CORRESPONDENCE

Multiple-puncture tuberculin testing

A J Johnson, MRCP, and J A Lunn, FFCM;
J T Hart, FRCGP..... 1624

Injuries to boys who scramble

R H Jackson, FRCP, and A W Craft, MD.... 1624

Fifty years of penicillin

Sir George Pickering, FRCP, FRS; V D
Allison, MD..... 1625

Thalidomide and the "Lancet"

I A H Munro, MB..... 1625

Ocular complications of temporal arteritis

B G Parsons-Smith, FRCP..... 1625

One-visit endoscopic clinic

A N Chatterji, MRCP..... 1626

Cervical cytology reporting

O A N Husain, FRCPATH; R R Harrod, MB 1626

Pressure on tracheal mucosa from cuffed tubes

D W Ryan, FFARCS..... 1626

Indications for electric convulsion therapy

Elizabeth A Burroughs, MD..... 1626

What the public wants to know

Rosemary Faunch, MPS..... 1627

"The Dignity of Labour?"

Margot Jefferys, BSC..... 1627

Self-injection of veterinary oil-emulsion vaccines

P B Stones, FRCPATH..... 1627

Marriage matters

S W Waydenfeld, MRCP..... 1627

Obsessionalism and response to lithium

E F Donnelly, PHD, and others..... 1627

Nebulised salbutamol in life-threatening asthma

J S Milledge, FRCP..... 1628

New approach to treatment of recent stroke?

G D Perkin, MRCP..... 1628

Antibiotic-induced interstitial nephritis?

C T Flynn, MRCP..... 1628

Treatment of tinnitus

T K Daneshmend, MB..... 1628

Dialysis and transplantation in young children

D G Oreopoulos, FRCP(C), and others.... 1628

History of an improvement

R S C Howell, FFARCS..... 1629

Doctors and children's teeth

I P D Stocker..... 1629

Cimetidine for hypertrophic prostate?

M Cohen, FRCS..... 1629

The dilated upper urinary tract

P H O'Reilly, FRCS, and others..... 1629

Biochemical testing for acute medical emergencies

C Sanderson, BSC..... 1629

Air embolism and intravenous catheters

E Metcalfe, SRN, and others..... 1630

Captain Cook and scurvy

B Williams, FRCOG..... 1630

Vaccination and pregnancy

Gillian C L du Mont, MRCP, and R C
Beach, MRCP..... 1630

An alternative to colostomy

N R Parrott, MB, and H B Devlin, FRCS.. 1630

Cyclophosphamide in chronic active hepatitis

R Althouse, MD, and others..... 1630

Management and severity of tetanus

J A Rennie, FRCS..... 1631

Medical reports and confidentiality

D L Gullick, MB..... 1631

Isolated communities and their doctors

C G Elliott, BM..... 1631

NHS certificates for hospital patients

R L L Simmons, MRCP..... 1631

Domesticated doctors

Ruth E Ferguson, MB..... 1632

Milage allowance

P R J Vickers, FRCS..... 1632

Review Body report

D J B Ashley, FRCPATH..... 1632

Correction: Community medicine

(Smithells)..... 1632

We may return unduly long letters to the author for shortening so that we can offer readers as wide a selection as possible. We receive so many letters each week that we have to omit some of them. Letters must be signed personally by all their authors. We cannot acknowledge their receipt unless a stamped addressed envelope or an international reply coupon is enclosed.

Multiple-puncture tuberculin testing

SIR,—The tine tuberculin test is attractive because it is safe and easy to use, and non-medical staff should be able to apply it. However, the conflicting reports of the accuracy of the test, as highlighted by your leading article (19 May, p 1300), suggest an inconsistency which is undesirable in a simple epidemiological and clinical test.

Drs D J M Sinclair and R N Johnston in the same issue (p 1325) reported excellent correlation with the Mantoux test, as have others.¹⁻³ To put balance and perspective into the situation, studies with adverse comparisons, in addition to our own, should be mentioned. Manuel and Service⁴ reported to a meeting of the Canadian Thoracic Society a study of 1278 subjects in which 45.2% of those positive to Mantoux testing were negative to the tine test; and Welke, Irsigler, and Kleeberg⁵ found a remarkable 64% false-negative rate. The Food and Drugs Administration⁶ in the USA, in a review of tuberculin testing, found 28.9% false-negative tine results in some areas studied but not all. Browder and Griffon⁷ noted at least 23% of PPD-S-positive subjects had a negative tine test. Two groups of workers have found the tine test to be the least satisfactory of a number of multiple-puncture disposable tuberculin tests.^{8,9}

These inconsistencies are obviously most unsatisfactory. Some investigators have suggested that the time of application of the tines

is important, but it would seem that one second is satisfactory.^{10,11} This is in accord with our own observations, as yet unpublished, that the introduction of old tuberculin from the tine unit into the skin depends on mechanical factors and not on dissolving from the tines. This suggests that an explanation for some of the great disparity in results could rest with the rapidity with which the disc is applied, skin tension, and the possible effects of different storage methods and variation in the temperature of skin and tine units.

Other issues were raised in the leading article, and we agree that further investigation of the variability of results obtained from experienced workers is highly desirable. We still feel we cannot support the use of the tine test unless the factors responsible for the varying results are identified and corrected.

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¹ Rosenthal, S R, *Journal of the American Medical Association*, 1961, **177**, 452.

² Capobres, D B, *et al*, *Journal of the American Medical Association*, 1962, **180**, 1130.

³ Badger, T L, Brietwieser, E R, and Meunch, H, *American Review of Respiratory Disease*, 1963, **87**, 338.

⁴ Manuel, S R, and Service, G F, 1974, personal communication.

⁵ Welke, H, Irsigler, G B, and Kleeberg, H H, *South African Medical Journal*, 1976, **50**, 2073.

⁶ Food and Drugs Administration, *Federal Register*, part II (Skin Test Antigens). Bethesda, Maryland, US Department of Health, Education, and Welfare, 1977.

⁷ Browder, A A, and Griffon, A L, *American Review of Respiratory Disease*, 1972, **105**, 299.

⁸ Furculow, M L, *et al*, *American Review of Respiratory Disease*, 1967, **96**, 1009.

⁹ Donaldson, J C, and Elliot, R C, *American Review of Respiratory Disease*, 1978, **118**, 843.

¹⁰ Schwartz, E, and Lubowitz, S, *American Review of Respiratory Disease*, 1963, **88**, 97.

¹¹ Patterson, I C, and Willey, R F, personal communication.

SIR,—Your leading article (19 May, p 1300) omits one important point in comparing the tine test with the Heaf and Mantoux tests. It is the only one that is stable at room temperatures and readily available without special equipment for use at the bedside.

Tuberculin testing in childhood remains a potentially important diagnostic measure which should be much more widely used in general practice than it now is. The tine test is prescribable on EC10. Systematic use in my own practice over the past 18 years has led to the identification of three new cases of open tuberculosis in adult contacts in a population of 2000.

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Injuries to boys who scramble

SIR,—Dr Maurice Place's paper (19 May, p 1322) provides interesting data relating to accidents to boys aged 6-16 indulging in motorcycle scrambling, and he concludes that

"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