc.,

Adelaide,
South Australia.

R. MUNRO FORD.

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Leucocyte Ascorbic Acid Levels

SIR,—The ascorbic acid studies reported by Dr. J. M. Williamson and his colleagues (1 April, p. 23) are extremely interesting. We have been studying leucocyte ascorbic acid levels with particular reference to alimentary disease. In patients with malabsorption our results have been similar to those reported by the Glasgow group. In the patients who have had gastric surgery our findings are different. We found similar depression of leucocyte ascorbic levels, but when the results were divided into those from a group of symptomless patients and those with post-operative clinical syndromes then a different pattern emerges. Our preliminary results are shown in the accompanying table.

	No. Studied	Leucocyte Ascorbic Acid ($\mu\text{g.}/10^8$ W.B.C.)	
		Mean $\pm$ I.S.D.	Range
Healthy adults	25	28.71 $\pm$ 8.08	18.72-53.53
Duodenal ulcer patients	20	11.31 $\pm$ 6.14	4.47-35.28
Patients operated and symptomless	20	22.89 $\pm$ 3.81	9.33-39.00
Postoperative syndromes	20	13.57 $\pm$ 7.42	3.03-27.86

In the group of twenty patients who were symptom-free and who were studied at a routine attendance at the gastric surgery