two days after the disappearance of purging and cramps, hydrocyanic acid and other remedies having failed to check it, I was led to try creasote, which I found to act immediately and effectively, the patient soon after becoming convalescent without any consecutive fever. Since then—viz., from July 1st, to August 1st,—I have had 224 cases of simple diarrhoea, twelve of rice-water purging, and eighteen of cholera. The first ninety-three cases of diarrhoea were treated with the usual chalk mixture and opium, the remainder with creasote. In those treated with the former, the diarrhoea continued for some days, and, when checked, was invariably followed by a disagreeable rumbling and flatus in the bowels. In the 131 cases treated with creasote, the diarrhoea immediately ceased, and was followed by none of the above symptoms. In the twelve cases of rice-water purging the effect was instantaneous, the first dose generally putting a stop to the discharge. In the eighteen cases of cholera, in all of which there were vomiting, rice-water purging, cramps, and blue skin, creasote had the same decided effect, at once checking the purging and vomiting, the cramps very soon afterwards subsiding, the pulse becoming full and soft, and a free perspiration breaking out over the body and extremities. Of the eighteen cases of cholera treated with creasote, I have only lost two, and these were far gone in collapse when I was called to them.

"The formula in which I use the creasote is—Creasote, twenty-four minims; mixture of gum arabic, half an ounce; compound spirit of ammonia, camphor, of each two drachms; chloric ether, three drachms; water, six ounces and a half. Mix. Two tablespoonfuls to be taken every hour. In simple diarrhoea, I only give two drops of creasote every two hours, with the above stimulants, and no astringent whatever."

Mr. CHIPPENDALE, after some other speculations on the pathology of cholera, remarks—

"Supposing chylication actually to take place, the current of fluid which is setting with such force towards the interior of the bowels may prevent the absorption of that pabulum which is to renew the vital stream. These (i.e., previous considerations) may all be summed up in the fact that the balance between the act of sanguification and the deterioration of the blood is destroyed. The process of respiration, therefore, becomes useless, so far as the evolution of animal heat is involved; carbonaceous matters are not burned off in the lungs; and the introduction of air into those organs has only the effect of lowering the temperature."

He subsequently adds—

"Upon these premises, then, a suggestion is hazarded in favour of the employment of cod-liver oil. We have ample experience of the efficacy of this substance in arresting colliquative diarrhoea; and thence we may seem justified in entertaining the hope that the same effect may follow its exhibition in this disease. From the fact of its being an oil, it may serve to lubricate the mucous membrane of the alimentary canal, and thus to allay the irritation; and by these two means to place the surface in a condition to resume its office of absorption; and this latter function being re-established, we have a plentiful supply of carbonaceous matter to be burned off, as well as to be deposited by the capillaries, in a more solid state, in different parts of the system adapted to its reception; and by both these means to cause the disengagement of heat and the consequent due supply of it to the whole organism."

Mr. Chippendale protests against any supposition "that the hint was taken from a letter which was inserted in *The Times* of the 14th of July, where a mode of treatment is advocated by means of the internal administration of three cupfuls of olive oil, and the subsequent swallowing of large draughts of water. The whole of this scheme (he says) is worthy alone of the combined efforts of Celsus and of Dr. Sangrado; indeed, were we acquainted only with the style of the former, and not also with the whole of his writings, we might positively have averred that the description of the treatment emanated from the Roman author, so exactly have we laid down the manner in which food and wine are to follow the remedy. There is but one unfortunate circumstance attendant upon the whole proposal, which is this: that it would let the patient out of his sufferings through the wrong door, as it would most indubitably terminate in dissolution; for, in the first place, olive oil is a direct purgative, and its administration would therefore tend directly to aggravate the worst symptom of the disease; secondly, it is not in the province of olive oil to cause vomiting; and thirdly, if it had such an effect, it would be one of the most undesirable events to promote, for there is

perhaps nothing which, more than nausea, will so much prostrate the vital powers.

"Thus far as a disclaimer of having been prompted by Mr. Merry, who commits himself so sadly in *The Times*. But it were, indeed, well, if that otherwise admirably-conducted journal would refrain from touching upon matters which in no way pertain to the office of a political paper.

"To take another recent example from *The Times*: we find a clergyman advocating the exhibition of a glass of brandy, containing ten drops of chloroform, every ten minutes, to patients seized with cholera. There can be few, one should suppose, who would not know that the effect of such treatment would be to hurry the victim out of the world in a state of beastly intoxication.

"The only legitimate channel for the promulgation of professional opinions must be a professional publication. The best recommendation which can be tendered to the laity for the avoidance or treatment of cholera, or of any other disease, is that which was given by a member of our profession, who was also a celebrated wit of the last century, and who, on being asked by a friend what he had better take for the relief of some ailment, answered, 'Sir, you had better take advice.'"

ON THE VALUE OF DR. AYRE'S TREATMENT OF MALIGNANT CHOLERA.

By W. I. COX, Esq., M.R.C.S.E., Poplar.

I DESIRE to record the results of my experience in the treatment of Asiatic cholera, (which has been pretty extensive as assistant medical officer to a large union district,) and, through the medium of your columns, to lay before the profession the conclusions at which I have arrived after giving a fair trial to a great number of vaunted remedies. I present these my principles of treatment in a tabular form, to save space, and in order that they may be embraced at a glance.

1. That Dr. Ayre's mode of treatment (as a grand basis to go on) is the only one on which any reliance can be placed.
2. That certain modifications in, and additions to, the said mode, hereinafter detailed, are highly advantageous.
3. That calomel (or indeed any medicine) in large doses, and in longer intervals than ten or fifteen minutes, is utterly useless.
4. That opium in any stage and in any dose is positively pernicious.
5. That heat applied to the abdomen is hurtful.
6. That stimuli, in the state of collapse, such as brandy, capsicum, chloroform, &c., are nearly useless, and must be used with great caution, as they may do infinite future injury by lighting up gastritis, &c.
7. That the hot-air bath and remedies of that class are of no service, and therefore to be discarded, as their employment induces the loss of valuable time.
8. That the treatment, to give a good chance of success, should commence actively, ere collapse is well established.

Now the modifications and additions I advocate are as follows:—

That the calomel be given *alone*; or, at all events, not in combination with opium. (I give it simply with crumb of bread.) I object to the opium, as it is quite powerless to check vomiting or relieve cramps, and certainly is, *prima facie*, contra-indicated, where the cerebro-spinal axis is in such a state of prostration as in cholera. Moreover, it manifestly promotes the tendency to congestion of the brain; to which, when the stage of collapse is over, there is a strong disposition.

That the patient drink plentifully of rice-water, ice-cold, and strongly acidulated with sulphuric acid. The acid answers two highly important purposes, neutralizing the free alkali secreted by the stomach, and tending to destroy electrically the intensely negative condition of the gastric membrane.

That the face, neck, and upper part of the trunk be freely and frequently sponged with cold water, slightly acidulated with nitro-hydrochloric acid. This powerfully contributes towards restoring the static equilibrium of the nervous centres.

That as soon as reaction commences effervescent be given with slight excess of sulphuric acid, and a few drops of hydrocyanic.

Lastly, that as little positive stimuli, alcoholic especially, be given as possible. In three-fourths of the cases we have here before lost, the patients survived the collapse, but died of the consecutive fever, which was, I am now convinced, rendered thus intractable and fatal, by the previous unlimited and injudicious administration of stimulants.

Great pressure of engagements prevents me, this week, from sending you reports of cases. In the course of a few days, however, I shall, with your kind permission, trespass on your pages, with brief abstracts from our case-books, of a considerable number, strongly corroborating the truth of the above views, and illustrative of the success of the method of treatment I have advocated.—I remain, Sir, yours obliged,
Millwall, Poplar, August, 1849.

Reviews.

On the Dependence of Animal Motion on the Law of Gravity.
By HENRY WIGLESWORTH, M.B. London: Baillière. 1849.
Part I. pp. 156.

THE preface of the author makes known his reasons for putting forth this work. He there observes:—"In the following pages I have collected a body of evidence, which has satisfied my own mind, that animal motion is due to the one great law of gravity. I give it (?) to the public, that they may consider and determine on it likewise."

We have perused it, (i. e., the first part of the essay published,)—we have considered it, but we can arrive at no other determination than that Dr. Wiglesworth has left the matters he discusses, undetermined.

We, indeed, become almost lost in the maze in which the author revels. Hypothesis on hypothesis is reared up by the magic wand of Imagination, with or without one solid portion of well-ascertained fact to rest upon. The foundation of all his arguments (and many of those are illogical) is hypothetical. Let it be denied that any gaseous products are formed in the nutrition of muscle, or in any of the nutritive or active processes of the body, and the whole superstructure of his deductions falls to the ground. For the very first position Dr. Wiglesworth advances is,

"That all motion in muscle is due to the pressure of the atmosphere on the surface of the muscle, a vacuum having been previously formed within it by the union and condensation of gases."

And in supporting this notion, he adopts a prevalent hypothesis, that the development of the contractile force in muscle is in some way dependent upon the chemical change which seems to be so essential a condition of it. He then assumes

"That urea is not directly formed by muscular action, but that the first chemical change is the production of ammonia, which substance is presented to carbonic acid in such a form as to produce urea and water, rather than the usual compound carbonate of ammonia."

He next goes on to say, that carbonic acid and ammonia are the invariable products of muscular action, and both remarkable in that their vapours, when united, are condensed into half their bulk.

"Ammonia contains more volumes—viz., six of hydrogen and two of nitrogen, which, on union taking place, are condensed into four volumes. Now carbon, oxygen, hydrogen, and nitrogen,—the elements of muscle,—on uniting, occupy half the space they previously did. Hence it follows, that if these substances are formed during muscular action, as sound evidence proves them to be, a vacuum must necessarily be formed also, unless the muscle is capable of diminishing in size, and filling the space occupied by the elements before their union."

This impending diminution in size from the union and condensation of gases, which, the author conceives, is the necessary consequence of its chemical changes, is designated "the vacuous state;" and this vacuous state is presumed to take place in the ultimate cells—i. e., the sarcous particles of muscle, and the simple pressure of the atmosphere upon a muscle in a vacuous state, to afford the correct explanation of the "vital force" of contractility.

Such reasoning is, to our mind, supra-hypothetical; a similar train runs throughout the volume. Whether it be the phenomena of muscular or of nervous force, of nutrition or of secretion, to be explained, the formation of a vacuum in the

cells of the active tissues is the primary force, the pressure of the atmosphere the consequent and secondary one.

In those instances where the tubular or cellular structure is deficient, the difficulty is got over by supposing it. Thus, in the case of unstriped muscular tissue, the fibres are assumed to be hollow; in that of the non-tubular or gelatinous nerve tissue, those filaments commonly regarded as actually nerve-cords are denied that property, and considered, with some German writers, merely as fibrous tissue, the tubular fibres met with in the sympathetic system being alone deemed as agents in the transmission of nervous power. Again, to explain the nervous force, the nerves are described as filled with a fluid, which transmits pressure exerted at one end of the nerve-tubes to the other, according to the law of hydrostatic pressure.

In the process of nutrition, the formation of vacuous cells is the active agent; but we are, at the same time, to admit the existence of pores in those cells, and the passage of exactly proportioned particles of definite figures through those pores, in order to account for the selective power of the several tissues. Similar pores, and similarly adapted particles, are likewise assumed in secreting cells, which are nevertheless to be rendered vacuous.

We cannot now follow the author further; he has not convinced us, even to a remote probability, of the correctness of his physiological notions; his course of argument is frequently specious, but not sound, and his assumptions are too startling to be received, even if it could be always predicated of them that their ultimate basis is laid in fact.

But in this, Dr. Wiglesworth has, in our judgment, succeeded, that he has shown the inapplicability of mere mechanical force to the interpretation of the phenomena of life. To so apply it is indeed no novel attempt; some of the most brilliant minds of past centuries have endeavoured to explain what we must yet call the vital functions, by mathematical and mechanical formulæ, but without any satisfactory result. Descartes and his disciples contrived diagrams to illustrate the arrangement of the pre-supposed particles of the body in various bodily functions, and with much ingenuity; but such ingenuity was misused, as it was, also, in the case of the extravagant problems of the schoolmen of the middle ages.

We would rather go with Professor W. Alison in the belief of the existence of vital forces and affinities, the latter of which, we think, he has rendered not improbable in his examination of the phenomena of nutrition and secretion, in his essays published in the *Transactions* of the Royal Society of Edinburgh. Dr. Wiglesworth, we would observe, in conclusion, has had no recourse to the teachings of comparative anatomy and physiology, to which, however, if he had appealed, we believe he would have found his hypothesis untenable.

Those of our readers who find gratification in diving into the profounder depths of biological speculations regarding the hidden springs of life, and have no objection to follow the windings of imagination let almost loose, may consult this essay without fear of disappointment, and with advantage, too, if the judgment be brought to weigh facts against theory.

Foreign Department.

Benefit of a Multitude of Counsellors.

M. GOSSELIN, surgeon to the Foundling Hospital, presented to the Surgical Society of Paris, on the 28th of June, 1849, a woman, sixty-nine years of age, with a tumour covering the velum palati and the left tonsil. It had been growing for about eighteen months, and is of the size of a hen's egg; it is elastic, and no pulsation can be felt in it. M. Gosselin's diagnosis pointed to the cancerous nature of the tumour, but he had brought the patient before the Society in order to elicit a discussion both on the diagnosis and the best mode of operating. We shall not enter into a detail of the various opinions propounded, but will merely point out the judiciousness of the course adopted by the French surgeon, and would be glad to see his example followed in this country. A vast deal of in-