Original Communications.

THE THEORY OF CHOLERA COLLAPSE.

By George Johnson, M.D., F.R.C.P.

I had formed a mental resolve that, in future, I would make no reply to criticisms upon my cholera doctrines. Dr. Woakes, however, appears to invite a reply to his very courteous criticism. I accept the invitation, and I will be as brief as possible.

I believe that the cholera-poison, like strychnia, causes cramp of the muscles by being conveyed to them through the blood. I have no reason to suppose that the minute arteries supplying the muscles are contracted. The notion that the poison is cut off from the muscles by spasm of their small arteries, is, I think, purely imaginary. Dr. Woakes asks why, if a blood-poison contracts one set of muscles, it does not contract them all; and why not the heart in particular, through which all the blood in the body passes. Now, we know that most poisons have textural affinities; they affect certain tissues and certain parts of tissues, to the exclusion of others. But we cannot explain this. We cannot tell why the poison of measles mainly affects the skin and the mucous membrane of the eyes and the air-passages, or why that of typhoid fever has a special affinity for the glands of the ileum. We do not even know why the sweat goes off by the skin, and the urine by the kidneys. The argument, that the heart should suffer most from blood-poisoning, because all the blood goes through its cavities, is, I believe, a fallacy. Its liability to be poisoned, so far as regards the mere quantity of blood, would be measured by the amount supplied to its tissue by the coronary arteries, and not by the volume of blood passing through its cavities. But, in fact, the liability, or freedom from liability, depends upon that very real, though indescribable influence, "textural affinity". Dr. Woakes is not correct in saying that I make "the theory of spasm of the pulmonary arteries the main pathological condition of cholera." I suggest this as a probable explanation of the fact that the blood is abruptly stopped at a particular part of the circulation. I am quite aware of the influence which is exerted upon the movement of blood through the capillaries by the interchange of materials between the blood and the tissues. Dr. Woakes argues, that a suspension of these changes, in consequence of an altered condition of blood, is the cause of the imperfect circulation, affecting first the systemic capillaries, and secondarily the pulmonary. Now here his theory is quite inconsistent with indisputable facts. During collapse, we know that the pulmonary arteries and the venous trunks are full, while the systemic arteries are nearly empty. If there were a primary obstruction in the systemic capillaries, or in the minute systemic arteries, the trunks of the systemic arteries would be as full as the pulmonary arteries are actually found to be. The systemic circulation is diminished in proportion to the smallness of the stream of blood which passes through the lungs, just as it would be diminished by compression of the aorta. There is no evidence of impediment in the systemic capillaries during collapse: there is positive evidence to the contrary, in the fact that the systemic arteries are so empty that the radial pulse is often not to be felt. This statement of facts is so indisputably true, that it would be useless to argue the question with any who deny its truth.

With regard to the suppression of bile and urine, I would ask Dr. Woakes to read carefully my paper "On the Physiological Correlation of the Lung, Liver, and Kidneys," published in the Journal for February 17th, 1866. He is not so good a physiologist as I take him to be, if, after reading that paper, he does not admit that there is some truth in this part of my theory. He is quite in error in stating that "no carbonic acid is found in the respired air during collapse." The amount of carbonic acid exhaled is diminished, as is the excretion of bile and urine, exactly in proportion to the arrest of the circulation, and the consequent suspension of the respiratory changes, with all the correlated phenomena.

Dr. Woakes says that rice-water stools occur before collapse. I admit that copious watery pale stools, pale from the copious dilution with fluid, occur before full collapse; but the genuine flocculent rice-water stool, as I should restrict the term—the stool in which bile can be detected only by chemical tests—is always exactly coincident with collapse. I undertake to say, from an inspection of the stools alone, whether the patient who passed them is in full collapse.

He says that, if spasm of the pulmonary arteries were the cause of collapse, collapse should always be sudden. But why so? From all that we know of the contractile power of the arteries, there is reason to believe that their contraction may be gradual or sudden, partial or complete, according to the intensity of the exciting cause. And, if it be asked why the choleraic poison should excite spasm of the pulmonary, and not of the systemic arteries, the reply is, that this is another instance of textural affinity. We know that the pulmonary and the systemic arteries have different vital endowments. The systemic arteries resist the passage of black unaërated blood, which the pulmonary arteries readily transmit, as it is their proper office to do. Again, Blake's experiments prove that certain salts injected into the veins pass freely through one set of arteries, but are arrested by the other set. We, therefore, can have no difficulty in understanding that the poison may excite spasm of the pulmonary, though not of the systemic vessels.

Dr. Woakes, I trust, does not suppose that I consider the intestinal discharges to be a consequence of the arrest of the circulation. I believe them to be a direct result of the action of the cholera-poison, first on the blood, and then on the mucous membrane of the alimentary canal; and I believe this to be as essential a part of the natural process of cure, as the eruption on the skin in a case of small-pox. One of the main reasons why collapse is so deadly is, that the arrest of the circulation impedes in a corresponding degree the eliminative process, so that the retained poison or ferment has more time to spoil the blood.

I believe that I have replied frankly to all Dr. Woakes's objections. I do not think it necessary to offer any further criticism on his theory. I think, however, that my own is more in accordance with the undoubted facts of the disease. He does not appear to have had any experience of the treatment which he recommends. When he has tried the plan, he will, perhaps, tell us the result of macerating his patients in a mixture of tepid water and intestinal discharges. Can he be sure that the cutaneous surface, while admitting the water, will refuse admission to the morbid poison which is thus presented to it, and allowed to remain in contact with it?