embolism of the pulmonary artery, even in cases conducted with aseptic methods. This fact should lead to precaution as to the dietary of pregnant women, especially in the upper classes, who are sometimes over-nourished.

The tendency to viscosity of the blood is to be met by diminished use of animal food and increased use of fresh, especially green, vegetable food. Where the circulation is languid alcohol in some form is of much value. Milk should be sparingly used and diluted with sodium citrate and soft water. The use of ether as an anaesthetic may perhaps prove unadvisable in cases where a hyperinotic condition of the blood is suspected.—I am, Sir, yours faithfully,

Grosvenor-place, S.W., Feb. 10th, 1913. DYCE DUCKWORTH.

RESEARCHES IN COLOUR VISION.

To the Editor of THE LANCET.

SIR,—I have just been reading a book with the above title by Sir W. Abney with the greatest gratification. When a scientific man has striven for the greater part of his life, against the greatest opposition, to establish certain new principles, facts, and methods, it is very pleasing to find that a man who has been his chief opponent should, though rather late in the day, have adopted those methods.

For 24 years I have advocated certain methods of testing for colour-blindness based on my own theory of colour vision. I have insisted, in opposition to Sir W. Abney, that colour names were essential in any test, and that coloured lights should be used instead of coloured wools. The first test mentioned is the Holmgren wool test; to this two new test skeins have been added similar to those in my classification test. Sir W. Abney writes: "If the examinee is asked to name some of the confusion colours, the giving of a wrong name to any of them will confirm what has probably been found out by the matches." The word "probably" is very significant. The next test mentioned is one by water-colour washes, in which squares of pale colours are named (see p. 105 of my book, "Colour Blindness and Colour Perception," International Scientific Series, 1891 and 1909). Sir W. Abney gives laboratory tests in which the examinee is asked to name portions of the spectrum in a similar manner to that described in my book, p. 111. The imperfect lantern test recommended by the Departmental Committee of the Board of Trade is described. This would have been a better copy of my lantern and more efficient if means for regulating the luminosity had been adopted. A test by simultaneous contrast is next mentioned. I was the first to point out that simultaneous contrast is more marked for the colour-blind than the normal-sighted (see my book, p. 270). My only regret is that Sir W. Abney has given two other methods—namely, matching yellow with a mixture of red and green and estimating the relative luminosities of red and green by means of the flicker method of photometry. Both of these are based on the trichromatic theory, and I have shown both to be erroneous, but only within the last few weeks; that is to say, a man who is so colour-blind as to confuse full red and full green may appear absolutely normal-sighted by these methods, whilst a normal-sighted person may appear colour-blind.

As Sir W. Abney states that his book is the substance of a large number of communications to the Royal Society, and I am unable to find the methods described by him in them with the exception of the two which I regard as erroneous, I think I may draw attention to one slight omission in that he has not mentioned my name throughout the book.

I am, Sir, yours faithfully,

F. W. EDRIDGE-GREEN.

The Institute of Physiology, University College,
London, Feb. 7th, 1913.

THE PATHOGENICITY AND VIRULENCE OF BACTERIA.

To the Editor of THE LANCET.

SIR,—In THE LANCET of Feb. 8th, p. 413, it is stated by Dr. F. H. Thiele and Dr. Dennis Embleton that, anticipating our criticism, they took full precautions to guard themselves against error in their recently published experiments. A year ago we published charts which showed that the use of water collected from either the condensing drip or the collecting tank of the ordinary distilling apparatus

leads to serious error in the study of experimental fever. This is due to the infection of both worm and tank that frequently occurs in consequence of the aspiration of infected air caused by cooling. We also showed that the use of filtered water introduces a further source of error owing to contamination of the filtrate with calcium and other salts if hardware filters are employed. Dr. Thiele and Dr. Embleton admit the use of the hardware filter and of water from the condensing drip in all their experiments, and it is therefore difficult to understand in what sense their precautions against error were complete. If, however, they will collect into sterile agar tubes 1 or 2 c.cm. of water direct from the condensing drips of a series of the distilling vessels in ordinary laboratory use, they will be able to see for themselves the percentage of tubes in which gross infection will have taken place. And if, further, they will add ammonium oxalate to water after passage through the Pasteur-Chamberland filter—which they apparently believe to be innocent of calcium—they will be able to confirm our observation that it contains calcium in considerable amount. After repetition of these two experiments they will, we feel sure, be the first to admit the accuracy of the demonstration we gave last March that water after filtration through hardware filters is no longer pure water, and that even unfiltered water when it fails to produce a fall of temperature when injected is also not pure water. As regards the use of the guinea-pig as a suitable animal in which to study experimental fever by repeated observation, the point of our remarks on the subject was that very inconstant temperatures had been found sometimes to occur in uninjected animals in spite of the fact that they had been kept for weeks on a constant and adequate food-supply. In view of this fact, study of an equally large number of animals will convince the most sceptical observer that this—the third source of error to which we referred—is one that cannot be ignored.

We are, Sir, yours faithfully,

E. C. HORT.

The Lister Institute, Feb. 8th, 1913.

W. J. PENFOLD.

AN INTERESTING EXHIBITION OF RAYNAUD'S PHENOMENA.

To the Editor of THE LANCET.

SIR,—The interesting article by Professor T. Wingate Todd in THE LANCET of August 10th, 1912, on The Vascular Symptoms in "Cervical" Rib, suggests that the following curious case may be of interest.

A B, a strong, healthy man, first came under observation in May, 1911. He is 45 years of age and married, a teetotaler, and has three healthy children, youngest 8 years; no history of syphilis, and has never had any illness. His occupation causes him to do a good deal of hammering with a heavy hammer. Complained first of "pins and needles" pains in the ulnar side of his right hand and of two inner fingers, severe enough to keep him awake at night. Liniments and the faradic current gave no relief. A small swelling about the centre of the ulnar side of the right forearm was explored for a foreign body, but nothing was found. The tingling pain soon spread to the radial side of the hand, and was accompanied by swelling and weakness of grasp. The reflexes, wrist and elbow jerk, were normal; cutaneous sensation to touch, pain, and temperature was diminished. Three months from the commencement of the attack the hand exhibited the milder phenomena characterising Raynaud's disease—viz., local syncope, especially after elevation and immersion in cold water, and local asphyxia after hanging the hand down. About this time a small swelling became manifest near the proximal end of the extensor secundi internodii pollicis tendon. This also was explored and nothing found. The patient was told to wear a thick glove and to avoid cold water. Owing to the severity of the pain small doses of anodynes were necessary to procure sleep. About this time it was noticed that the right subclavian pulse was very much more difficult to feel than on the left side, the brachial pulse was very feeble, and the radial and ulnar arteries imperceptible. Faradism having quite failed to give relief, early in 1912 galvanism was tried, with the approbation of Dr. W. Mitchell Stevens, of Cardiff, who took great interest in the case. The patient began with eight cells of a constant current battery of 24 dry cells, going on to 16 and using the method originally

recommended by Raynaud and practised by Sir Thomas Barlow—that is, one electrode over the cervical spine, and the other in a basin of warm water in which the hand was immersed. The patient, being desirous of saving his hand, persevered with this treatment, although many small burns occurred on the skin of the nape and upper arm from the strength of the current employed. The vaso-motor disturbances and the pain gradually lessened. The grip strengthened, and the patient returned to duty in July, 1912, doing light work at first, and has now resumed his full work. Dr. Stevens stated that the maximum blood pressure was 130 mm. No tumour could be felt anywhere in the course of the arterial trunk of the affected limb. Embolism appears out of the question, as his heart is perfectly healthy, and the symptoms commenced in the ulnar nerve. Thrombosis also seems out of court on the same grounds; and the fact of recovery after the use of galvanism points to the phenomena being due to implication of the branch of the cervical sympathetic chain which controls the vascular supply of the upper limb.

After reading Professor Wingate Todd's interesting cases, we naturally expected to find the shadow of a cervical rib in this case, but no trace of such a shadow was found. Possibly in the cervical rib cases the rib is merely a predisposing cause, and some slight injury to the root of the neck is the exciting cause of the pseudo-Raynaud symptoms. Obviously the abnormal rib has been present from birth, whereas the symptoms, instead of being congenital, appear in active adult manhood, when traumatism is likely to occur.

I am, Sir, yours faithfully,

EDWARD C. B. IBOTSON.

Briton Ferry, Glamorgan, Feb. 8th, 1913.

ARTIFICIAL RESPIRATION.

To the Editor of THE LANCET.

SIR,—One strongly deprecates the habit nowadays of going outside one's province. This is an example, when an attempt is made to explain in the physical plane what has evidently taken place in the spiritual plane. However, whatever has been paraphrased loosely from the received Hebrew text is due to unskilfulness and arbitrariness. The powers, too, of the translators were limited. The word in the Septuagint which is translated as child in the English version is *παῖδιον*. This is a diminutive from *παῖς*, and means a young or little boy. We may take it that the person was a child and not an adult. In the same passage the same term *παῖδιον* is used of the servant, and in classical Greek it means a young slave. The Verse 34 which has been quoted in your issue of Feb. 1st is part of a paragraph. The paragraph commences at Verse 18 as follows: "18. ¶ And when the child was grown, it fell on a day, that he went out to his father to the reapers. 19. And he said unto his father, My head, my head! And he said to a lad, Carry him to his mother. 20. And when he had taken him, and brought him to his mother, he sat on her knees till noon, and then died." From this last Verse 20 some considerable period has elapsed before we come to the remarkable Verse 34, in which the child seems to have been revived by what Professor Leonard Hill would style "Elisha's method of artificial respiration." It might interest a few to fathom the nature of the child's malady from the surgical or medical point of view if we venture so boldly as to go outside our definite range. I am, Sir, yours faithfully,

Alton, Hants, Feb. 8th, 1913.

JOHN FREDK. BRISCOE.

To the Editor of THE LANCET.

SIR,—Since you published my letter in THE LANCET of Feb. 8th in reply to Dr. Leonard Hill I have discovered the following passage with reference to the matter in "The Diseases of the Bible," by Sir Risdon Bennett, M.D., F.R.S., Ex-President of the Royal College of Physicians (The Religious Tract Society, London, 1887):—

Disease of the Shunammite Woman's Son.—"And when the child was grown, it fell on a day, that he went out to his father to the reapers. And he said unto his father, My head, my head. And he said to a lad, Carry him to his mother. And when he had taken him, and brought him to his mother, he sat on her knees till noon, and then died. And she went up, and laid him on the bed of the man of God, and shut the door upon him, and went out." This case has usually been considered as one of sunstroke or insolation, and this is probably the correct view. But if the child's exclamation, "My

head, my head," is to be understood as intimating sudden severe pain of the head, it is possible that it may have been an instance of sudden *meningitis* (inflammation of the membranes of the brain) supervening in a delicate child. There are several varieties of sunstroke, all of them endangering life, though many cases recover. In the severer form, or sunstroke proper, the brain and nerve-centres are overwhelmed, the respiration and circulation are arrested, and death may ensue rapidly. The patient becomes unconscious, the pulse fails, and the skin is cold. Such cases, if not immediately fatal, often prove so in the course of a few hours. If eventually they recover, it is after a period of reaction accompanied by symptoms of severe injury to the nervous system. That death had actually occurred in this instance there can be no reasonable doubt. A period of ten or twelve hours at least must have elapsed before the arrival of the prophet. And it was not till after having kept the child on her knee till noon, and then seeing that he was dead, that the mother hastened to fetch Elisha. During her absence the child was laid on the bed of the prophet, and the door shut upon him; no remedies therefore were used, nor any attention given to the child. Gehazi, who arrived first at the house, on laying the prophet's staff on the face of the child, found there was "neither voice nor hearing." "And when Elisha was come into the house, behold, the child was dead, and laid upon his bed." The body was then cold, and it was not till after Elisha's prayer and stretching himself on the child, that "the flesh of the child waxed warm," and that subsequently he opened his eyes, after sneezing seven times. There are here no natural means, the use of which could explain the recovery.

I am, Sir, yours faithfully,

Feb. 10th, 1913.

SEARCHER.

SERUM TREATMENT IN MELÆNA NEONATORUM.

To the Editor of THE LANCET.

SIR,—A reference is made in the Annotations in THE LANCET of Feb. 8th, p. 403, to serum treatment in cases of melæna neonatorum by Dr. Franz. Bearing on this subject, I referred to this method of treatment in some detail in a thesis which is reprinted in the last volume of the Guy's Hospital Reports (Vol. LXVI., p. 328). It is here shown that Welch and Nicholson, of Philadelphia, have had equally successful results in the treatment of these cases with normal human blood serum.

I am, Sir, yours faithfully,

Lowestoft, Feb. 10th, 1913.

WILSON TYSON, M.D. Cantab.

THERAPEUTIC USE OF VACCINES IN TYPHOID FEVER.

To the Editor of THE LANCET.

SIR,—In THE LANCET for Jan. 18th, p. 184, under "Annotations," is a review of my paper in the *American Journal of the Medical Sciences* for October on the therapeutic use of vaccines in typhoid fever. I wish to thank you for the very careful, complete, and concise review you gave of this paper. There is one mistake, however, evidently a typographical error, to which I would like to call attention. In the twelfth line of the second column occurs this sentence: "He begins with 500 millions, and at intervals of 14 days, as long as necessary, gives a dose increased by 100 millions." The sentence should read: "He begins with 500 millions, and at intervals of *four* days, as long as necessary, gives a dose increased by 100 millions." I hope you will make this correction, as the difference is very important. Your review will be read by more people than will see the original paper.

I am, Sir, yours faithfully,

New York, Jan. 31st, 1913.

J. G. CALLISON, M.D.

THE ORDER OF ST. JOHN OF JERUSALEM.

To the Editor of THE LANCET.

SIR,—On the last day of January the hand of death removed from amongst us three really distinguished men: the Earl of Crawford, Admiral Sir F. G. D. Bedford, and Lord Ilkeston. The first was a scientist and a soldier, the second a naval administrator and a fighting sailor, the third a medical man and a politician. The two first named were Knights of Grace of the Order of St. John of Jerusalem in England; the last was not. The practice of conferring the decorations of this Order upon naval and military officers, members of the peerage, and gentlemen with influential friends at Court, to the exclusion of those who have actually done something towards the relief of suffering and sickness, has been prevalent for a long time; but it is quite time that a reform should be instituted. I am not unmindful of the fact that there are a few medical men.