Communications.

Mr. Edison on the Microphone.

To the Editor of the Scientific American:

In reply to the communication of Messrs. Pitt and Dopp, which appeared in your issue of June 29th, under the heading of "The Microphone," I wish to say that had the above named gentlemen read carefully what I have said in regard to the variation in the electric conductivity of carbon and other semi-conductors when subjected to pressure, they would have saved themselves the trouble of writing you. I stated, and proved, nearly two years ago, that conductors of electricity when finely divided and moulded in the form of buttons varied their resistance by pressure, and subsequently that the whole effect was due to surface contact, and not to inter-molecular action. Mr. M. Richards, of the Colt s Arms Co., also came to the same conclusion over a year ago. The explanation offered by Professor Hughes, which your correspondents referred to, is capable of being shown as absurd, and only tends to prove that he did not gain his information by experimental research, but simply by piracy.

T. A. EDISON.

Menlo Park, N. J., June 24, 1878.

Driving Piles in Sand.

To the Editor of the Scientific American:

Your correspondent states in your issue of the 22d of June that he drove a large number of piles through sand in Pensacola, and intimates that the failure of others was due to the puny attempts with too light hammers. When the navy yard, Brooklyn, was constructed by Mr. W. McAlpine and many thousand piles driven, there were used a steam hammer giving rapid short strokes, and hammers weighing two tons and, I believe, two and a half. Very frequently a pile could not be driven beyond a certain depth, but if after some hours' rest the pile driving was again renewed it could generally be driven several feet further. The impact of the pile had pressed out the water from the sand at the foot of the pile, the angular particles of sand interlocked and formed a series of arches that effectually resisted the blow on the pile until sufficient time had been allowed for the water to percolate in and loosen the aggregated particles. Sometimes a pile after being driven would come spontaneously clean out of the ground.

The jet of water to put down piles was first used, I believe, by me in 1852, in making the foundation of a lighthouse in water in Pungateague Bay, in the Chesapeake, under Major Hartman Bache. The piles were 18 feet long, hollow, 7 inches in diameter, with a trumpet-shaped base flaring out to 3 feet diameter. A 1 inch pipe was passed down through the pile to the sand, and a hand force pump sunk the pile 11 feet in about 2¼ hours. At the commencement the pile would sink through the upper stratum of sand without any external agitation; on reaching the subjacent blue clay it would remain stationary for some time, until the permeated clay would ascend the shaft and overflow at the top. Fourteen piles forming the foundation were sunk in two days. Some years subsequent a patent for this process was taken out by somebody in England. CHARLES PONTEZ.

Omaha, Neb., June 19th, 1878.

Is our Globe Hollow?

To the Editor of the Scientific American:

I see an article on a subject on which I wrote you more than a year ago, in the current number of the SCIENTIFIC AMERICAN, namely, "Is our Globe Hollow?" I would say yes, and here is my reason, given in the article which I sent you May 8, 1877:

"In or about the year 1826, Sir Richard Phillips propounded the theory that what is called gravitation is the result of the annual and diurnal motions of our globe. He says: 'If a progressive motion acted alone on a mass, it would form a train of the rarer parts, and disperse them. If a rotative motion acted alone, it would direct the parts in tangents, and disperse them. Their combination directs the parts to the center, and the two become a force of aggregation, centripetal force, gravity, or weight.'

"Admitting the correctness of this theory, it follows that if the two forces were equal, they would neutralize each other at the center, and our globe would be solid. If they were unequal they would be neutralized some distance from the center, and the globe would be hollow. As the annual motion is much greater than the diurnal, it seems reasonable to suppose that the dispersing force is also greater. If the dispersing force of the annual motion be represented by 12 and the diurnal by 8, a hollow of 8 would be the result. Not having seen a single argument in support of the hollow globe theory, this is sent for the consideration of your readers, as the only theory imaginable.—A. R."

Washington, Texas, June 19, 1878. JOHN ALEXANDER.

The Best Pen Wiper.

To the Editor of the Scientific American:

Take a few sheets of the softest tissue paper you can get, and fold and roll them all together into a bundle about eight inches long. Put an india rubber band around the middle of the roll, and then cut off the tops so as to allow insertion of pen for wiping, making the packet into hour glass shape. The advantages I find are that it cleans the pen better than anything I ever saw or imagined. C. F. S.

THE ETIOLOGY OF ASIATIC CHOLERA.—A NEW THEORY.

BY BELLEROPHON. MADRAS: HIGGINBOTHAM & CO., 1878.

All the way from Madras comes a neat pamphlet bearing the above title. The author, although intrenching himself behind the title page motto, "*Honi soit qui mal y pense*," nevertheless invites criticism; such as we have to bestow may be expressed in a few words.

After carefully clearing Mr. Bellerophon's "theory" from its investiture of very bad orthography, etymology, and syntax, we have at length succeeded in laying bare his idea, and of this our limited space permits us to exhibit the skeleton only.

It seems that Mr. Bellerophon, after a course of personal observations and reflections, extending over a period of four years (although he states that it took him but forty days to write his essay), has been led to believe that visitations of cholera follow in the train of great battles, or in that of a sudden and widespread mortality among cattle. The corpses, having undergone putrefaction, are at length reduced to an ultimate poisonous, pulpy mass, "teeming with infusoria and animalcules in every stage of development, deposited constantly in such numbers that myriads of them may be attached to a single grain of dust." This matter, which then sinks into the underlying soil, he calls a "binomial poison (A + B);" furthermore, for the sake of distinction, he gives it a name, "Necrophagine." The soil saturated with the "necrophagine" having become dry, is afterwards wafted on the wings of the wind to the uttermost parts of the earth and falls almost anywhere, totally regardless of consequences. Should part of it, however, get wet during its flight, then a remarkable phenomenon takes place, and its "binomial" nature exhibits itself; for while the dry portions may be producing an epidemic of cholera in one portion of the globe, the damp portion, deprived of its "ichorous" matter, undergoes a change, and the germs of which it is composed develop into a secondary form of existence—entomozooids capable of engendering foot and mouth diseases in cattle that feed on food in the vicinity where it fell.

"At the same time the sceptic (*sic*) portion (that is, the A in the binomial quantity) has been diluted and partially decomposed, and being swept down streams it percolates into wells and reservoirs and causes an epidemic typhoid." A portion of the B in the binomial quantity undergoes a development likewise and produces an abundance of flies, mosquitoes, and other insects from the germs which were deposited contemporaneously with the necrophagine (!). This horrible cholera poison may be taken into the system by means of drinking water, and even "the rinsing of the mouth or of a glass in which wine or any other drink is given, cleansing the teeth with a brush dipped in water containing it, or bathing in that water, may possibly leave a particle on the gums or teeth or lips which by subsequent salivation can be taken into the system." So it becomes "possible for people who have never drunk any water all their lives (!) long to take the poison that is in the water, nevertheless, and die of it." It becomes at once evident, then, that water is a beverage that should be regarded with suspicion, and its use prohibited on sanitary grounds! The author remarks that "the two most inexplicable pathognomic symptoms of cholera are: First, the denudation of epithelium, and second, the flowery (*sic*) or pasty coating found in the duodenum or in the lower parts of the smaller intestines."

The *modus operandi* of the "necrophagine" to produce these results is thus stated: "The binomial poison becoming diluted, the infusorial germs begin, under human heat, to show signs of vitality in the stomach; meantime a portion of the diluted sceptic (*sic*) fluid would by endosmose enter the system, and if it does not cause symptoms similar to those of the disease *cellulitis venenata*, an incipient malaria would be the result. The poison passing lower down the intestinal canal, more of it would enter the circulation, while the animalcules play upon the epithelium, causing a double irritation.

'The action of the ichorous matter would be to separate the serum of the blood from the parts that would afterward coagulate. The serum would be discharged into the stomach and intestines—the lacteals would at the same time discharge a part of the chyle they had taken up, a portion of which in a curdled state would mix with the serum and present a rice-water-like appearance in the evacuations. Then the irritation by the animalcules (which would burrow like a species of Hippa), combined with the efforts of the system to rid itself by abrasion (that is, the stomach and intestines working in the same way inwardly as a man might rub or chafe, or scratch himself outwardly through an itching, by working his arm against his bare body) would result in what has been termed the *denudation* of epithelium" (!).

Such, to be as brief as possible, is the gist of this "New Theory." Criticism on this work is hardly necessary, but a few words of kind advice to the author may not be out of place. We would recommend him, then, before he elaborates the "necrophagine theory" any further, to obtain a few textbooks and make himself familiar with some of the most elementary principles of physiology and natural history, of which he now appears to be ignorant. This done, he may possibly by hard study and close observation give to the scientific world, in the course of time, some ideas on the disputed causes of the Asiatic cholera which shall prove truly worthy of attention. And that he has peculiar facilities for making such observations lies in the fact that he resides in a region where (to use his own words) "cholera hovers perpetually like an incumbent nightmare."

Diagnosis.

That we may form a diagnosis it is essential that we possess a theoretical knowledge of disease. We must find out what the patient suffers from if we want to relieve him by rational well directed treatment. If it were true that every disease has its specific remedy—an exploded notion—it would still be necessary to find out the disease in order to meet it by its appropriate remedy. And if we could not discover the disease, we might still, on the good old empirical plan, make a shot at it by firing into it a volley of remedies, counting that perchance one among them would hit the doubtful mark. "Every bullet has its billet" should be the maxim of the empirical practitioner. Since, however, we are not always able to realize the first rational indication to detect the disease, we may for a time fall back upon the plan of observing how the whole system labors, and how any particular function is in difficulty. This will furnish a provisional indication in treatment. Medication on this principle is usually safe, and ought to be safe. Two or three rules of practice will carry you a long way. Thus, when in doubt, give salines. There is hardly any disease in which salines will not do good at the beginning. There is hardly any disease in which they will do harm. By giving salines you gain time for observation, for finding out the more precise indications for treatment. The next rule—it ought perhaps to be the first—is, enjoin rest. The Pharmacopœia contains no remedy of so much value, of such universal application. In addition to its other advantages, it has the merit of giving time for leisurely observation. The third rule might be to relieve any organ suffering from difficulty in the performance of its function. But this rule requires to be followed with great discretion. For example, it is not always wise to purge because the bowels are not relieved. On the contrary, opium may be indicated, as in intussusception. And you may often greatly relieve one disabled organ by inducing other organs to do at least a portion of its work. If you observe these three precepts, you will fulfill the fourth great maxim—the maxim, great in its positive good because great in its negation of harm, laid down by Hippocrates. If you do not see your way clearly to do your patient good, take care at least that you do him no harm.

Your first interview with your patient is your opportunity. A mistake made at this critical moment may damage yourself as well as him; and he may give you no opportunity of retrieval. Later on you may make a mistake, and the consequences, to yourself at least, may be less difficult to get over. Take care, then, of your first step. Start quietly; proceed warily. Do not put your faith in intuition. Distrust those who "see through a disease at a glance." They are shallow people, and are easily seen through themselves. The motto of the true physician is "Thorough."

Now we may proceed to diagnosis. Guided by the principle that when a part of the body is diseased the whole suffers, we must examine the condition of the body in its parts and as a whole. This makes it necessary to examine with method. What is the best method? I do not think myself competent to say. But I can point out one which will fairly answer in practice. The history, diathesis, inherited or acquired, and the antecedent diseases stand on the threshold of the inquiry. These disposed of, examine the functions and organs in a certain regular order: (1) Aspect, plumpness, color and state of the skin generally; (2) the circulation, pulse, respiration, and temperature; (3) nutrition, the tongue, appetite, digestion, stomach, intestines, defecation, and bile; (4) the urinary organs, the kidneys and bladder, as to pain, as to retention or other characters, as well as the characters of the urine itself; (5) the nervous system, sleep, motor power, general languor or exaltation, excito-motory system, mental state, delirium, pain, and its seat and kind; (6) in women, the sexual organs, the menstrual function, child bearing, and the secretions.

All these phenomena should be, as far as possible, explored by the aid of manipulation and the appropriate instruments of exploration. It is a dangerous thing to form a subjective diagnosis; it is a dangerous thing to accept your diagnosis from the patient. Until recent times, however, all diagnosis of uterine disease was subjective. The result was hopeless ignorance, causing disastrous errors. And so it was to a great extent in nearly all diseases before the stethoscope, thermometer, sphygmograph, test tube, and microscope came into practical use.

The advantages of pursuing some such method as that which I have just pointed out to you are—1. You are not so likely to overlook what you are in search of. 2. You will not seldom detect complications, that is, associated diseases in addition to that, the most obvious and apparently urgent one, which, as the French say, "*saute aux yeux.*" 3. You avoid the serious mistake of going over the ground two or three times—of beating about the bush. It gives a bad impression to your patient if you ask him the same question two or three times, when he has already answered it. He will be apt to conclude that you are talking at random, and have no clear idea of what you are about.

You may ask, Why have I, who am specially called upon to aid you in studying gynæcology, touched upon all this? Simply because there is, in truth, nothing more special in gynæcology than there is in the study of heart disease, lung disease, or any other disease. All disease must be studied on the same principle and after similar methods. A long process, you will say. But practice enables one to go through much of this long inquiry quickly, and in the course of other inquiries.—*Dr. Robert Barnes.*