"a casualty rate of 1 per 433 rider races does not seem excessive, and does not make it the most dangerous of sports provided all adequate safety precautions have been taken."

It is useful to have this information, but our conclusions from his data are very different. Eleven boys had definite fractures or a haemarthrosis, and three possible fractures, and no less than one in six (four casualties per race) of all riders had some sort of accident. Moreover, a doctor had to be called to see a child on 87 occasions. One important piece of information not given by Dr Place is the number of boys actually taking part: it is likely that one boy will take part in more than one event per season and in more than one race per event. If so, the casualty rate per child will be much higher. A further minor point about the figures is that an average of 25 riders per race, with 24 races per event and 12 events a season, gives a total number of rider-races of 7200 and not 5760 as stated. We have used the figure of 7200 in calculating the number of times a doctor was called to see a child that is quoted above. It should be noted too that these accidents happened only during properly conducted scrambles: we wonder what happens to those children who have accidents during practice when doctors and ambulance-men are not present. We had an 8-year old boy in hospital last year with a fractured maxilla—since this injury is not listed by Dr Place the accident must have occurred during practice.

It is pertinent to point out that it is almost a matter of chance whether an accident results in a graze or a bruise or in something much more serious, and it will obviously not be long before some child is killed if one in six participants has some sort of injury. Dr Place should realise that children of 6 simply do not have the capability of assessing speeds and distances, or the physical co-ordination of both hands and at least one foot necessary to ride a motor-bicycle with reasonable safety. Encouraging children as young as 6 to take part in this sport is wrong and, unfortunately, has the effect of producing a competition to become the parent of the youngest child ever to ride a motorbike. Within the last three months we have had an inquiry from a seat-belt manufacturer for our opinion on the wisdom of his firm providing free, as a sales promotion gimmick, the safety equipment listed by Dr Place for a child who had been bought a 50 cc motorbike by his parents—the child's age being 10 months.

Ability to judge speeds and distances is no less important in scrambling than in ordinary motor traffic, and it is the firm opinion of many Swedish experts that children under the age of 12 are not capable of making the correct judgments that these circumstances necessitate. We agree with this, and feel that Dr Place's encouragement of sport and adventure—which we ourselves advocate—is misplaced in relation to motorcycle scrambling for boys under 13 or 14 years of age.

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Fifty years of penicillin

SIR,—May I add two experiences to the discussion of why Fleming did not do more with penicillin (leading article, 28 April, p 1101)?

I was appointed to the chair of medicine at

St Mary's in the spring of 1939, though I did not begin until 1 October 1939. One of my sponsors was T R Elliott, who greatly admired Wright. He sent me to see Wright in the summer of 1939. When I called on him Wright was in his laboratory discussing a problem with one of his staff, whom we may call H. H had discovered that an agent with which he was working produced a pleural effusion and that the effusion was full of white cells. He wanted to try to analyse the effusion to identify the agent causing the migration of cells. Wright was opposed to this. He told H, "You have done your job in demonstrating the phenomenon using the techniques of which you are the master. Publish it and leave the analysis to others using techniques of which they are the masters."

I was appointed to the Medical Research Council's Committee on Wound Shock when war broke out. Early in 1940 the MRC organised a meeting on war wounds which was held in the lecture room of the London School of Hygiene. Fleming spoke on penicillin. He showed that it prevented the growth of the bacteria that were the main causes of wound infection and that, unlike the antiseptics that had up to then been used to treat wound infection, it did not kill leucocytes. I was most impressed. When I got home I at once wrote to C R Harington, who was the best biochemist I knew, and said to him, "This is obviously the antimicrobial of the future. Why don't you purify it, identify it chemically, and synthesise it, so that you can make even better agents?" He replied to say that Florey had already embarked on this and he was not going to compete.

It was evident to me that Fleming had gained by his own work the essential knowledge outlining the therapeutic possibilities of penicillin. I know, of course, nothing of any discussions between Fleming and Wright. But the experience which I have related would suggest that if Fleming had had ideas of pursuing penicillin further he would not have got encouragement from Wright. And Wright had a formidable mind and was a formidable personality. When I had written an obituary of Thomas Lewis, I gave it to Elliott to read. He said, "I notice you have not called Lewis a genius." "No," I said, "I don't really know what a genius is." "No," said Elliott, "I am not sure I do. The closest approach to one in my experience in medicine was Almroth Wright."

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SIR,—With reference to the correspondence on "Fifty years of penicillin" (26 May, p 1423), the following may help to clear up the misunderstanding of the statement that Almroth Wright had attempted to prevent the suggestion in Fleming's original paper that penicillin might have clinical value.

For almost five years I carried out research work, as a Beit Memorial research fellow, with Fleming on lysozyme in the inoculation department, St Mary's Hospital. Thereafter he was my greatest friend until his death in 1955. During the war I was deputy senior medical officer in the Ministry of Health and bacteriologist in the Emergency Public Health Laboratory Service, stationed in the Institute of Preventive Medicine, Cardiff. As part of my duties I was expected to attend weekly meetings, chaired by Sir Wilson Jameson, CMO, at the Ministry of Health, Whitehall. In 1941,

Fleming's home in Danvers Street, Chelsea, became uninhabitable as a result of a bomb raid. I offered him and his wife the use of our home in Shepherds Hill, Highgate, which he accepted. My home thus became a pied-à-terre during my regular visits to London.

At this time Florey, Chain, and their colleagues were actively developing the purification of penicillin, and on one of my visits to London Fleming was discussing with me the future possibilities of penicillin. It was during this discussion that Fleming referred back to his original paper on penicillin and told me that Almroth Wright ("the Old Man"), a strong believer in the development of the natural defences of the body against infection, had objected to the inclusion in the paper of the possible use of penicillin as "an efficient antiseptic for application to, or injection into, areas infected with penicillin-sensitive microbes." Fleming did not accede and the opinion was duly published.

After Fleming's death in 1955, I had two lengthy interviews with André Maurois in the Royal Garden Hotel, Kensington, when he was preparing his biography of Fleming. During one of these interviews, it was I who told Maurois about Wright's objection to Fleming's forecast of the possible value of penicillin in his original paper. I hope the matter now rests.

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Thalidomide and the "Lancet"

SIR,—In his review (9 June, p 1553) of the *Sunday Times* book, *Suffer the Children: the Story of Thalidomide*, Dr Roe rightly praises the persistence and courage of Mr Harold Evans and his colleagues in pursuing their inquiries into this tragedy. Referring to the findings of Dr W G McBride, of Sydney, Dr Roe says "... it was not until 16 December 1961 that his discovery came to light openly in the form of a letter to the editor of the *BMJ* (a letter previously turned down by the *Lancet*)." Dr McBride's letter appeared in the *Lancet* of that date and not in the *BMJ*. That much is certain. What is still unclear to me is whether or not Dr McBride submitted, earlier in 1961, a paper to the *Lancet* which was refused (as related again on p 91 of the book). Many years after 1961 the *Lancet* learned that Dr McBride was stating that such a paper had been submitted and that he held a *Lancet* letter dated 13 July 1961 giving the journal's decision not to publish it. I have twice asked him and the *Sunday Times* to confirm this account of an article and the timing of it by showing me this letter. They have not done so. Dr Roe's record of what happened in December 1961 is plainly wrong.

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Ocular complications of temporal arteritis

SIR,—The reference to my recommendation for the treatment of blindness in giant cell arteritis (leading article, 2 June, p 1443) fails to convey the sense of urgency which I urge on all general practitioners.

Actually I had written¹²: "The treatment of an elderly patient who suddenly loses the sight

of one or both eyes should start with an immediate intravenous injection of dexamethasone 10 mg and then, as in a cardiac disaster, the patient should be rushed to hospital for admission, if possible to an intensive care ward. The cause of the blindness is likely to be an underlying medical condition and it is best to proceed without delay to treat the patient. Dexamethasone should be continued 4 mg intramuscularly every six hours for three days. Prednisolone 20 mg three times daily should be started on the first day and continued as described in your leading article."

I have not, moreover, recommended temporal artery biopsy in view of the very precarious circulatory balance which may exist in giant cell arteritis between the internal and external carotid circulation. Temporal artery biopsy has disturbed this balance and produced ipsilateral blindness.

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¹² Parsons-Smith, B G, in *Medical Ophthalmology*, ed F Clifford Rose, ch 23. London, Chapman and Hall, 1976.

One-visit endoscopic clinic

SIR,—I was interested to read the paper entitled "Evaluation of one-visit endoscopic clinic for patients with dyspepsia" by Dr A K Beavis and others (26 May, p 1387). This will be a lesson to those who use heavy sedation—up to 50 mg of diazepam for such a tolerable procedure.

We undertake on average 20-25 endoscopies a week and most cases are referred directly to the endoscopy unit as day cases. Ninety-five per cent of the patients have no premedication or sedation during the procedure. Full cooperation of the patients is achieved by explanation of the procedure to them by the referring doctor and the endoscopist. It seems that Dr Beavis and his colleagues have achieved such a high success rate (187 out of 200 patients) by having developed a rapport with the patients at the time of their detailed examination. I would agree with them that men and older patients tolerate the procedure well but I find that women, particularly under the age of 30, are reluctant. It is interesting that 81% of their patients preferred barium meal examination and only 8% preferred endoscopy. Up to two-thirds of our patients prefer endoscopy after realising the value of direct visualisation.

I hope that the barium meal examination in each patient was performed for the purposes of their paper and that it is not a routine practice for each patient to have both endoscopy and a barium meal; otherwise this would invalidate cost-effectiveness and patient convenience.

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Cervical cytology reporting

SIR,—I should like to comment on the correspondence on this subject by Dr R E G Sloan (19 May, p 1356) and Dr M J Sworn (2 June, p 1489). It has been the concern of many pathologists throughout the country to

devise a simple, rapid, and accurate assessment of the cell content of smears for purposes of quality assurance. We in this laboratory have successfully practised such cell scoring for the past 10 years, the screeners entering the relevant data in the research boxes 26-30 at the bottom of the national cytology request/report form (HMR 101/5) while screening the smear or smears.

The average smear under the 22-50 mm coverslip, using a $\times 10$ eyepiece and $\times 10$ objective, is approximately 30 fields horizontally and 10 fields vertically, giving a total of around 300 fields. In box 26 the technician scores the total cell content as C_1 if over 10 000, C_2 if over 20 000, C_3 if over 30 000, and C_4 if over 40 000. In box 27 the number of endocervical cells is similarly scored, together with any metaplastic and transformation zone cells, from E_0 (no cells) to E_4 , when 10 to 20 sheets of cells exist. Boxes 28 and 29 are used to denote blood (B), pus (P), or spermatozoa (S) in a series of degrees according to whether or not their presence obscures a quarter (B_1), half (B_2), three-quarters (B_3), or the whole (B_4) of the epithelial element in the smear. This cell scoring is by no means difficult or time-consuming as the technicians become very adept at assessing the average cell content in spite of the widely differing cell content per field. Moreover, this practice keeps the cytotechnicians alert and interested while screening and provides a greater job satisfaction, especially as they initial the corner of box 22, where a written report may or may not be recorded. By this method we can rapidly check the detection rate of endocervical cells by one cytotechnician against the laboratory norms. A further check is made by a senior or chief technician to note the adequacy of the test and score (when a further initial is placed in the local code box beside box 26), and to assess whether or not the smear needs to be check-screened. Then that person's initial goes beside that of the screener's in box 22. The proportion of double-checked reports with two initials amounts on average to 10-15% of all gynaecological smears. Such cell scoring helps the pathologist when signing out the report to consider the possible need for a repeat, its time interval, and a suggested further investigation.

I would say that Dr Sworn's rough assessment of 10-15% of smears lacking endocervical cells is probably a gross underestimate, as many of the published findings quoted have demonstrated that 50% or more of smears lack these cells—and this is so for those in the child-bearing and sexually active phase of life.^{1 2} It is obvious that we cannot make the presence or absence of endocervical cells a strict measure of the adequacy of a smear; but such a practice of scoring, when understood by the clinicians and general practitioners served by a laboratory, does help to provide a measure of confidence in a particular test.

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¹ Husain, O A N, *et al*, *Journal of Clinical Pathology*, 1974, **27**, 935.

² Husain, O A N, *Tumori*, 1976, **62**, 303.

SIR,—I was the partner referred to by Dr R E G Sloan (19 May, p 1356). As he stated, we collected reports on all smears over a six month period. Although one technician did request a higher number of repeats than the others, this was far from significant and the numbers involved were not adequate for a firm conclusion to be drawn. As Dr M J Sworn commented (2 June, p 1489), perhaps this technician was the most accurate.

Surely the point of the cervical smear is to exclude malignant or premalignant change,

and if it has to be repeated to be certain then this is in the patient's best interest if one accepts the validity of taking the smear in the first instance. Different laboratories obviously have different reporting techniques, and if phrases such as "No malignant cells seen" are used a word with the local pathologist is needed to decide exactly what is meant. The interpretation of technicians and cytologists varies as much as, if not more than, the accuracy of the clinician taking the smear; and it is my impression that the fixing of the smear and the shelf life of the fixative (quoted as indefinite) is more significant than either of the previously mentioned factors.

It seems to be everybody's experience that the smears you are sure are inadequate are reported as normal, and the ones you think are certain to be all right show no endocervical cells.

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Pressure on tracheal mucosa from cuffed tubes

SIR,—The use of a low-pressure high-volume cuff (5 May, p 1173) on endotracheal and tracheostomy tubes would appear to offer several theoretical advantages, particularly in the long term^{1 2}; but I have reviewed the experience of our unit using the traditional Portex low-volume, high-pressure cuff in over 1000 consecutive cases of endotracheal intubation and failed to find any cases of tracheal damage or stenosis. My experience of the newer, "floppy" cuff is that the endotracheal tube is difficult to insert via the nose (our usual method for long-term intubation) without bursting the cuff; it sometimes has been difficult to deflate and remove; and the point at which an airtight seal is achieved is as difficult to determine as with the traditional cuff.

It is probably of equal importance to use the correct size of endotracheal tube so that cuff overinflation does not occur, that careful inflation with the minimum volume of air ensures an airtight seal, and that the cuff then remains continuously inflated until it is changed or removed.

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¹ Cooper, J D, and Grillo, H C, *Annals of Surgery*, 1969, **169**, 334.

² Crawley, B E, and Cross, D E, *Anaesthesia*, 1975, **30**, 4.

Indications for electric convulsion therapy

SIR,—Inability to cry is a recognised feature of so-called endogenous depression. ECT is said to be more effective in endogenous than in reactive or neurotic depression. I was surprised that Drs David Gill and John Lambourn (5 May, p 1169) did not mention inability to cry as a possible indication for ECT, as in a study I carried out at the University of Sheffield this symptom was a better prognostic sign than early waking, loss of weight, or diurnal variation.

A comparison of the therapeutic effects of unilateral, bifrontal, and bitemporal ECT was carried out.¹ Subjects completed the Wakefield depression inventory² before and after their course of treatment. The length of course