tuberculosis may exist or may develop" (*Erythema Nodosum*, John Wright and Sons, 1928), founding my opinion on the fact that in some 20 per cent. of my cases there was definite evidence of tuberculosis at some period of the patient's history. The higher figures obtained by Dr. Collis are, of course, due to the routine tests he applied—the skin reaction to tuberculin, and the detection of tubercle bacilli by the gastric lavage method. When one recalls the difficulties there are in recognizing the tubercle bacillus—difficulties recently emphasized by Wilson's Report to the Medical Research Council (No. 18)—it seems possible that the percentage of cases ascribed to tubercle may be found later to have been unduly high.

As one who has persistently maintained that erythema nodosum is a definite specific fever, due to some variety of streptococcus, I am still unrepentant, and regard Dr. Collis's results with streptococcus endotoxin (of which he kindly sent me a supply) as confirming this view. I could have wished, however, that the word "rheumatic" had been omitted from this report, and in all cases the word "streptococcal" substituted.

Gradually the group of erythematous diseases is being cleared up, just as the so-called rheumatic diseases have been reclassified and defined. Whether there be, or not, a separate disease, erythema nodosum, due to a definite variety of streptococcus is a matter of minor importance. The real practical point is that all clinicians shall recognize that erythema nodosum is in the main a condition associated with tuberculosis, and cease to regard it as a skin disease or a manifestation of rheumatism.—I am, etc.,

Clifton, Bristol, Dec. 29th, 1933. J. O. SYMES.

Pharyngeal Haemorrhage

SIR,—I was interested in Dr. P. Shackleton's two cases of pharnygeal haemorrhage recorded in the *Journal* of December 23rd, 1933 (p. 1167). Severe and sometimes fatal haemorrhage arising from a tonsil abscess is rare, but several cases have been recorded.[1] [2] I have seen only two cases, and another of aneurysm of the internal carotid in a boy of 8. The aneurysm followed the incision of a tonsil abscess. The boy nearly died of haemorrhage, and had a temporary hemiplegia. The haemorrhage is due to an erosion of a large vessel, and not to the incision of the abscess. The incision has been unjustly blamed.

Erosion of the internal carotid is more likely to occur when there is a siphon bend of the artery, bringing it into an abnormal position immediately behind the pharyngeal wall and posterior pillar of the fauces. This is the site of a lateral retropharyngeal tonsil abscess. In such cases of haemorrhage the tonsil should be enucleated and the bleeding point secured. This has been successful in two cases to my knowledge. The bleeding may arise from a large vessel, but not always from the carotids. The tonsil can be enucleated under local anaesthesia, but a general anaesthesia is usually necessary, when morphine and chloroform is definitely indicated, and not ether. If it is certain that the haemorrhage arises from the carotid vessels the bleeding point can be secured by an external incision in the neck, but I have no experience of this method of attack except in war injuries of the neck. That is another story. Incidentally, war injuries of the internal carotid artery sometimes resulted in death from an embolus or vascular lesion of the brain, a possible cause of death in Dr. Shackleton's first case.—I am, etc.,

London, W.1, Dec. 25th, 1933. EDWARD D. D. DAVIS.

[1] Hutchison, Arthur J.: *Journ. Laryngol.*, April, 1919, p. 122.
[2] Negus, V.: *Trans. Laryngol. Sect., Roy. Soc. Med.*, February, 1930, xxiii, No. 4, p. 516.

Chilblains

SIR,—I was interested to read Dr. J. T. Ingram's letter in the *Journal* of December 23rd, 1933 (p. 1184). Having used thyroid in small doses (t.i.d.) in the treatment of chilblains for nearly ten years I can confirm its usefulness when restricted to persons not under weight. I have sometimes found it necessary, however, to push the dose a little to get the best effect. For persons who are under weight calcium iodide (gr. jss, t.d.s., p.c.) is useful but not quite so effective. In my own practice I have never appeared to get any convincing results from calcium and parathyroid, or from colloidal calcium injections.

General hygienic supervision is, however, at least as important as any internal or local remedy in the treatment of chilblains. Thus, a well-balanced diet (with additional fats if these can be tolerated by the undernourished patient or a suitable form of them found for her), attention to the bowels, adequate bodily exercise supplemented by limb exercises if necessary, warm underclothing, warm leg- and foot-wear, and non-exposure of the extremities to excessive heat and cold are all indicated. Even improvement in the housing conditions may occasionally be a factor in the treatment of chilblains; for the fact of a good hot-water, as well as an internal, water supply being available for increasingly large numbers of the population has undoubtedly, I think, tended to lower the actual incidence of chilblains.

Just one word finally as to the assessment of any treatment for this condition. A proper therapeutic test requires a really prolonged cold spell for the establishment of any remedy. How often in this changeable climate of ours does a sudden or a gradual rise of temperature so obviously invalidate our therapeutic results!—I am, etc.,

J. INGLIS CAMERON, M.B., F.R.F.P.S.

Glasgow, Dec. 27th, 1933.

Blood Transfusion in Puerperal Sepsis

SIR,—The letter from Mr. E. Hesketh Roberts published in the *Journal* of November 11th, 1933 (p. 894) is interesting as it helps to explain the dramatic results which attended the treatment of three cases of puerperal sepsis by blood transfusion. This is, of course, recognized as being of value in the treatment of septicaemic cases, but in the literature at my disposal I have been unable to find any reference to its employment as a routine in cases of puerperal sepsis. I have ascribed its success in these cases to the replacement of the volume of blood lost at confinement, and, thereby, to the stimulation of the powers of resistance which had been lowered by this loss. This I think partly agrees with the theory and facts as expressed by Mr. Hesketh Roberts.

My first case occurred three years ago, when, after manual delivery of the placenta, sepsis developed on the fifth day, and in spite of treatment the hyperpyrexia continued and the condition was grave. A blood culture on the ninth day gave a growth of short-chained streptococcus, and the patient was given 500 c.cm. citrated blood on the tenth day with 50 c.cm. anti-streptococcal serum. On the twelfth day of illness the temperature fell to normal, and continued so for the remainder of her convalescence, the only complications being two pyaemic abscesses, one in the upper arm and the other in the vulva, which, on being drained, cleared up. The second case was seen at the end of the second week of illness after treatment with intrauterine glycerin, collosol iodine, etc., and was complicated by a *B. coli* cystitis. Blood culture on the eighteenth day was sterile; 500 c.cm. of citrated blood was given on the twentieth day, and on the twenty-third day of illness the temperature fell to normal, after which convalescence was uneventful. The

third case developed sepsis after a Caesarean section. The condition was critical on the sixth day, and the patient was given 400 c.cm. of citrated blood while in a delirious and semi-conscious condition. Temperature fell next day to normal, and she was sitting up in bed reading after forty-eight hours. Convalescence was, however, complicated by the development of an extra-peritoneal abscess round the site of the abdominal scar, which nevertheless settled down after drainage.

It is not suggested that blood transfusion should be regarded as a specific or only employed as a *dernier ressort* in critical cases, but it is thought that its employment would appear to have some physiological justification, and at any rate should do no harm.—I am, etc.,

W. LIONEL FRETZ, V.H.S.,
Rawalpindi, N. India, Dec. 9th, 1933. M.B., B.S., Major R.A.M.C.

Use of Quinine in Normal Labour

SIR,—On reading Dr. Douglas Mitchell's letter in your issue of July 15th, 1933 (p. 126), I determined to try his method of giving small doses of quinine during the last few weeks of pregnancy.

In November I was consulted by a primigravida, aged 21 years; last menstrual period was one week late, and occurred at the end of March; general build slight; teeth all perfect; pelvic measurements normal. She did not come, as directed, to see me again four weeks later, but on December 7th sent for me. At 2 p.m. she had experienced some discomfort "in the back passage" and had a bowel motion. At 4 p.m. she had slight recurrent pains; at 5.40 she delivered herself of a female child weighing 6¾ lb. The child was delivered by one long continuous pain, which expelled all the uterine contents, including placenta. The nurse, who was present, stated that she hardly seemed to be in labour before this pain occurred. The birth was followed by a fairly brisk haemorrhage for five to six minutes, controlled by pituitrin, and her subsequent history was entirely uneventful.

Had I administered the doses of quinine I should have been much impressed by the easy labour that followed such treatment. I may add there was no doubt that the patient was a primigravida.—I am, etc.,

A. G. HAWTHORNE ENGLISH.
Birkenhead, Dec. 21st, 1933.

Vision of Colour and Brightness

SIR,—I have read with the greatest pleasure the report of Professor Haldane's lecture in the *British Medical Journal* of December 23rd, 1933. I have for many years given similar demonstrations to the Physiological Society and elsewhere without anyone disputing the facts, and with the same conclusions. These facts were predicted by my theory of colour vision (see "Simultaneous Colour Contrast," *Journal of Physiology*, 1911; "Simultaneous Colour Contrast," *Proceedings of the Royal Society*, 1912; "Colour Adaptation," *Proceedings of the Royal Society*, 1913; *Physiology of Vision*, G. Bell and Sons, London, 1920).—I am, etc.,

F. W. EDRIDGE-GREEN.
Board of Trade, S.W.1, Dec. 24th, 1933.

SIR,—Professor J. S. Haldane, in his interesting address (*Journal*, December 23rd, 1933, p. 1153), admirably refutes mechanical materialism, but in doing so he unfortunately falls into the camp of idealism. Materialism to-day has progressed considerably since the days of the French mechanical materialism, with its ponderable matter like billiard balls and its knowledge of absolute truth. Materialism to-day simply states that objects exist independently of our sensations, and these give us a correct reflection of this objective reality, providing us with relative truths.

Professor Haldane states that we cannot find consistency, which is the mark of truth, in the supposed correspondence of our perceptions with an objective world existing apart from our sensations. He also states that our physical and mathematical interpretation of phenomena is only partial and abstract. Naturally we agree that these interpretations are only partial—that is, that biological phenomena can never be fully explained by physical, chemical, and mathematical means only. We must also use, as Professor Haldane states, "the biological conception of life" in our interpretations; but we deny that these interpretations are abstract and have no mark of truth. These physical, chemical, and mathematical interpretations are true reflections of objective biological reality. The mere fact that we can use these interpretations for our own purpose in practice points to their correspondence to objective truth and reality, for if our interpretations of them were at all divergent from their objective existence then we would fail in our use of them, and only then would "Nature mock at us."

To take the example of brightness and colour: although Professor Haldane states "we cannot have any objective measure or standard of brightness and colour; we can only have a 'human' measure thereof," we must realize that our human measure and standard gives a true, if only approximate, measure and standard of reality, as evidenced in our use of these standards with success in our practical day-to-day life. If, for instance, a new colour or degree of brightness is discovered existing in Nature to-morrow, by means of any instrument, surely it does not imply that the colour or degree of brightness did not exist yesterday because we did not perceive them. We are thus constantly making and renewing our standards, which, although relative, are continually approaching absolute reality as our knowledge advances with the progress of the human race.

Professor Haldane concludes by co-ordinating objective and subjective worlds in his biological interpretation of phenomena. Unfortunately his theory is an idealistic one, for his objective and subjective worlds are both spiritual—that is, mental—in form. His physical is an abstraction from the biological, the biological is an abstraction of the personality or psychological, and the individual personality is derived from the divine personality: thus, after a whole series of abstractions, we come to the supreme abstraction. Professor Haldane thus teaches that mind is the prius, and matter is derived from it, whereas the biological sciences and the law of evolution prove that mind is derived from matter. With the growth and evolution of the nervous system in animals we find simultaneously the growth and evolution of the mind, until now, in man, it has become the highest function of matter—that is, the brain.

We may well ask Professor Haldane what was the interpretation of the world millions of years ago, when the world was a molten mass and no life existed thereon. The answer, I suppose, would be, "The Creator's only"; and at the time of the ichthyosaurus the answer would be, "That of the ichthyosaurus plus the Creator." Until we realize that matter is the prius and mind is derived therefrom—until we recognize the existence of an objective reality and our approximate interpretation thereof—we shall find scientists tending to lapse into fideism.—I am, etc.,

S. LEFF.
London, E.5, Dec. 31st, 1933.