

PRODUCTS OF COMBUSTION.
ACTION OF CARBONIC ACID.

To the Editor of THE LANCET.

SIR:—Having read your brief notice of Dr. Golding Bird's paper "On the Action of Carbonic Acid," and vainly expected a report of some discussion upon the subject in your last week's Number, I cannot but express a selfish regret that public societies, embodying men of first-rate attainments, capable of effecting immense good, if energetically devoted to single objects, are too prone to suffer important topics to escape that rigid investigation which is so essential to the accumulation of established facts and indisputable "constants." It is sad to the independent cultivator of science, exempt from every species of bigotry, and ardent in the search for truth, to witness members of various societies rivalling the industrious bees in flying rapidly from flower to flower, but neglecting to exhaust to the uttermost each flower of its sweets, and to store the rich treasure of honey for their mutual benefit,—*"Sic vos non vobis mellificatis apes!"*

With this prelude may I again refer your readers to a subject which, although discussed again and again, is still unsettled; and, although teased, torn, and worried, still remains unexhausted,—I mean to the effects of mixtures of carbonic acid gas, oxygen, and nitrogen, and to those produced by the combustion of charcoal.

As it has been very satisfactorily demonstrated by Messrs. Allen and Pepys (*vide* "Phil. Transactions of the Royal Society," for the year 1809), that a human being cannot exist in an atmosphere containing 10 per cent. of carbonic acid gas, which atmosphere will be constituted as follows, viz. :—

	Cubic feet.						
Carbonic acid gas10	}	or	}	or		
Oxygen10					15.65	31.3
Nitrogen80					15.65	31.3
	100		156.5		313.		

And as it is also known that every pound of pure charcoal will, during its combustion, abstract the oxygen from 156.5 cubic feet of atmospheric air, and leave—

	Cubic feet.
Carbonic acid 31.3
Nitrogen 125.2
	156.5

It follows, that the combustion of a pound of charcoal will render 313 cubic feet of atmospheric air totally destructive to human existence, and composed as follows, viz. :—

	Cubic feet.
Carbonic acid gas 31.3
Oxygen 31.3
Nitrogen 250.4
	313.

Here, then, is one limit for the solution of the first important problem, viz., the effects of simple mixtures of carbonic acid gas, oxygen, and nitrogen, upon human beings, beyond which it must be useless to refer. But as the combination of charcoal evolves many emanations of a compound character, some of which I believe to be too subtle to be detected in any manner but by their invisible agency, another problem will necessarily follow for the elucidation of this latter subject.

It appears, then, that the first problem, viz., in what proportions carbonic acid gas, oxygen, and nitrogen, can be safely inspired for a continuous period of some duration, should be satisfactorily solved, before we can confidently approach the more intricate investigation. The maximum innocuous limits of the most injurious gases, viz., of the carbonic acid, and of the nitrogen, having been ascertained both separately and together, and demonstrated beyond the possibility of doubt, we can, without much difficulty, influence the combustion of charcoal in such a manner that a similarly proportioned mixture of the gases above mentioned shall be effected; and of course no injurious results should ensue unless from some other cause than from the carbonic acid. Thus, the opinion I have so obstinately adduced may be tested, even although the destructive agent in question may evade detection.

If I am not mistaken, Dr. G. Bird still maintains that the evil results which ensue from the slow combustion of charcoal are solely attributable to the carbonic acid gas, which (with the exception of carburetted hydrogen) he states to be the sole product of the combustion. These conclusions I at once dispute, having never yet been able to procure any charcoal, how carefully soever it may have been prepared, that did not yield when burnt in a Joyce's stove,—

1. Aqueous vapour;
2. Nitrogen;
3. Carbonic acid gas;
4. Carbonate of ammonia;
5. Hydrochlorate of ammonia;
6. Sulphate of ammonia;
7. A volatile oil.

And I feel perfectly conscious of the existence of some other product which "eludes the grasp."

The relative proportions of the hydrochlorate of ammonia to the sulphate of ammonia, are as 100 : 17.8, and of the elements chlorine to sulphur, as 100 : 6.47.

I offer Raspail's objections to Liebig's method of analysing organic substances, which I conceive to be quite sufficient to demonstrate inevitable fallacies in all the results. Could not a committee of medical chemists determine a course of experiments on this mysteriously interesting subject, and, abandoning all prejudices, fully and fairly unravel its nature? The sanction of such a powerful authority would be infinitely more satisfactory to your readers and to the public, than the solitary investigations of a rustic amateur. Yours very truly,

CHAS. T. COATHUPE.

Wraxall, near Bristol,
April 1, 1839.

ORIGIN OF HYDROPHOBIA.

To the Editor of THE LANCET.

SIR:—I have just purchased Dr. Klein Grant's new and improved edition of Hooper's "Medical Dictionary," and having naturally turned to the article "Hydrophobia," I, with some little surprise, read as follows:—"Of the cause of this peculiar distemper in dogs, nothing certain is known; that it originates *spontaneously* in them is now the general opinion."

Permit me to ask this gentleman on what ground he rests this assertion? Does he refer to veterinary practitioners, who may be supposed to have the fairest chance of arriving at the truth respecting their own patients? Nineteen out of twenty will tell him, that "the cause of this peculiar distemper in dogs is known; that it never originates *spontaneously* in the dog; but is propagated from one to another by the bite." I am aware that there are a few who maintain a contrary opinion, but I am speaking of the overwhelming majority of practitioners.

Does he refer to the practitioners of human medicine? I must, in this case, remind him that in the year 1831, a Committee of the House of Commons inquired into the subject of rabies. To six of the medical gentlemen who were examined, this very question was put:—"Do you believe that rabies originates spontaneously in the dog?" "No," was the unhesitating reply of three of them,—Drs. Babington and A. T. Thomson, and Mr. Earle. Mr. Morgan stated that it was the preponderance of his opinion that rabies was a disease to be communicated solely by the bite of another animal previously rabid." Sir Benjamin Brodie said, that "he had not been able to make up his mind, whether the disease does or does not, ever arise spontaneously." Mr. Frankum alone professed his belief in its spontaneous origin.

It seems that it was not then, "the general opinion" that rabies "originates spon-

taneously" in the dog. What has caused the sudden change of opinion in medical men; or has any change really taken place? Where, and by whom, has it been expressed?

Against some previous sentences in Dr. Grant's account of hydrophobia I must respectfully enter my protest. This disease has been communicated by herbivorous and omnivorous animals; and, if the account of Magendie and Breschet is to be depended upon, by the human being. This subject is somewhat fully discussed in the "Veterinarian" for June 1838. I must confess it to be my firm opinion, that every animal, capable of being infected by the rabid virus, can propagate the disease. Yours obediently,

W. YOUATT, V.S.

Adam's-terrace, Camden-town,
April 1, 1839.

PRESERVATION OF DEAD BODIES.

To the Editor of THE LANCET.

SIR:—A year or two ago various notices appeared in several of our British Journals, both literary and medical, of the process discovered by Signor Girolamo Legato, a Florentine gentleman, for preserving, in an extraordinary degree of perfection, the human body, and all objects of natural history. By some the whole discovery was treated as a fable; by others, who had seen the preparations, accounts so extraordinary were promulgated, that the ever-cautious scientific bodies of this country received them with distrust. This incredulity was greatly strengthened by a pamphlet, written about 1835, by the Advocate Pellegrini, a friend of Legato's, and of which several copies found their way into Britain. Nothing could have been more calculated to injure the discoverer than this publication, couched in the most high-flown language, and containing statements often directly at variance with the laws of science. I have not, at present, the opportunity of consulting the various accounts above alluded to, nor is it necessary to do so, as I here wish to relate only the results of my own observation of the specimens, without trusting to the accounts of others. On visiting Florence, in May, 1838, I immediately inquired for the cabinet of Legato, he himself having died nearly two years before. It was with considerable difficulty that I obtained permission, through the kindness of Professor Betti, to inspect the preparations, as they are not now shown to every visitor. Signor Fumigalli, in whose house they are at present preserved, allowed me to handle and examine each specimen at my leisure, and most courteously assisted me with all the information in his power. The following is