interrupted respiration, but to a looker-on appeared to threaten life.

Now, Sir, I will make a few very brief remarks on the cases in which tracheotomy, when *timously* and judiciously resorted to, holds out reasonable hopes of utility. First, where foreign bodies are lodged in the larynx or upper portion of the trachea; secondly, in œdema of the glottis consequent on inflammation of the larynx; thirdly, in croup, where the inflammation has not extended to a serious degree into the bronchial ramifications. In the last case the unsatisfactory results which have attended the operation are, in my opinion, to be attributed to the circumstance of the operation being too long delayed—the result of such delay being that the lungs become engorged to an extent bordering on hepatization or pulmonary apoplexy, and are hence incapable, even when the mechanical obstruction is overcome, of resuming their function. It is hardly necessary to say, that this is the effect of continued interruption to the transit of the blood through these organs.

In conclusion, I would ask your readers, what appearance the corpse of one dying of laryngismus or suffocation presents? What is the state of the venous system—especially the sinuses of the brain—and also the condition of the heart? It only remains for me to re-assert, that in hydrophobia the patient does *not die suffocated*, which in laryngismus *he does*. That the only derangement of the respiratory function observable is *temporary* as to duration and purely *symptomatic*—*ergo*, that the operation recommended on such deservedly high authority is no more to be relied on in this most dreadful malady than any other means hitherto so vainly employed.

I am, Sir, your obedient servant,
Newcastle-on-Tyne, July, 1853. GEORGE FIFE, M.D.

ST. MARY'S HOSPITAL.

CHOLERA.

To the Editor of THE LANCET.

SIR,—I rely on your candour to give me space for a few remarks on the report of a cholera case at St. Mary's Hospital, which appeared in THE LANCET of July 16th. It is the introductory remarks to which I allude more especially.

The author admits that according to some authors the presence or absence of bile in the intestinal secretions forms the actual and almost only distinction between Asiatic and English cholera. He admits that cramps, vomiting, coldness and blueness of skin occur under the latter denomination, and that the registrar's reports (which, be it remembered, are of deaths only) show this English form to have been pretty prevalent of late. At the same time he is at great pains to assure us that an epidemic of this "pretty prevalent" disease is not existing at the present time. He raises the old etymological objection to the Asiatic disease being called cholera, and states the English pest, with its bilious dejecta, to be quite a different thing from the epidemic, spasmodic, or *algide* variety; although he owns it to be pretty prevalent (that is, *epidemic*, one would have thought) with cramps, (certainly *spasmodic* then,) besides coldness (that is an *algide* state) and also blueness of skin.

That "indiscriminate apprehension" should reign among the public if they see, and see *through*, such specious arguments will, I think, be natural enough. If they are to die of a disease which has diarrhoea, vomiting, cramps, and blueness, among its symptoms, and a very short course to its fatal end, what on earth can it signify to the expectant sufferers whether their motions will be with or without bile?

Instead of denying the identity between two diseases which cannot be distinguished from one another by any essential difference, let us rather admit, without quibbling, the presence of a formidable epidemic, happily as yet less fatal and less general than we so lately had it, but still at any time liable and likely to break out with its former fury, when, in addition to atmospheric viscidities, which we cannot control, those causes of pestilence which are as it were of our own making, spring up again in their former rankness.

Not by special pleading (however ingenious) as to its nomenclature and minor diagnostics is this destroyer to be stayed, but by looking it, as an enemy, in the face, and by not only attacking it with early and vigorous treatment, but cutting off from it those supplies of bad air, bad water, and bad food, without which its malignity is powerless—is, indeed, annihilated.

I have the honour to be, Sir, your obedient servant,
Knightsbridge, July, 1853. F. A. B. BONNEY.

PROJECTED AUTHORS' PUBLICATION ASSOCIATION.

To the Editor of THE LANCET.

SIR,—May I be permitted, through the medium of your pages, to address myself to the profession and to the authors of scientific works in general, in relation to a proposal for the formation of an Authors' Publication Association, for the extension of the sale of their works.

It is a notorious fact that the sale of a book, in common with every other commodity, is in proportion to the cheapness of its cost. This fact has been very strikingly exemplified by the sale of a small work I published two years ago. This work was issued from the press at 2s. a copy, and an edition of 500 copies was sold in about seven months, at a certain expense of advertisements. The expenses of the work having exceeded the estimate for printing and getting it up, on the issue of a second edition, the sale price was augmented to 2s. 6d., when, to my mortification, with the same amount of advertisements, the sale fell off nearly one-half. The consequence necessarily was, that instead of gaining anything by the additional 6d. paid by the purchaser, I sustained an augmented loss, and the circulation of my work was materially impeded also.

With these facts before us, and seeing that the publisher's charges on the sale of a book exceed 40 per cent. on the author's receipts and sale price, exclusive of the profits accruing to him on the printing, paper, binding, &c., in getting up the work, it is proposed to form an author's publication establishment, by which at least 30 per cent. of the charges now paid by authors may be saved to them, and the sale of their works, by this reduction in its price to the public, may be as proportionately increased.

In furtherance of these intentions, if those gentlemen who are favourable to the objects proposed, will communicate with me, we will convene a meeting for the purpose of considering the subject, and determining upon the plan to be pursued for carrying out the same. I will be prepared to submit to the meeting very simple and efficient means for the organization and carrying out all the requirements of such an association, the outlines of which may be communicated in a few words: it consists in establishing, in the Strand or other locality, an authors' dépôt for the publication and sale of works at a commission price, which shall do no more than pay the expenses of the establishment; and further, if approved, for printing, binding, and getting up of the work, the profits of which alone there is little doubt would pay all the expenses of the establishment; and seeing that books may be now sent at a small charge by post, not only to any place in Great Britain, but to the colonies also, the present may be considered a very favourable opportunity for carrying out these views, and placing authors in a much better position in relation to their works than has hitherto existed—emancipating them, indeed, from the trammels of an intolerable monopoly.

I am, Sir, yours obediently,
14A, Weymouth-street, Portland-
place, June, 1853. C. SEARLE, M.D.

ON THE PHYSIOLOGICAL USES OF THE GANGLIONIC NERVOUS SYSTEM.

To the Editor of THE LANCET.

SIR,—Having perused your report of Dr. Davey's paper, on the Physiological Uses of the Ganglionic Nervous System, read before the Medical Society of London, and Mr. T. L. Walford's letter, claiming the merit of the doctrines advocated for his old preceptor, Dr. Fletcher, I feel mightily disposed to trouble you with a remark or two on the said merit, with a view to determine its amount.

The view proposed is, that the ganglionic is an independent nervous system; and not only so, but that the integrity of the whole organism, as well as all the organic functions, is dependent upon it. The first part I pass over, and deal only with the second, which states either that all the organic functions are absolutely dependent on the organic system, or else so controlled and governed by it as to be incapable otherwise of maintaining the bodily integrity: it is not quite clear which is meant, but both seem to be wrong—certainly the first.

And why do they seem to be wrong? For a very simple reason—because we see the organic functions performed perfectly well, and without any defect, in creatures which have no vestige of a nervous system at all—for instance, in plants and cellular animals. Here absorption, assimilation, growth, excretion, &c., all occur where there are no nerves at all, and seem to be inherent powers of the living structures. Is it, therefore, reason-