

may be noted that in republishing these lectures in the form of a work called "Practical Lithotomy and Lithotripsy," at page 179, Sir H. Thompson has altered the foregoing sentence into: "Having tried both methods myself, I have no hesitation in preferring the former." And the same disingenuousness in not mentioning Mr. Coulson's name in connexion with the operation has been observed till the present time; hence my reason for addressing you.

Allow me, in conclusion, to state that I emphatically deny that Sir H. Thompson's operation differs in any essential respect, either as regards manipulations or success, from that now performed by most hospital surgeons.

I am, Sir, your obedient servant,

August 29th, 1868.

BUXTON SHILLITOE.

* * We can insert no more letters on this subject.—ED. L.

THE HOLOCAUST AT ABERGELE.

To the Editor of THE LANCET.

SIR,—I have just had an opportunity of reading the interesting and valuable remarks contained in THE LANCET of August 29th on the immediate cause of death in the cases of the ill-fated thirty-three. I wish to express my concurrence in your opinion that death was due to a twofold cause—(1) the shock of a suddenly and intensely elevated temperature; and (2) asphyxia from the inhalation of the products of the more or less complete combustion of the hydrocarbon, paraffin.

I was inclined at first to the belief that the paraffin, vaporised by the heat as well as by currents of air, had acted rapidly as an anæsthetic. I had previously obtained evidence that this substance possessed some power of this kind, for in the course of my investigations on anæsthetics I persistently inhaled paraffin vapour to the dizziness of (as it seemed) commencing narcotism. I endeavoured to test the anæsthetic power of paraffin in the case of animals. Mr. John Lees, of Upper-street, assisted me in making the observations.

1. On August 29th I placed a healthy guinea-pig beneath a bell-glass of 1600 cubic inches capacity, and forced into the glass an atmosphere impregnated, at the ordinary temperature, with paraffin. No sign of anæsthesia resulted.

2. I repeated the experiment, facilitating the vaporisation of the paraffin by placing the vessel containing it in hot water. By all our methods, though the bell-glass became filled with a highly-charged atmosphere, it was impossible to induce a sign of anæsthesia.

I was hence compelled to conclude that paraffin is practically no true anæsthetic. Consider that the symptoms produced upon myself by the agent were alone due to its nauseating qualities.

3. I thought it well, however, to try the effect of burning paraffin. Accordingly, I placed a rat in an iron cage, and surrounded it by a circle of lint soaked in paraffin afterwards ignited. I hoped that the animal would be sufficiently far from the heat to be uninjured by it; but though it evinced no sign whatever of any impairment of function, its feet suffered from the heat communicated to the metal. It was immediately chloroformed and killed.

From this experiment I should conclude that the asphyxiating elements at Abergele, where the combustible fluid was in great excess, were contained in the black smoke—in the products of the incomplete rather than in those of the complete combustion.

Whether shock or asphyxia, or both united, were the immediate cause of death, there can be little doubt that the victims were spared those horrible tortures with which death from fire is usually associated.

I am, Sir, your obedient servant,

City-road, Sept. 1868.

A. ERNEST SANSOM, M.D.

TREATMENT OF HÆMORRHOIDAL AFFECTIONS.

To the Editor of THE LANCET.

SIR,—In THE LANCET "Mirror" of Sept. 5th there are recorded some very interesting cases of hæmorrhoidal affections treated by Mr. Henry Smith, of King's College Hospital. As I have had some experience in the employment of the clamp since I first had the opportunity of witnessing Mr.

H. Smith's operation in the theatre of the hospital to which he is attached, I shall be obliged by your giving insertion to this communication on the subject.

I have operated upon thirty-three cases by means of the clamps; some of these were of a formidable character, and in all the cases obtained the most complete success.

In one case, that of a Colonel B., who, on his return from India, broken down in health, anæmic, emaciated to an extent I have never before witnessed from repeated loss of blood, I found a large pendulous hæmorrhoidal excrescence, very much resembling in shape a moderate-sized bunch of grapes, with a large exposed surface of intestinal mucous membrane. Having placed the patient under the influence of chloroform I removed the whole of the mass, and four tolerably large portions of mucous membrane, by means of three clamps, without the loss of more than an ounce of blood. The patient was confined to bed for five days only, never having experienced a single symptom calculated to interfere with his rapid recovery. At the expiration of the time I have mentioned the gentleman was able to take carriage exercise; he made a rapid convalescence, and he is now in the enjoyment of perfect health.

The clamp modified and perfected by Mr. H. Smith is admirably adapted for the purposes for which it is advocated, and I feel persuaded that any surgeon who will take the trouble to employ the clamp method of removing piles will find that it possesses great advantages over the barbarous practice of the ligature operation. The result of my experience testifies to the total absence of secondary complications in the former, and to very frequent retention of urine and a return of the disease after operative procedure in the latter.

The toy-like ivory appendage to Mr. H. Smith's instrument I regard as unnecessary, and only calculated to increase the already extravagant charges of the instrument maker.

I am, Sir, your obedient servant,

Southampton, Sept. 1868.

JOHN WIBLIN.

THE DIRECT REPRESENTATION OF THE MEDICAL PRACTITIONERS IN THE MEDICAL COUNCIL.

To the Editor of THE LANCET.

SIR,—Dr. Andrew Wood's remarks on the speech I recently delivered at Oxford, have convinced me that I unintentionally erred in saying that *all* the representatives of the medical corporations in the Medical Council were against the profession on the vital question of direct representation. The fact is, I had been so deeply impressed by your report (THE LANCET, July 11, p. 46, *et seq.*) of Dr. Wood's statesmanlike speech in the Council on behalf of the profession, that I instinctively assumed the orator to be a University delegate. As a member of the Royal College of Surgeons of England myself, I did not suspect, until expressly told, that a speech so liberal and able as Dr. Wood's could be delivered by the representative of the Council of a British College of Surgeons.

When I said that medical corporations who oppose the profession must be "subjugated," I did not mean destroyed, as Dr. Andrew Wood assumes, but reduced to obedience to the will of the profession for its own advancement and for the public good.

Touching the British Medical Association, I accept Dr. Wood's position. It stands in need of reform quite as much as the medical corporations. As a governing body, the Council of the Association is a myth, while its Committee of Council is practically an irresponsible oligarchy, constituted and ruling in direct violation of the very first principles of representative government.

Confidently relying on your support as one of the foremost champions of medical reform, I remain, Sir,

Your obedient servant,

Birmingham, Sept. 6th, 1868.

SAMPSON GAMGEE.

HYSTERICAL VOMITING.

To the Editor of THE LANCET.

SIR,—The correspondence which you have published on this subject shows that the above term has been erroneously applied to some cases in which uterine irritation did not exist, and that specific source of irritation not to have been discovered in some others in which the term would have been appropriate.