propagation of cholera, we are, on the other hand, supplied with more full and sound evidence towards the support of certain facts and opinions before promulgated, and the further support of which, or the proof of their instability or erroneousness, were, in reality, the things most needed. At the same time, too, that it is seen which facts and opinions the given data support, it is also made apparent which views they negative or render doubtful. It is in this respect that the Reports before us must be regarded. We already knew, or we believed it to be the case, that an epidemic of cholera is of very unequal and partial distribution, quite the contrast to an epidemic of influenza, and this whether a large or small space was concerned; that it is more fatal on the coast than in the interior—by the sides of rivers than upon hills; and that its special malignity is shown where, in addition to these intensifying circumstances, the dwellings are undrained, the apartments crowded and unventilated, the surface soil covered with decomposing organic matter, the drinking-water tainted by it, and the inhabitants themselves addicted to drunkenness. We believed, also, that we might be satisfied of the fact, that the time of the duration of an epidemic of cholera in a town was markedly long in comparison with that of some other affections; that it has periods of increment, acme, and decrease; that a high temperature favours its growth, that a cold one retards it; that after a time the epidemic disappears altogether; and passing over many of the generally-accepted doctrines, we may say that we were all of pretty well one accord in this, that much as we knew, or seemed to know, it was clear “that some unknown cause exerts an important influence over the degree of prevalence of cholera; the decrease in the force of the epidemic has been due to some other agency than the ordinary meteorological conditions, and at present the nature of this agency is unknown,” (pp. 39, 40.)

To all these things, our ignorance not excepted, the Reports before us add further confirmation; and it is in the clear and evident way in which the proof is afforded, the cautious and eclectic manner in which opinions are arrived at and inductions formed, that, upon most of the points touched upon in Dr. Baly's Report, the evidence *pro* and *con*, so far as it is before the profession, may be said to be exhausted. We shall now lay before our readers a condensation of some of the more important conclusions arrived at by the reporter as demonstrated in the Report:—1. The theory that the cause of the disease is a general state of the atmosphere—a “general atmospheric influence,” or “epidemic constitution,”—has been found untenable, but the inference is suggested that it is a material substance, and only partially distributed. 2. It is inferred that the matter which is the cause of cholera increases, and finds the conditions for its action under the influence of foul or damp air with the aid of some degree of warmth. 3. The increase of the epidemic at its climax results chiefly from the increased number of individual localities then affected, and that state of the atmosphere which favours the transmission is generally associated with a high temperature, and that which arrests it with a low one. 4. It may be further inferred, “from the very local and persistent character of the outbreaks of cholera, that its cause is not a gaseous diffusible substance, but rather matter in the form of solid or liquid particles capable of attaching themselves to surfaces of other bodies,” (p. 218.) 5. “A large body of evidence renders it certain that human intercourse has at least a share in the propagation of the disease, and that it, under some circumstances, is the most important, if not the sole means of effecting its diffusion,” (p. 218.) 6. But this propagation of the disease by human intercourse does not prove its contagious nature,—the chief evidence we have is in favour of the belief that the propagation of the malady cannot be maintained by any matter emanating from the bodies of the sick; nevertheless, there is other evidence which, though conflicting “is in some instances of such a character as to preclude the absolute rejection of the view

that the disease has a contagious property, even though it does not usually spread by virtue of contagion,” (p. 222.) Finally,

“The question whether the poison of cholera enters the body through the lungs or through the alimentary canal has not been conclusively solved, but no sufficient reasons have been found for adopting the theory that the poison is swallowed with the food or drink, is reproduced in the intestinal canal, propagates the disease by finding access in the same vehicle to the stomachs of other persons.”—p. 222.

We regret that the space at our command will not permit of our dwelling upon Dr. Gull's labours; this, however, we must less repine at, seeing that his Report is,

“From the state of our knowledge, necessarily fragmentary. The design has been to set down the principal ascertained facts, and to draw such conclusions as were possible from them. It is obviously not intended to be an essay on the disease.

“The section on treatment contains little that is positive; its value, if any, lies in removing the obstructions which prevent a simpler view of the subject; since, having learned what is useless, we gain a better ground for future success.

“The epidemic of 1849 was already declining when this report was undertaken, hence no opportunity was afforded for original inquiry.”—p. iii.

To all interested in the subject of cholera (and who of the profession is not?) the Reports of the College of Physicians may be strongly recommended.

ENQUIRIES INTO THE NATURE OF EPIDEMIC CHOLERA.

To the Editor of THE LANCET.

SIR,—Having attempted to arrive at some positive deductions as to the nature of Asiatic cholera, perhaps you will allow me to repeat them, with mention of an additional fact.

1. “That such cholera is infectious.” It is evident that the Board of Health have returned towards their opinion of 1832, for now they merely state that this disease “has everywhere overleaped the bounds of quarantine;” and do not, as in 1848, absolutely deny the infection of the cholera poison. Numerous additional proofs of the same are appearing in the medical and other journals, and the balance of proof is in favour of cholera being infectious. Of course it may also be epidemic.

2. “That natural depressing agents alone accounted for those individuals being attacked who were so.” There has not been, I believe, any invalidation of this belief: indeed all the arguments for hygienic measures act upon this principle.

3. “That the disease corresponds in every particular to an erysipelatous inflammation of the intestinal mucous membrane, due to a peculiar poison, which may be considered to be of the class of low inflammatory poisons, and by its intensity analogous to, and ranking with, the separate sub-class or division of erysipelatous inflammations.”

I wish to add one more fact towards this statement. It was proved in 1848 that the epithelium of the intestinal mucous membrane is not found in the stools of cholera. This appeared to be an argument against the existence of inflammation. However, in examining the intestines lately of a man succumbing in the algide collapse, an immense quantity of flakes of epithelium were found in the canal, though the motions contained none such. This epithelium of the columnar kind was generally separated in extended layers composed of some dozen or two epithelia normally cohering. The villi of the mucous membrane were often entirely denuded. The motions had been examined both in the middle of the attack and after death.

As so few thorough post-mortem examinations are being now conducted in cases of cholera, it is difficult to obtain confirmation of this fact; perhaps some of your readers, however, may be able to furnish further proof of a statement apparently insignificant, yet of great importance. It is difficult to conceive the possibility of asphyxia of the lungs, altered state of the blood, or fungi in the intestines, producing such intense local effect in a part of the body protected from ordinary external agents as the intestines, as should lead to destruction of the tissue of the part.—At least no instance is yet known, while such effect is the common result of a low poison exciting inflammation. Such results are never due to congestion, to whatever intensity it may rise. Mr. Hillier, of University College Hospital, also examined the flakes in this case with the same results.—I am, Sir, your obedient servant,

S. F. STATHAM