reasonable hypothesis, extremely small. The magisterial critics are quite justified in saying that the probability of other events might be smaller still. I was told many years ago that a zoologist with a large family of daughters christened them with the generic names of Polychaeta, which are pleasant names—Nereis, Aphrodite, Eunice, etc. It is not likely to be a common practice, and the probability that the sample would contain two young women each named Nereis might very well be much smaller than the probability of including two sisters. But probabilities, like other measures, are great or small in relation to other probabilities. An addition to or subtraction from the annual income of, say, an emeritus professor, of £50 means much more to him than the same change in the national income means to Dr. Dalton. When an event occurs which on some hypothesis, say that of "chance," is very rare, any sensible person asks himself whether there is a reasonable alternative explanation. Dr. Rewell, like a sensible man, has entertained the alternative hypothesis—that the events were *not* independent, that there *might* be a genetic correlation. He did *not* say that correlation had been proved. If the rare event had been the appearance of two persons named Nereis, as the *a priori* probability that having a particular Christian name predisposes a person to die of cancer is extremely small, no sensible person would entertain the alternative. He would simply conclude that a very improbable event had happened—as improbable events do happen every day; in fact every time four people sit down to play a hand at bridge.—I am, etc.,

MAJOR GREENWOOD.

Animal Physiology

SIR,—Our attention has recently been drawn to a review of our book, *The Physiology of Farm Animals,* which appeared in your issue dated May 4. The reviewer calls attention to certain so-called "factual errors," 32 of which he claims to have listed, and later advises students desiring reliable information on the physiology of domestic animals to read the late Sir Frederick Smith's *Manual of Veterinary Physiology.* Of the so-called factual errors quoted by the reviewer, two—namely, that referring to the grazing horse and that referring to the cure of the swollen legs by moderate exercise—are included in the textbook to which he directs students' attention; and since he states that he is not aware "that horses whose legs become swollen or oedematous through much standing may often be cured by moderate exercise" we are left in considerable doubt as to whether the reviewer has carefully read the book he so warmly recommends. The description of the tuberculin test given in our book (p. 144) is correct and was not intended to be a description of the double intradermal test to which the reviewer is probably referring when making his criticism. In criticizing us for giving a range of temperature in the case of the hen, the reviewer is probably unaware that such a wide range exists; incidentally, the alleged correct temperature quoted—104.5° F.—is that of the broody hen and not the non-sitting hen, whose average mean temperature is 106.7° F. Moreover, the other so-called errors cited express differences of opinion and cannot be characterized as actual errors.

Finally, we would call attention to the fact that the book recommended as an alternative to ours, excellent though it may have been at the time of publication, is now quite out of date, particularly in the sections relating to endocrinology, vitamins, and the physiology of reproduction.

We apologize for troubling you with this letter, but thought it only right to inform you that the review itself contains errors and is scarcely helpful.—We are, etc.,

F. H. A. MARSHALL.
E. T. HALNAN.

School of Agriculture, Cambridge.

Shock Treatment of Bronchial Asthma

SIR,—I am much interested in Dr. E. Brauer's letter (June 1, p. 849) on the "pyrifer" treatment of asthma. The method he advocates is certainly heroic when one considers how asthmatics have a nasty and unexpected way—at times—of reacting severely to any foreign protein, especially if injected intravenously.

I have practised for some years now the non-specific protein treatment (using Koch's old tuberculin, and more recently P.P.D.), and although my series of cases is as yet too small for definite conclusions to be drawn, the results so far obtained seem to agree with those of Maxwell (*Journal,* 1930, **1**, 854) and others. Complete disappearance of the attacks can be obtained in over 30% of the cases treated. My final doses (as much as 0.5 ml. of the old tuberculin) are generally higher than those recommended by Maxwell and others. In the more refractory cases possibly an optimum maintenance dose at one to three months' interval might be advisable. But if one could feel entirely satisfied that the method suggested by your correspondent was really free from the possibility of dangerous reactions, a real advance in the treatment of asthma will have been made.—I am, etc.,

Kingussie. FELIX SAVY.

Congenital Malaria

SIR,—Since the publication of the paper on this subject (March 23, p. 432) I have had a report from Prof. Eckstein of three more cases that he had seen in his clinic at Ankara during the month of January, 1946. At that time the mean temperature was below 0° C. Under such conditions in Turkey all surviving anopheles are hibernating and do not bite. The mother of one of these babies was for some time in hospital under treatment for osteomalacia. For years she had suffered from untreated malaria. Her labour started with an attack of fever and she was delivered by Caesarean section. After birth the baby developed pyrexia. Its blood was negative on the first examination but positive on the second.

Dr. R. F. Jarrett (April 27, p. 662) has himself seen cases of congenital malaria during his stay in Turkey. In his bibliography he refers to the case which occurred in London and was reported by Tanner and Hewlett (*Lancet,* 1935, **2**, 369). Sir Philip Manson-Bahr was called in consultation to this. They suggested that in the absence of a known placental trauma or infarct, premature placental separation was the most probable cause of the transmission of the infection.

Since my return from abroad I have had access to further literature. K. T. Chen *et al.* (*Chinese Medical Journal,* 1944, **62**, 190) have collected 39 cases of congenital malaria from the world literature. In some of these the infant's blood was positive on the first day after birth. The case they reported was that of a baby born in mid-winter in Chungking. Wickramasuriya (*J. Obstet. Gynaec. Brit. Emp.,* 1935, **42**, 816) has had an extensive experience in Ceylon and has made a comprehensive study of malaria in pregnancy. He is of the opinion that transplacental foetal infection with malaria occurs more often than is supposed. In his paper he records 6 cases of babies in whom transplacental foetal infection was proved. The infecting parasite was *P. falciparum.* He has demonstrated infecting parasites in fresh foetal brain squash under the microscope without any staining. In these cases he has easily seen living and dead parasites in the cerebral capillaries.

Placental transmission is a subject worthy of further research. The wonder is that the disease is not transmitted to more of the offspring of the millions of pregnant women suffering from malaria.—I am, etc.,

London. W. C. W. NIXON.

*** This correspondence is now closed.—ED., *B.M.J.*

Infantile Paralysis Fellowship

SIR,—May I draw the attention of the medical profession to the Infantile Paralysis Fellowship? Founded in January, 1939, the I.P.F. now has a membership of some seven hundred. We are affiliated to the Central Council for the Care of Cripples, and out of our executive committee of ten, eight are disabled persons. It is one of the few organizations *for* the disabled run *by* disabled. With the recent increase of fresh cases—both civilian and Service—many a lonely sufferer from anterior poliomyelitis may be happy to associate with fellow-sufferers who have faced and largely overcome the problems of this terrible scourge, and we appeal to members of the medical profession to bring our existence to the notice of any polios they may be treating. Briefly, our objects are:

1. To associate sufferers from infantile paralysis in fellowship for the encouragement and development of their interests and abilities.
2. To find means of training its members and, where necessary, of re-educating them, for occupations in which they can support themselves and make their contribution to the economic and social life of the community.