and pyloric orifices were not at all involved in the disease. The liver, spleen, and left kidney were healthy; but several cysts appeared upon the other kidney.

CASE II. J. L., aged 56, a greengrocer, who worked hard and had had a good deal of mental worry, came into the Hospital on the 18th of April, 1855. He was in good health until eighteen months ago, when he began to experience occasional "twinges" of pain in his stomach, and during the past three months they had attacked him more frequently. A fortnight before his admission he vomited for the first time; and a week later he suffered incessantly from hiccough and eructations. He was pallid and emaciated; the bowels were confined; pulse small and thready; skin cold; and he was unable to lie down, owing to the constant eructation.

April 19th. At night he vomited nearly three quarts of thin watery fluid. A dose of aloes and senna was ordered for the morning; and then he commenced taking the nitrate of bismuth with magnesia.

April 20th. Very low spirited. The sickness returned at intervals. Brandy was ordered; and a blister put on the epigastrium.

April 24th. Somewhat easier. The vomiting ceased on the 22nd. An enema every evening kept the bowels gently open; whilst a few drops of chloroform added to the mixture acted almost magically in relieving the epigastric pain.

April 30th. He vomited a quantity of dirty blackish fluid. There was more singultus; the pulse was very weak; and in the afternoon he had two attacks of syncope. A creasote mixture was prescribed, and acetum cantharidis rubbed on the epigastrium. This relieved the pain, and he got some sleep; but on the following morning the sickness returned, and continued until midnight, when he sank from exhaustion.

At the examination, which was made thirty-six hours after death, all the thoracic viscera were found healthy. The stomach was large, and greatly distended, and its pyloric end was intimately adherent by old bands of lymph to the under surface of the liver; whilst the pancreas adhered to it behind. The contents were chiefly bloody grumous fluid. On laying it open, a large and deep ulcerated cavity was observed near the pylorus, about the size of a crown piece, and here the mucous and muscular coats were entirely destroyed. The adhesion of the pancreas alone prevented any communication between the cavity of the stomach and the sac of the peritoneum. The pyloric orifice was encroached upon by the ulceration, and the surrounding mucous lining was highly vascular. The whole of the muscular coat was thickened, especially around the pylorus; but here the thickening entirely depended upon chronic inflammation. All the other organs were healthy.

REMARKS. Examples are here afforded of two very dissimilar predisposing causes of subacute inflammation in the mucous lining of the stomach: privation when the system required support amidst undue physical exertion, in the one case; in the other, mental anxiety and its consequent depression. If, according to Dr. Budd's theory, we regard the secretion of gastric juice as a reflex process which is sometimes excited, even in the absence of food, by mental emotion, and which may cause softening of the mucous membrane, it is fair to infer that long continued harass weakens the vitality of the stomach; that the gastric juice, in common with all other secretions, is vitiated; and that this twofold change renders the viscus obnoxious to any exciting cause of inflammation.

In the second case, where the greatest amount of mischief existed, vomiting did not come on until a late period: pain, however, had throughout been an urgent symptom. Its severity and long continuance arose from the extent of ulceration in the neighbourhood of the pylorus, where, necessarily, the food in its passage came in contact with a surface stripped of its protecting tissue.

The first patient was, indeed, not free from pain, but the situation of the ulcers rendered them less exposed to constant irritation from matters taken into the stomach. The short and hurried breathing no doubt arose from a desire to avoid the frequent descent of the diaphragm upon a tender part.

Cases in which a complete chain of ulcerations existed all along one or other of the curvatures used to be reckoned extremely rare; but a paper by Dr. Habershon in the last volume of *Guy's Hospital Reports* contains several instances. I cannot find any similar records of continuous ulceration along the greater curvature. It is somewhat remarkable that in Case II. traces of inflammatory thickening were found only about the pylorus. The state of the muscular coat in the other parts of the stomach appeared to be one of hypertrophy. This hypertrophy was general, and not limited to the pyloric

extremity, as might have been supposed, owing to an impediment at the outlet, requiring increased muscular power to propel the food. I incline, therefore, to regard it as dependent upon frequent irregular muscular contraction; the result of implication of the sympathetic coronary plexus and the pneumogastric ramifications.

In Case I. even, where no obstruction existed at the pyloric outlet, there was general hypertrophy, probably from the same cause. That this notion is not far fetched, experiment and observation daily force upon our attention. Many cases of gastralgia, and supposed congestion of the liver, especially its left lobe, are now found to recover by the aid of nerve tonics; proving the immense, although obscure, influence of the nervous system upon the stomach. And this leads me to remark, in conclusion, that where it may be presumed from the history that superficial ulceration of the mucous coat is commencing, it would be well to give the nitrate of silver a more extensive trial than it has yet enjoyed.

Original Communications.

ON THE AIR-CELLS OF THE LUNGS.

By THOMAS WILLIAMS, M.D., F.L.S., Physician to the Swansea Infirmary.

IN common probably with many readers of the JOURNAL, I have read with great interest the lectures of Dr. Edward Smith "On the Nature of Phthisis," recently published. If permitted, I will take the liberty to place before the author a few of the doubts which disturb my mind with reference even to the very groundwork of his theory.

1. It assumes as true that which is, by many very competent observers, strongly disputed, viz., that the air-cells of the lungs are lined by an epithelium. Upon the basis of this hypothesis he erects a graceful and airy fabric, itself remarkably fanciful. To this proceeding I object, that the truth and soundness of the *foundation* should first be placed beyond all doubt; in other words, I contend that that part of his general proposition should be presented *in limine* as an incontrovertible *certainty*. But of this portion of his theory Dr. Smith seems to have thought the least. In my article "On the Organs of Respiration" in the *Cyclopædia of Anatomy and Physiology*, I have adopted the view advocated by Schröder, Kölliker, and many of their pupils, in opposition to that so ably and originally enforced by Mr. Rainey. At that time, from careful observations which I had undertaken, I felt very confident that Mr. Rainey was wrong; subsequent researches have diminished that confidence, but only to such an extent as to impress me with the desire that the whole question should be again calmly and impartially examined. In the last number of Siebold and Kölliker's *Zeitschrift für Wissenschaftliche Zoologie* is contained, by Dr. Carl Semper of Altona, an able article on the air breathing gasteropoda, in which, in opposition to the results which I had published on the same subject, he labours with great care to prove that the lacunose network of blood channels which line the walls of the pulmonary sac of these molluscs, being the precise situations at which the respiratory process proceeds most actively, *are not lined with any epithelium whatever*, and that the epithelium is limited in its distribution to the course of the larger blood trunks.

In Müller's *Archiv* for 1853, Leydig, a very distinguished observer, states with great stress that in the fish, *cobitis fossilis*, "die Stelle des Darmes welcher die Athem-function zuertheilt ist eines Epitels entbehrt, und vielleicht dürfte sich eine solche Verhältniss selbst bei den Lungen höherer Thiere nachweisen lassen." Such a statement proceeding from such an authority should at least command respect. Since the date of my article in Todd's *Cyclopædia*, I have worked probably as industriously as any anatomist on the subject of the special histology of the respiratory organs. The results of my researches will be given at length in the first part of my forthcoming work. I wish only at present to lament that Dr. Smith should vainly expend so much talent and energy upon the construction of a pathological theory, which founds itself upon a histological question still notoriously undecided. No competent judge will accept his views, unless he adduce *proof* that the *groundwork* is beyond dispute.

2. But let it be admitted that the air-cells are really lined by a layer of epithelia, Dr. Smith's doctrine as to the nature and

origin of phthisis still assumes as unquestionable that which is more and more doubted as the science of animal and vegetable morphology advances. The cell theory of Schleiden and Schwann is every day less and less implicitly received! If this theory should be demolished, Dr. Smith's hypothesis will "leave not a rack behind" to bear even a sepulchral witness to the credulity of its author! If Mr. Wenham's recently published views with reference to the mechanism of cell-formation should be confirmed by the march of discovery, Dr. Smith's hypothesis will become at once a "dissolving view!" If, by the progress of correct observation, the highly philosophical and probable opinions of Wedl in relation to cancer and its varieties should receive further support, Dr. Smith's speculations will lose at once all bodily form!

The preceding criticism I offer with the utmost respect. Dr. Smith is capable of better things than his "Theory of Phthisis." He may rest assured that his speculations will conduct him further and further away from that pathway of cautious induction, along which the correct thinker on such a subject as that on which he writes should even desire to travel. He seems very skilled in weaving one vision into another. But I think that any earnest student, who during the last few years has followed the current of discovery in organic chemistry, normal histology, and pathological morphology, will agree with me, that in the present remarkably transitional state of physiological and pathological knowledge, no living man can summon to his presence adequate materials for the construction of even a probable theory either of tuberculosis or cancer.

ON CHOLERA: ITS DIAGNOSIS AND TREATMENT.

By C. W. BELL, M.D., K.L.S., late Physician to H.M. Embassy in Persia, etc., Buxton, Derbyshire.

WEARY though most people are of the very name of cholera, it appears a duty to take advantage of the comparative immunity this country has enjoyed from that disease during the past year as a more favourable time for dispassionately considering the information we possess respecting it, than either while it is present, or its advent apprehended.

I have devoted much leisure to the perusal of recent contributions to the literature of the subject, as well as of works considered by the profession as authorities, and truly a less satisfactory course of reading could hardly be pointed out. So varied are the views adopted by different authors, that, though it was at first my intention to try to elicit truth by a process of exclusion, reviewing opinions and rejecting what appeared untenable or illogical, I soon became satisfied that no one would consent to wade through such a mass of inconsequential lucubration as this plan would have entailed upon the reader, or have the patience to inquire how conclusions the most opposite, arrived at by authors, according to the point of view from which their opportunities permitted them to survey the disease, were capable of being reconciled like the celebrated dispute of the two knights who each saw only one side of the shield. In many of these treatises, it might even be doubted, but for the title-page, whether we had not by mistake taken up a work on a totally different subject instead of one on cholera asphyxia, maligna, Asiatica, algida—for such are the recognised epithets bestowed on the disease in question. Upon the whole, therefore, it appeared the preferable course, as being the only one that would admit of the whole subject being brought within reasonable bounds, altogether to abstain from the citation of authorities, and to trust instead to the readers' knowledge of individual opinions when such are alluded to, and of the facts which I have made the basis of my argument.

A careful examination of the literature of cholera has satisfied me that more than one disease essentially distinct in nature, cause, course, and character, has been described under that name; and until this be fully admitted by the profession, we must despair of arriving at any settled conclusions with regard to its true nature and treatment. The evil that existed in 1831, when all Europe was turning with alarm to the East to watch the progress of the coming pestilence, still remains undiminished—we mean, the want of a definition of cholera: nor is it a definition only that is wanted, for strange and incredible as it may appear, there is not one single symptom, circumstance, or condition of the system, that all authors concur in describing as invariably present in an attack of this malady. It is a source of no small disappointment that no new light has been shed on this disease by the most recent work on the

diseases of India (*Clinical Researches on Disease in India*, by Charles Morehead, M.D.: 1856); I had looked with eager expectation to its appearance, in the hope that the distinctions which I am about to attempt to draw from reason might there have been established by practical authority.

The nearest approach which I can find in all the various descriptions of the disease to agreement in noticing any single symptom as characteristic, is a kind of general admission of the existence of a remarkable condition of the circulation and of the blood in bad cases. It signifies little to my purpose that very different degrees of importance are attributed to this condition of the circulation in cholera, as a symptom, and that very different opinions are entertained as to its cause. It is much to be able to fix upon even one fact admitted by the majority as constant in this disease.

A review of the whole subject would justify the conclusion that this peculiarity consists not in primary poisoning or alteration of the blood, but in a species of paralysis of the powers which circulate the blood, and such a change in the action of the capillaries as respects nutrition, secretion, and the evolution of animal heat, as can only be referred to disorder or cessation of the agency which the sympathetic system of nerves is acknowledged to exert over these functions in health; but were we to attempt to go into the proof of this before the subject has first been disencumbered of those forms of disease which have been mistakenly described as cholera, we should only be plunging once more into that abyss of darkness and confusion we are so anxious to struggle out of.

Let us, then, in the first place, attempt to distinguish in the different maladies described as cholera all such symptoms as do not essentially belong to its purely congestive form which we regard as the type of the disease, and refer each of them to its true cause, in order that we may perfect our diagnosis, and clearly perceive the rationale of the treatment required in each several instance.

Almost all climates, but especially the more tropical, are liable to epidemics of diarrhoea, which attack very suddenly. These very often commence as hepatorrhoea (if the term be permitted), a copious pouring out of irritating bile. This in hot climates commonly induces inflammatory ulceration in the glands of the colon in the course of a few hours, and the case becomes one of confirmed dysentery. In this climate a similar hepatorrhoea, attacking as suddenly, is what we call English cholera; but here, instead of inducing dysentery, it generally passes off without mischief after the bile has been evacuated by plentiful vomiting and purging.

Of this nature, too, is a large proportion of the cases of diarrhoea which occur every autumn, and which in general attract very little attention; but if cholera of the true congestive type occur at the same season, all such attacks are then termed cases of premonitory diarrhoea; not because this condition necessarily, and in all cases, precedes cholera, but because a tendency to bowel complaint of any kind becomes in times when that disease breaks out epidemically, a predisposing cause of great power to induce an attack of algide cholera.

The fact is well known, that even slight attacks, such as at other times would be thought too trivial to require attention, are liable, if neglected, to be quickly followed by symptoms of fatal collapse when cholera is epidemic; but to cure this diarrhoea, and so guard the patient from becoming the subject of that epidemic influence, is quite a different matter from curing cholera. Too many people's ideas are governed by a name. The title of premonitory diarrhoea, given by authority to such affections, is considered by many to denote that such diarrhoea is an actual stage of cholera; and the practitioner who uses the term in that sense naturally concludes that in curing the purging with a little chalk, or lead and opium, he has cured a case of cholera. This is one of the greatest sources of error and confusion; for in the great majority of cases called "premonitory", the diarrhoea has in the first instance no connexion whatever with true cholera. The most ordinary means will arrest it; but this must be done at once, for if allowed to proceed, the disturbance of the circulation by excessive intestinal secretion, as well as that it produces on the whole sympathetic system, will very soon so invalidate vital resistance to the epidemic influence, that algide cholera is superinduced.

It is necessary here to observe, that cholera is not the only disease capable of being superinduced on simple diarrhoea. It is true that in this country we can hardly conceive the devastation suddenly spread over a whole district by the invasion of epidemic dysentery, to which the slightest disturbance of the bowels will then give rise; and we read of idiopathic peritonitis having been similarly epidemic, and its attacks connected with