Correspondence.

"Audi alteram partem."

PERICARDIAL FRICTION AND CORONARY THROMBOSIS.

To the Editor of THE LANCET.

SIR,—In an annotation in THE LANCET of June 5th (p. 1232) you call attention to transient pericardial friction as a sign of coronary thrombosis, and to six cases reported by Dr. L. W. Gorham, of Albany. You speak of this condition as "almost unrecognised," and "not mentioned in the text-books." Dr. Gorham's cases are very interesting, but it is hardly true that patches of pericarditis at the sites of coronary thrombosis have not been fully recognised in England. If your readers will do me the honour to turn to my work on "Diseases of the Arteries," Vol. II., pp. 460ff, they will find these events discussed at some length. The occasional relation to angina pectoris also is indicated, and suggestions made to explain the occurrence of anginal pains in some cases in which, with coronary thrombosis, there is no pericarditis.

I admit I have not had the good fortune to meet with the considerable pathological proofs which have fallen to the lot of Dr. Gorham, but do not let us in England appear ignorant of the subject.

I am, Sir, yours faithfully,

Cambridge, June 12th, 1920.

CLIFFORD ALLBUTT.

WAR OEDEMA AND EPIDEMIC DROPSY.

To the Editor of THE LANCET.

SIR,—I have read the letter by Dr. J. A. Nixon in THE LANCET of April 24th on the subject of War Œdema and Epidemic Dropsy. I do not consider that any confusion will result from Dr. A. D. Bigland's valuable article on "Œdema as a Symptom in So-called Food-deficiency Diseases"; on the contrary, it gives a very clear and complete account of our present knowledge of the subject. Indeed, Dr. Nixon himself somewhat obscures the issues when he states that "the disease called by Macleod epidemic dropsy, with its itching, burning, and erythema or urticaria which preceded or accompanied the anasarca, was probably ankylostomiasis." In the course of my investigations on the Œtiology of Epidemic Dropsy in India, published in the Scientific Memoirs of the Government of India, Nos. 45 and 49, I obtained no evidence that the disease was ankylostomiasis; on the other hand, the results of my researches pointed definitely to epidemic dropsy, as seen by me in Calcutta and other places in India, being a form of food-deficiency disease. The statement that "this dropsical disease was by no means confined to the impoverished and sickly" proves nothing, because it is necessary to remember in this relation that the diet of well-to-do people, although expensive, may be, from the modern view-point, nutritionally deficient; thus highly polished rice, which is consumed by the richer classes in India, is more costly than the coarser varieties of rice, but, as is now well known, the former is more deficient in vitamins than the latter.

I am, Sir, yours faithfully,

E. D. W. GREIG.

Pasteur Institute of India, Kasauli, May 21st, 1920.

THE STANDARDISATION OF COLOUR PERCEPTION.

To the Editor of THE LANCET.

SIR,—I have read Mr. Herbert Parsons's letter in THE LANCET and have to thank Mr. Percy Dunn for his reply in your last issue. I shall welcome Mr. Parsons's attack on my views, but I hope it will not appear in a lay paper. The editors of different lay papers have received communications depreciating my work, and though none of these have been inserted, in any case I wish to reply through a scientific medium. Mr.

Parsons should be the last person to belittle my work, as he is now using my methods after having opposed them long and fiercely. In 1889, as member of the International Code of Signals Committee, I constructed the lantern, which is now the official test of the navy, for the Board of Trade. Opinion at that time, however, was in favour of the wool test and the non-use of colour names. A special committee of the Royal Society was appointed which recommended the wool test. This discredited me for many years. The wool test is now obsolete, as more than 50 per cent. of dangerously colour-blind persons can pass it and a very large percentage of normal-sighted are rejected. Yet in 1912, in a new edition of his book, Mr. Parsons wrote:—

"The fundamental axiom in colour testing is that no importance should be attached to the naming of colours..... Holmgren's wools are sufficiently good."

This statement was made when nearly the whole world had recognised the inefficiency of the wool test and the necessity for the use of colour names. In the Ophthalmological Section at the British Medical Association meeting at Brighton, 1913, a resolution was proposed condemning the wool test. Mr. Parsons was the sole member who voted against the resolution.

I have for many years whenever a foreign scientist interested in vision has visited me, and has told me that he was going on to one of my opponents, requested him to let me know of any objection made so that I could answer it. In every case on returning he has told me that certain particular people, the same in every case, had nothing good to say of my work, but were quite unable to point out any fact or deduction which was wrong. I naturally look forward to having something to which I can reply.

I am, Sir, yours faithfully,

London, June 12th, 1920.

F. W. EDRIDGE-GREEN.

To the Editor of THE LANCET.

SIR,—Mr. Percy Dunn's saeva indignatio has led him into making some rhetorical exaggerations. "The 100 per cent. of those who have failed to prove that any of his statements are wrong" is one such. The following is an example refuting this pronouncement. Lord Rayleigh stated that he could distinguish between the wave-lengths included in a monochromatic division to the extent of discriminating between the colours of the two D lines.¹ Dr. Edridge-Green traversed this statement and attributed the error to "the admixture of small quantities of white and coloured light and to certain physiological influences."² Dr. Wm. Watson conclusively disproved any such influence of admixture with white light,³ and found that 14 observers gave the following mean values with sodium light: width of monochromatic patch = 4.5 μ ; difference in wave-length easily detected as a change in hue = 1.4 μ . It is further to be remembered that there is a wealth of argument against some of Dr. Edridge-Green's theories, so that opponents may not have felt it necessary to controvert them. Thus, I know of, at any rate, one scientist whom Dr. Edridge-Green claims as a strong adherent, who yet dissents entirely from his theory as to the action of the visual purple.

With regard to the "prejudicial attitude" assumed against Dr. Edridge-Green, this, if true—and I believe it originated in events occurring before I became actively interested in the subject—was a personal matter and does not affect the scientific arguments. I have yet to be convinced that the Royal Society was so influenced as to commit the worst crime against science—that of perverting scientific truth from ulterior motives.

I have already said that I consider Dr. Edridge-Green's colour perception lantern good, and it is not surprising, for reasons stated in my last letter among others, that various Government departments have adopted it. I fail to follow Mr. Dunn's arguments that a test requiring skilled knowledge is better than one more "fool-proof." Would a test for visual acuity be

¹ Proc. Roy. Soc. B., 1910, vol. lxxxii., p. 458.² Ibid., 1911, vol. lxxxiv., p. 116.³ Ibid., 1911, vol. lxxxiv., p. 118.

better if a high degree of special training were required to use it? That depends, I take it, upon the degree of accuracy required for the given purpose. I must confess that I cannot interpret the innuendo contained in the last sentence of Mr. Dunn's letter.

I am, Sir, yours faithfully,

J. HERBERT PARSONS.

Queen Anne-street, Cavendish-square, W., June 14th, 1920.

POISONING BY EXHAUST GAS.

To the Editor of THE LANCET.

SIR,—On May 8th I was called to see two children, aged respectively 11 and 6 years, with the following history:—

They started to drive 10½ miles in a half-covered business car, open to the front, which contained groceries. They were inside the car, the mother sitting on the front seat by the driver. When they had driven 10 miles the mother heard one of the children crying; she got down and found the younger child delirious, with her head fallen forward; the elder child was trying to comfort the younger one. She got them out of the car, when the elder child staggered about the road and fell down. She put them on the seat by the driver and came on to Lichfield.

I found them both in a state of semi-collapse. Pulse small and feeble. Faces pale. No cyanosis. They were constantly sick, which ultimately became bilious in character. In the evening the elder child complained of headache. The younger child was better, but developed a croupy cough during the night, with no physical signs and no infection of the fauces. The next day they were better and taking food, but the elder child was feeble and wanted to lie down when she was dressed. They went home in the afternoon, and I am told that the cough in the younger child continued for three or four days. They ultimately got quite well.

No. 3 spirit was being used in the car, the exhaust pipe of which only reached half-way to the bottom of the car. What was the gas which caused these symptoms? Does No. 3 spirit contain benzol, which, I believe, contains toluol and xylol? Was it a case of toluol poisoning?—I am, Sir, yours faithfully,

Lichfield, Staffordshire.

GEORGE W. HOMAN.

* * Cases of serious disturbance to health by the inhalation of light hydrocarbons, used as propellants of motor vehicles, are well known, but the above letter raises the question whether, in the circumstances described, the poisoning was due to leakage of hydrocarbons or to the products of their complete or incomplete combustion. The symptoms described are in accord with those produced by benzene inhalation (or petrol itself), and toluene vapours (methyl-benzene) act much in the same way as benzene. References to similar occurrences will be found in issues of THE LANCET dated April 5th, 1913 (p. 975), Jan. 27th, 1917 (p. 161), and Oct. 12th, 1918 (p. 494).—ED. L.

THE "TOXICITY" OF METHYLENE-BLUE.

To the Editor of THE LANCET.

SIR,—In your issue of Feb. 17th, 1917, you were good enough to insert a short letter on the above subject. Since then, while employed as consultant in malaria for the Scottish Command, I have been able to make more extended researches on the value of this remedy in the treatment of malaria and other affections, and also especially to note the presence or absence of toxic symptoms.

The total quantity of methylene-blue used in one hospital alone (the Edinburgh War Hospital, Bangour) amounted to over two pounds in weight, representing about 5000 doses of 3 gr. each. The number of malaria cases treated numbered over 3000. It may therefore justly be claimed that conclusions reached from observations on such a wide base are worthy of consideration.

I wish to combat the idea that methylene-blue possesses toxic properties, and the following few facts regarding the results obtained by its use may be of interest; a fuller statement on these points will be published later.

The drug was chiefly used in the treatment of malaria and flagellate infections of the intestine. In

acute malarial attacks it was found that though in many cases favourable results were obtained, the efficacy of methylene-blue in cutting short attacks was distinctly inferior to quinine. On the other hand, it frequently proved a valuable remedy in preventing relapses, especially in cases where quinine, administered by the mouth or by intramuscular and intravenous injections, had signally failed.

The results obtained in flagellate infections were fairly good, though often unsatisfactory.

Regarding "toxicity," although some of the patients received over 400 gr. in a course of treatment extending over several weeks, beyond occasional complaints of slight bladder irritation no troublesome symptoms were manifested which could be attributed to the use of the drug. It may therefore be accepted that in methylene-blue we possess a remedy of great value; and that, provided a pure supply is obtainable, it may be freely used without fear of "toxic" symptoms resulting.

I am, Sir, yours faithfully,

Edinburgh, June 10th, 1920.

D. G. MARSHALL.

THE SURGICAL TREATMENT OF PROLAPSE OF THE UTERUS AND VAGINA.

To the Editor of THE LANCET.

SIR,—I do not think it is my business to attempt to disperse the fog that enshrouds the thoughts and impressions of Mr. R. H. Paramore. That he lacks lucidity and is, therefore, unsuited to teach was amply manifest in his first letter. I ask you to imagine, Sir, a medical student endeavouring to understand and assimilate what, no doubt, Mr. Paramore considered words of wisdom. Several intelligent practitioners have assured me that they obtained no glimmering of light after reading his letter through more than once. I have never before myself been accused of nebulosity, and on the present occasion Mr. Paramore would have been well advised if he had avoided the cheap form of argument that is abhorred by every school-boy. However, my only concern in taking notice of his first letter was to prevent those who might read his remarks imagining that the statements concerning myself were correct, for, as Mr. Paramore says, criticism in THE LANCET receives wide publicity.

It is necessary for me once more to controvert the intentional inaccuracies of Mr. Paramore, who, in spite of being exposed for similar inaccuracies in his first letter, in your issue of June 12th, not only attributes to me more views and statements I have never held or made, but also makes two definite aspersions: one is that I do not know the meaning of the word "floor": the second implies that I do not understand the significance of the adjective "congenital." I am afraid I must ignore any further puerile insinuations of this sort until I have some evidence that Mr. Paramore has taken the trouble to read my book, "The Principles of Gynaecology," third edition, if the abstract of my lecture in THE LANCET, which should be read as stated in conjunction with this work, does not make everything clear to him. In my book he will discover the term "congenital" qualified on p. 212 and elsewhere, as it was qualified in my lecture, and he will find the pelvic floor discussed on p. 47, and in my lecture I described, for want of a better name, as I stated definitely at the time, an operation for "the reconstruction of the posterior segment of the peritoneal aspect of the pelvic floor"—indeed, I asked for a better description of the procedure.

We must have lucidity and honesty, and until we get them there will be much diverse teaching in regard to the surgical treatment of prolapse, which, after all, was the subject of my lecture. Had Mr. Paramore heard some of the remarks I have heard from doctors and patients regarding so-called cures for prolapse he would have realised that facts and figures are important. (Honesty may be assumed until the contrary has been proved, in accordance with British traditions of justice.) Moreover, good operations are the result of good reasoning, whatever may be Mr. Paramore's subsequent interpretations of them, and technique is of considerable importance. In my lecture I was speaking to a large