

upon actual inspection of the dead body. Before we can form a just estimate of the nature of a disease, which is certainly new to every indigenous English practitioner of the present day, it is necessary to accumulate a large body of facts, to multiply observations, to eliminate all erroneous impressions, and by a careful comparison of symptoms of morbid appearances, and of the effects produced by different therapeutic agents, to afford a foundation on which the future nosologist may erect a safe and lasting superstructure. Such an accumulation of facts can only be made by the combined industry of medical men in the various branches of the profession. Let the practical physician, the physiologist, the chemist, and the morbid anatomist, each in his own department, ascertain facts and collect observations, and we shall then by an accurate survey of the whole, have the means of forming a just estimate of this novel and fatal disease. With this view, I take the liberty of stating one or two facts connected with the pathological anatomy of cholera, which I have observed; they are not perhaps of much importance, but as their accuracy is established by various preparations, now in my possession, they may be accepted as a mite to the fund which the industry of abler investigators will no doubt soon accumulate. As a preliminary, I would remark, that we are not to trust to the colour or apparent vascularity of an intestine, as a proof of existent inflammation. Dr. Venables has exposed the chemical error of believing every greenish fluid contained in the stomach of a cholera patient to be bile; the florid colour of the intestines has frequently led observers into a similar error, which a careful inspection of the prepared intestine would have removed. I have remarked, on many occasions, that exposure to air, to water, and perhaps the action of the various substances contained in the intestines, have communicated a florid colour to the blood in the capillary branches, and thus given the intestine a colour nearly as deep as in inflammation; but by pressing on the trunk of the vein, the dark blood would be driven in to some of the smaller branches, and in part diminish the florid appearance of the injection.

The mucous coat of the intestines of patients who die, both in the primary and secondary stages, sometimes exhibits large florid vascular patches which look very like inflammation; by preparing the intestine I have ascertained that these injected patches are formed by extravasation of serum, holding in solution the colouring matter of the blood, into the submucous tissue; the extravasated fluid is not blood, for it is too light-coloured; it is generally darker in the

neighbourhood of the vessels from which it is poured out, and gradually becomes lighter and more transparent.

I have also prepared portions of the great and small intestines in various patients, and in all have found the arteries distributed to the villous coat, remarkably contracted, often reduced to mere lines, so as to require a magnifying power to render them apparent, and in a condition quite incompatible with the existence of inflammation, while the veins were distended in their trunks, and injected in their smallest branches. I do not mean to affirm that this condition of the arteries is constant, but simply declare that I found it where the intestine was very florid and vascular.

I shall conclude these few observations by the statement of another fact, which indeed the great serous discharges per anum would lead us *a priori* to conclude; viz. that the capillary mouths of the veins opening on the intestines are preternaturally relaxed. This conclusion is warranted by the observation of a circumstance which has occurred to me more than once in the course of my dissections.—While removing the intestinal contents from a portion of gut tied at both ends, and opened by a short incision to give exit to the rice-water fluid, I have noticed that the first portions of the fluid were quite milky, but on pressing the gut between the fingers the fluid always became pinkish; on allowing the fluid to stand for some hours it separated into two portions, one white and fibrinous, the other, which floated on the top, reddish, and composed of serum holding the colouring matter in solution; from this fact I conclude that very slight pressure was sufficient to make the serum and colouring matter exude from the capillaries, and also that the additional fluid was discharged by the capillary veins, because on minute inspection I found the veins dilated, and the arteries contracted. These few observations are given as the result of some little practical acquaintance with the pathological anatomy of cholera, and may perhaps lead to more extensive and useful researches.

NOTE FROM DR. O'SHAUGHNESSY.

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To the Editor of THE LANCET.

SIR,—The editor of a contemporary journal has paid me the compliment of informing his readers that I am the sub-editor of THE LANCET. Permit me to state that he is mistaken, as I have never occupied that situation.

I am, Sir, your obedient servant,

W. B. O'SHAUGHNESSY, M.D.

WESTMINSTER MEDICAL SOCIETY.

Saturday, April 14th, 1832.

Dr. STEWART in the Chair.

THE CHOLERA.

Dr. GREGORY commenced the discussion by asking Dr. Johnson whether he still adhered to the opinions he advocated some months ago in a letter or two addressed to the *Courier*. Dr. Johnson had therein stated it as his opinion that the variations of temperature were prejudicial to the violence and spreading of such a disease as the cholera in this country, and he now took the liberty of asking him whether circumstances had caused any alteration in his judgment.

Dr. JOHNSON wished that his catechiser, when quoting, would stick a little more closely to the facts. He was glad at all events to find that an opinion of Dr. Johnson's should, after a lapse of several months, form a topic of deliberation for the mind of Dr. Gregory. But to the point. What had he stated? Why he had run the risk of prognosticating, as an individual, that the cholera either would not attack this country, or that, if it did, the climate, assisted by the habits of the people, would put an efficient check upon its course. He had travelled somewhat further from his native land than Dr. Gregory; in fact, from the land of the origin of this disease to the West Indies, and having thus been enabled to compare its effects on various countries, he had been enabled to predict that it would be devoid of its usual virulence in this country. Had not the result fully confirmed his views? Look at it in London and Paris. In England this epidemic had now been six or seven months; in that time it had visited twenty or thirty individual spots, and carried off two or three thousand persons. Did not this support his argument? Fever had killed an immense number compared with the "raging pestilence." In particular spots, as at Musselburgh and Ely, where the cholera had been very destructive, there were peculiar local circumstances to account for it, and in London eight-tenths of the cases occurred on the right bank of the river.

Dr. GREGORY thought Dr. Johnson unnecessarily warm. His object in popping the question had been merely to ascertain whether Dr. Johnson's present opinions corresponded with those of yore, and now having been satisfied with the reply, he should have the more pleasure in examining the details of the letters referred to.

Dr. GRANVILLE. — The little rencontre

commenced by Dr. Gregory was very amusing. He, and no doubt the Society generally, thought that the object of the query was, after obtaining Dr. Johnson's admission, either to convict him of error, or to give confirmation to his opinions. But no; the only consolation after all was, that the learned Doctor would now look back and peruse the documents to his perfect satisfaction. Really Dr. Gregory, from his official opportunities, might have been expected to bear some testimony, either negative or confirmatory, to the statements of Dr. Johnson, which, in the opinion of the speaker, had been fully borne out.

Mr. GREENWOOD, adverting to Dr. Johnson's observations on the cases in the Borough hospitals, said he had, from personal investigation, acquired certain facts, overlooked by Dr. Johnson, and tending to completely overturn his deductions. The account then given by the steward of Guy's was, that the cholera had been introduced by a poor man, who having been found lying at one of the gates on Friday, was taken into Petersham's ward, and there died on the succeeding Sunday with every symptom of cholera. Well, upon going to the matron's room in this ward (so we understood) she answered his questions about the nurses. He found that in that ward out of thirteen cases, eight had had cholera, of whom five died. One of the nurses, Eliz. Powel, a night-nurse, was allowed the liberty of going out during the day. One evening she returned, after being absent some hours, when being taken suddenly with the different symptoms of cholera, she was removed to Petersham's ward and died the next morning. In this ward the nurse who succeeded her ten days after, slept in the same bed. This was followed by an attack of the same disease. Eliz. Crabbe, one of the night-nurses, went to Bermondsey where the cholera was raging, stopped there a week, returned, and was seized with the cholera morbus. What conclusion was now to be drawn from the cases in this hospital? He thought that Dr. Johnson had leaped to his conclusions without fully examining into facts, and considered that the details he had given were decidedly corroborative of the doctrine of contagion. He had seen in the *Morning Herald* of a late date a letter from the French correspondent, who stated that a change of opinion seemed to have taken place amongst the faculty in Paris, who now thought that the disease was an epidemic spreading by contagion. This was a set-off to what he had designated the "hasty conclusions." He concluded by referring to the excellent effects caused by the institution of distinct cholera hospitals; no doubt the insulation thus established by the Board of Health had tended most mate-