chest, but I was unable to satisfy myself that it came from that part. I also examined, as well as I could, the abdomen, but I was still equally unsuccessful in satisfying myself as to that origin of the fluid. I stated, indeed, that I could not say whence it came. I would neither call it hæmoptysis nor hæmatemesis, but I said we would wait patiently until we could clearly ascertain its source. Before my second visit, Dr. Davison found this lady in possession of a lancet, with which it appeared she pricked her gums, and she then sucked them, and presented the blood. I believe a paragraph has appeared in some of the newspapers stating, that we were all deceived in this case, that the girl was not only an impostor, but that she practised her imposition successfully. Now that account is perfectly incorrect, for we never pretended to know the origin of the blood. We could not, as I have already stated, allow that it was hæmoptysis, neither could we allow it to be hæmatemesis, but said nothing about it, having determined to wait and see whence it came; and so doing, we discovered it to be a trick.

There is a patient now in the hospital spitting blood, which exhibits the same appearance. She has had extreme sensibility of the chest and abdomen, but by taking iron the affection has left the former situation, and is confined to the latter. I may mention that she used the shower-bath, and had there been much chronic inflammation, it would most likely have done her injury, which it did not. Now this girl shows blood in the same way, and I dare say that if she have not a lancet, she procures the fluid by suction.

Both these ladies, I think, have some sort of depraved desire, for each of them was found to have a catheter in bed. The first patient had no reason to use it, for I believe her urine passed freely; but still a catheter was found there, though it was a very small thing compared with some articles that have been found under similar circumstances, and she showed a strong desire to secrete it. The other girl, who is now in the hospital, and who spits forth blood, pretends to require the catheter, not for the vagina, but for the urethra, and asserts that she cannot pass her water without it. The instrument has been taken from her, and her water is regularly drawn off twice a day; but it is very likely that this is all a pretence, and that she can pass her water if she pleases, without the aid of an instrument.*

* This turned out to be the fact, and the young woman now not only passes her urine freely, but has lost her tenderness nearly entirely, under the use of large doses of iron and a cold shower-bath twice a day, though, before her admission, she had in vain been cupped and starved. — RER, L.

PROPAGATION OF THE CHOLERA.

LETTER FROM DR. JAMES JOHNSON.

To the Editor of THE LANCET.

I CAN assure your "Correspondent" that it is neither my wish nor my interest to misrepresent either men or books; nor has it been my practice, as a journalist, except by inadvertence. Some allowance is to be made for the hurry of a debate, and for the inaccuracy or misapprehension of reporters. Thus in the report in question I am made to deny, that I ever advocated "contingent contagion," though I never ceased to advocate that doctrine. What I denied was, the having changed my opinions. A hasty perusal of the article in *THE LANCET*, on the evening of the discussion, impressed me with the idea that the writer of that article had greatly modified, if not altered, his opinions respecting the propagation of cholera, and a reperusal has only tended to show me, that it would probably have been more correct to say, that "in the opinion of *THE LANCET*, cholera has changed its character relative to the mode of propagation, since it left Newcastle."* The writer maintains that up to the above period, it travelled "step by step, and with all the characters of a disease still *essentially* contagious." But subsequent facts and successive irruptions of the disease in distant and unconnected localities, have induced the writer to acknowledge that "the statistics of cholera have undergone the most complete alteration." "These facts," observes the writer, "prove that *infection* is no longer the *essential* cause, and even tend to indicate that, for a time, its operation may be materially suspended." The above passages led me to conclude, that the writer had changed or greatly modified his opinions, though perhaps my mode of expression was too strong, but certainly not designed to misrepresent the meaning of the passage.

Before closing this note, I would ask the writer of the article, what better proof is

* Had Dr. Johnson here said, "the indications of its character of propagation," &c., he would have more accurately described our statements and opinions. We repeat, that up to the time when cholera reached Newcastle, every step in its progress indicated the march of an essentially contagious disease. On the other hand, since that time its many-leagued jumps as plainly point to the probable operation of another cause. We also willingly admit that the evidence supplied by the Haddington, Kirkintilloch, and Paris irruptions, materially operates against the conclusions deducible from the statistics of the disease up to the time when it reached Newcastle. We repeat our opinion, however, that no reasoning being, unendowed with the spirit of genuine prophecy, could come to any other conclusions than those we drew from the premises supplied us at each successive period of our debating this subject. — ED. L.

there of the introduction of cholera into Sunderland from Hamburgh than from Newcastle into Haddington? What better evidence have we that the contagious germ crossed the ocean from the continent to England, than from Haddington to Kirkintilloch, or any other distant locality? One thing is now clear, that, since the disease has come under our own observation, it has either lost its essentially contagious character, or more rigid scrutiny has divested it of a character which it may not have possessed, notwithstanding the imposing accounts, and the progressive statistics, of the epidemic. A cholera report is now hardly published without some remote and isolated spot exhibiting the disease, while all attempts to trace its footsteps, so visible (as is said) on the continent, are here given up by almost universal consent.

Among the various appellations by which the *nova pestis* has been described, it is strange that no one has christened it "CHOLERA SALTANS" or the leaping cholera; for no disease has shown such a propensity for hopping from one town or county to another, without touching those places over which it leaped. There is reason to believe that cholera is changing its character in more respects than one. In Paris they have three kinds of cholera, one of which is the *red-hot* cholera, as contradistinguished from the "cold-blue." In Dublin it appears, by a recent communication, that "the malady has decidedly altered its type since first introduced. Not one patient in thirty has it now in the original form." Its more general form on the banks of the Liffey is that of fever, with cramps, and gastro-enteric irritation. These changes in the disease itself, may enable us all, with less inconsistency than is seen in higher quarters, to modify our opinions on the score of expediency, where we are unwilling to attribute such changes to conviction. I am, Sir, yours, &c.

JAMES JOHNSON.

Suffolk-place, 15th May, 1832.

A Treatise on the Injuries, the Diseases, and the Distortions of the Spine, founded on an Essay to which the Jacksonian Prize for the year 1826 was adjudged by the Royal College of Surgeons. By R. A. STAFFORD, M. R. C. S., &c. London, Longman, 1832. 8vo. pp. 302.

THE numerous and important affections resulting from disease of the spinal marrow, of its membranes, or of the bony canal by which this portion of the nervous system is enclosed, have long engaged the serious

attention of practical men. Amongst English authors who have written most fully on diseases of the vertebræ and spinal marrow, we may enumerate Earle (Sir John), Macartney, Baynton, Copeland, Ward, Shaw, and Boyle, while the excellent treatise of Ollivier* shows that our continental neighbours have pursued the subject with equal attention.

The treatise now before us, is an addition, though not a voluminous one, to the works on this branch of human maladies,—a plain practical comment on the most important disease of the spine; not a lengthened disquisition, full of historical details, but confined entirely to such points as are most likely to require the attention of the surgeon at the bed-side of the patient. The several affections which the volume embraces are illustrated by numerous cases, the value of which is considerably enhanced by accurate accounts of post-mortem appearances. We sincerely wish that more of our modern authors would imitate the example of Mr. Stafford in this respect, and so substitute important facts, for ill-contrived and groundless theories.

The arrangement of the subject-matter in this work is good. Congenital diseases of the spine are first treated of; next its injuries, comprising concussion, fractures, and dislocations; then diseases of the vertebræ; and, finally, diseases of the medulla spinalis and its membranes. We do not propose to follow Mr. Stafford through the whole of this arrangement, which necessarily contains much with which medical men are familiar, but shall confine our observations to some extremely well-illustrated points.

Of the malformations which arise from imperfect development of the medulla spinalis, or the vertebral column, the chief is that denominated *Spina Bifida*, a congenital disease consisting in a deficiency of the spinous processes, or the rings of the vertebræ. In consequence of this deficiency, a pouch or tumour is formed at some part of the back corresponding with the preternatural opening, and consisting, usually, of the membranes of the medulla, distended by the fluid contained in the canal, which is secreted in increased quantity. The

* *Traite de la Moelle Epiniere.*