

TAPPING IN HYDROCEPHALUS.

To the Editor of THE LANCET.

SIR,—The paper published in your valuable periodical of the 27th instant, entitled "Hydrocephalus unsuccessfully treated by Tapping," is written in a very candid manner; but the deductions, I think, are not fair inferences from the premises, because the performance of the operation on which ultimate success so much depends, certainly was not so accurately attended to as the case required. The puncture was made with a common lancet, a little to the left of the great longitudinal sinus, and after a portion of fluid was evacuated, the lips of the wound were found to offer an impediment to the free exit of the fluid. The wound healed, and the lancet was again resorted to, not more than a few ounces of water coming away. In the course of a week the child died, and we are then told that a lancet is by no means a good instrument for performing the operation, as the least motion on the part of the child, may disturb the lips of the wound, and entangle the edges of the dura mater in the outer opening, or divert them from the exact line of communication! And although the operation was performed with this improper instrument, we are to infer, that even the most temporary relief is not to be expected, in the majority of cases, from its performance. Now, sir, from the success which I have witnessed in cases where the operation has been performed, not with a lancet, but with a small trocar, when the quantity of fluid to be abstracted could be regulated according to the discretion of the operator, and not according to that of the dura mater (never being contented with removing a few ounces only, when it was essential that more should be evacuated), it does appear to me that we are not justified in decrying an important operation, which does at all events remove an effect, when perhaps the cause may have ceased to exist; or at all events affords the practitioner a better opportunity of administering his remedies, the unnatural pressure on the brain being removed. I trust ere long to have an opportunity of detailing some practical proofs that tapping in hydrocephalus is not altogether useless. At all events, let me request that you, sir, and the medical public will, for a short time, suspend your judgment on this operation.

I am, Sir, yours respectfully,
M.

London, June 29, 1835.

CANCNUM ORIS.

To the Editor of THE LANCET.

SIR,—Although a stranger to both parties, I have lately observed with regret the controversy carried on between Mr. Hoare and Mr. Vicary. I use the term "regret" because great pains have been taken to show that the child died from the effect of mercurial salivation; and it has been as strenuously asserted that the mercury taken was not likely to produce fatal ptyalism.

The fracas strongly reminds me of the two Knights who fought about a Shield. If the disease was not caused by the action of mercury, what then was its real character? This question (and it is an interesting one) seems to have been overlooked. I am well aware that such diseases are generally considered by our metropolitan hospital functionaries, as the consequence of mercurial action occurring under certain cachectic states of the system; but our metropolitan hospitals have long since ceased to be the oracles of the profession, and, like Friar Bacon's Brazen Head, they can now only sorrowfully ejaculate "The time's past."

My attention was first drawn to this destructive inflammation of the mucous membrane of the mouth and fauces, by Dr. Whiting of the *Surrey Dispensary*, a gentleman of very comprehensive mind, and I have seen it occur in various states where mercury has and where it has not been administered; it is termed "Cancrum Oris." In a work published some few years ago by Dr. Hamilton, (of Lynn Regis?) one of the best practical physicians of this or any other country, the doctor ably describes the disease, with its appropriate treatment, and to this book I beg most respectfully to refer Mr. Hoare and Mr. Vicary, for the purpose of amicably settling their quarrel. Its perusal, indeed, would benefit the profession generally, the disease being little known and still less understood. I am, sir, your humble servant,

J. TAYLOR.

Old Kent-road, June 29, 1835.

CONCENTRATED INFUSIONS.

To the Editor of THE LANCET.

SIR,—A former communication of mine was published by you under the signature "S. G." in answer to a paper in *THE LANCET* of April 18th, signed "J. T." In speaking of "Bass's Concentrated Infusion of Senna." On that occasion, I did

not mean that its purgative effects were inoperative, but only that a given quantity of the Infusum Sennæ P. L. will act with more energy than the concentrated infusion (after the separation of a portion of its cathartine) diluted in the usual manner. My communication appears to your correspondent "very erroneous, and so far from just," that he firmly believes that the Inf. Sennæ is the only one good for anything at all; after which he states that he has "cause to know that almost the whole of the bitter concentrated infusions, as usually prepared, have their flavour given them by the bitter principle extracted from "Quassia and termed by Dr. Thomson Quassin" (alias Quassine). Will your correspondent inform us from whence he obtained his information, and should he favour us with a rejoinder, confine his observations to Mr. Bass's preparations, which were exclusively mentioned in the paper referred to; for although there are others, of their comparative merits I plead ignorance. Lest it should be thought that I have some interest in preferring the infusions of Mr. Bass, I beg leave to state that we are perfect strangers, and I hope that the serious charges brought against the said concentrated infusions will meet his eye and be satisfactorily refuted, or that he will deny the *ipse dixit* of the pharmaceutical assistant of the St. Marylebone Infirmary. In regard to the infusions of orange-peel and gentian, the only bitter infusions referred to by this latter gentleman, it would perhaps be a fair test of his delicate taste, if blindfolded he could point out which of the two kinds is the "less grateful and pungent." As to what is said of *Decoctions* &c., I am at a loss to understand what they have to do with *Infusions* in the strict sense of the term. I am, sir, your obedient servant,

W. S. G. DAVIES.
Surgeon, R. N.

Brighton, June 27, 1835.

LETTER FROM DR. CLANNY.

To the Editor of THE LANCET.

SIR,—Since my arrival in London I have seen, for the first time, Dr. Stevens's work on the Blood, and I cannot avoid expressing openly my astonishment at the freedom, to use a mild expression, of his comments upon my chemical investigations of that important fluid.

In prosecuting my laborious experiments upon the blood, I had no theories to serve, founded upon certain impressions in re-

spect to its change of colour, whether by agency of salt and water or carbonic acid gas, well knowing that such experiments are anything but chemical analyses. Being in London (to attend a Committee of the House of Commons on the subject of accidents in coal-mines), after an absence of several years, I respectfully invite any chemist to refute by direct experiments anything that I have published in the pages of *THE LANCET* upon the subject in question, and I will render every assistance in my power, in order that the *opprobrium medicorum* in respect to the exact nature of the animal fluids may be removed. If Dr. Stevens, who I understand resides in London, be inclined to make direct chemical analyses of the blood by which any of his theories may be supported, *which heretofore has not been the case*, I promise that I will attend and witness them, and be the first to acknowledge their value—if any value they may have.

It is a pity that such philosophers as Doctors Prout and Turner do not perform some experiments upon the blood, and discover the truth, whether it lie in the bottom of a porringer or in the "bottom of a well."

I do not intend to reply to any comments upon this communication except such comments are supported by direct and correct chemical experiments. I remain, Mr. Editor, your most obedient servant,

W. REID CLANNY.
Colonnade Hotel, Charles-st., St. James's,
June 29, 1835.

NORTH LONDON HOSPITAL.

ACNE ROSACEA, SUCCESSFULLY TREATED WITH KREOSOTE.—EFFECT OF KREOSOTE ON VOMITING.

ANNE DRAPER, aged 33, was admitted; Nov. 8, 1834, under the care of Dr. ELIOTSON. For seven years she has been subject to a red eruption of the face, presenting the following characters:—It commences with a sense of heat, and is shortly afterwards followed by the appearance of a small pimple, which enlarges, and becomes gradually yellowish, and when it breaks, discharges a yellow matter; it then dries up, no scab, but a little scurf remaining. She has been subject to headaches from the first appearance of the eruption. She complains, too, now, of a pain in the forehead, with a sense of tightness, which she also experiences under the eyes; she is particularly nervous, very thirsty in the morning, and subject to acid eructations. As there is a slight