

pulmonary blood pressure is regulated by variations in the systemic circulation?—I am, Sir, yours faithfully,

STEPHEN G. LONGWORTH.

District Asylum, Melton, Suffolk, Feb. 13th, 1909.

THE PHYSIOLOGY AND PATHOLOGY OF WORK IN COMPRESSED AIR.

To the Editor of THE LANCET.

SIR,—In THE LANCET of Jan. 30th, p. 297, Sir Thomas Oliver published an address on the Physiology and Pathology of Work in Compressed Air, in which he described, without acknowledgment, certain experiments of mine as if he had performed them himself with the assistance of Dr. Alfred Parkin. The experiments I refer to in particular are some which I communicated to the Royal Society nine years ago. I constructed a small chamber with thick glass windows, placed a curarised frog inside and spread the web of the feet over one window, illuminated it with the arc light, and observed the capillary circulation when the pressure was raised or lowered several atmospheres. By this means I was able to disprove the often-repeated theory that increased air pressure influenced the circulation mechanically and show that bubbles of gas appeared in the vessels on decompression and blocked the circulation and disappeared on recompression. This experiment I have repeatedly demonstrated, projecting the web on the screen in my laboratory, and in 1906 I showed it at a lecture I gave at the Royal Institution. To Dr. Parkin of Newcastle, who was publishing a thesis on some cases of compressed air illness, I gave, at the request of a colleague, particulars of this experiment and where he could have a similar chamber made. Dr. Parkin repeated my experiments and in his thesis acknowledged his indebtedness to me. Sir Thomas Oliver has now assumed the authorship of these experiments for the second time. The first occasion was at a lecture which he gave at the Royal Society of Arts, for which he was awarded a gold medal. I took steps privately to inform Sir Thomas Oliver of my opinion on this occasion so that I feel assured he has on this second occasion deliberately repeated the suppression of my name and assumed the credit of these experiments himself. On this occasion Sir Thomas Oliver has also published as if they were original experiments performed by Mr. Greenwood and myself, which prove that relatively high percentages of carbon dioxide do not contribute to the ill-effects of compressed air—a fact previously controverted by Sir T. Oliver and of commercial importance, seeing that the London County Council has wasted enormous sums of money on a useless excess of ventilation in their recent Thames Tunnel caisson works. How little Sir Thomas Oliver knows of the actual experiments of which he assumes authorship, how incapable he is of interpreting correctly the experimental results, is shown by two statements in his address. He says the experiments were done on pithed frogs. This is impossible, as the circulation ceases in the web of the pithed frog. He says he acclimatised dogs to breathing at 50 pounds pressure 1.6 per cent. of carbon monoxide. In making this statement he shows that he is either ignorant of or suppresses the fundamental experiments of Haldane and Lorrain Smith on carbon monoxide, and also shows no knowledge of the law of partial pressures, a law in which he might perfect himself if he spent more time in fulfilling the duties of the chair of physiology which he continues to hold and less in setting forth other men's work as if it were his own.

I am, Sir, yours faithfully,

Loughton, Essex.

LEONARD HILL.

INFLUENZA: AN UNUSUAL SEQUELA.

To the Editor of THE LANCET.

SIR,—During the last month I have been attending a considerable number of cases of influenza and have noticed what to me is an unusual sequela. The cases have all been of the ordinary type, characterised by the usual symptoms of fever, severe headache, pains in the back and limbs, &c., but in none of the three cases I wish to refer to particularly were there any symptoms referable to the liver, gall-bladder, or bile-ducts till the patients were getting convalescent, when each complained of a sickly feeling after medicine or food. In two days after the conjunctiva and skin were stained the deep

yellow colour of jaundice. The stools and urine exhibited the characteristic colours also. The patients had not previously had any liver affection or passing of gall-stones. The cases followed the normal course of a simple catarrh of the bile-duct and are now nearly convalescent. During a considerable experience of influenza since its reappearance in this country 20 years ago I have noticed, in common with others, that each epidemic is followed by a special sequela. In one, gastric symptoms is the leading; in another, chest affections; and in a third, nervous symptoms, &c. Although one recognises the hydra-headed character of the after-effects of influenza, these cases are, in my experience, so unusual that I thought it might be worth while putting them on record.

I am, Sir, yours faithfully,

Huddersfield, Feb. 9th, 1909. DAVID WILSON, M.D. Glasg.

THE EFFECTS OF INJURY BY EXPLOSIVES.

To the Editor of THE LANCET.

SIR,—In a book which has just been reprinted and published by Messrs. Maclaren and Co., entitled "Adventures in the Rifle Brigade," by Captain Sir John Kincaid, I find the following remarkable story at p. 171 in connexion with Kincaid's account of the Battle of Waterloo:—

Two of our men, on the morning of the 19th, lost their lives by a very melancholy accident. They were cutting up a captured ammunition wagon for firewood, when one of their swords striking against a nail sent a spark among the powder. When I looked in the direction of the explosion, I saw the two poor fellows about twenty or thirty feet up in the air. On falling to the ground, though lying on their backs or bellies, some extraordinary effort of nature, caused by the agony of the moment, made them spring from that position, five or six times to the height of eight or ten feet, just as a fish does when thrown on the ground after being newly caught. It was so unlike a scene in real life that it was impossible to witness it without forgetting, for a moment, the horror of their situation.

I ran to the spot along with others, and found that every stitch of clothes had been burnt off, and they were black as ink all over. They were still alive, and told us their names, otherwise we could not have recognised them; and, singular enough, they were able to walk off the ground with a little support, but died shortly after.

Is anything similar to this known in the history of injury by explosives? The convulsive leap to the height of 10 feet must be an unconscious exaggeration, just as truthful people will estimate the amplitude of a wave in the open sea at 70 or 80 feet when it is probably only 25. But that clonic convulsions might occur is, I suppose, likely? The only powder known at the time of Waterloo was, I imagine, ordinary black powder. Can any of your readers throw any light on the matter? I may add that the rest of Kincaid's book is written in the most matter-of-fact manner and free from any exaggeration.—I am, Sir, yours faithfully,

ETONENSIS.

SANITATION IN INDIA.

THE PREVALENCE OF ENTERIC FEVER AMONGST BRITISH TROOPS IN INDIA.

(FROM A SPECIAL CORRESPONDENT.)

VIII.¹—Flies as Carriers of Infection.

THE rôle of insects generally in the spread of tropical diseases should of itself urge closer attention to the possibility of the infection of enteric fever being carried by flies, and preventive measures should not fail to include some attempt to reduce these pests in Indian cantonments. Experimental proof that house and other flies common to dwelling-houses, barracks, and places where human excrement is deposited play a part in the spread of enteric fever is not wanting. Celli² in 1888 was the first to show that the bacillus typhosus was capable of being transmitted in a virulent form by flies in their excrement, and Ficker,³ after experimentally feeding flies on typhoid cultures, recovered the bacilli in a living state from their intestines, feet, head, and wings. Firth and Horrocks,⁴ experimenting at the Army Medical School, Netley, in 1902 also found that flies were capable of carrying enteric infection from

¹ Nos. I., II., III., IV., V., VI., and VII. were published in THE LANCET of Jan. 2nd (p. 62), 9th (p. 134), 16th (p. 197), 23rd (p. 277), and 30th (p. 353), and Feb. 6th (p. 427) and 13th, 1909 (p. 501), respectively.

² Johns Hopkins Hospital Reports, vol. vii., pp. 30, 31.

³ Archiv für Hygiene, Band xlvi., pp. 247-282.

⁴ Brit. Med. Jour., vol. ii., 1902, p. 936.