

"the unscrupulous a ready means of perpetrating their deeds of darkness." I will not dwell upon this objection, because I think it unfair to argue against a remedy on the ground of the abuses to which unprincipled persons may apply it. Such an objection would be out of place when addressed to the practitioners of medicine. But I fear we shall hardly be justified in concluding that these agents are altogether safe from some kind of abuse even in the hands of legitimate practitioners. There arise in practice frequent temptations to expedite delivery by the forceps; and when one great objection to their use, the pain apt to be inflicted, is removed, we cannot resist the conviction that they will often be resorted to unnecessarily. The sensitive mind shrinks from contemplating the mischiefs that will result. In Great Britain one mother in twenty dies under the forceps, and more than one child in five; to say nothing of lacerations, contusions, and other deplorable evils.—(Churchill.)

Because I demurred to accepting the arguments advanced by Dr. Simpson in favour of the use of chloroform in natural labour, the stupid charge of "inhumanity" has been directed against me. It remains to be proved whether the interest of "humanity" sanctions the general employment of chloroform in midwifery, or whether it does not emphatically condemn it. On this subject I cannot better express my own feelings than in the words of one of the most reflective writers of the present day—my colleague, Dr. Tyler Smith. The quotation is as applicable now to chloroform as it was a year ago to ether. "More recent experience has certainly not been in favour of enlarging the limits of its applicability in practical midwifery. In our own department there is good reason to believe, that after a short time, unless some certain mode of binding or disciplining this Prometheus shall be discovered, it will be rarely, if ever, used in difficult parturition or obstetric operations, and certainly never in natural labour. It will be a disappointment to have to turn from this promised good; but it is better to do so than to follow an ignis fatuus, if so it prove, to the neglect of real and scientific advancement. Let us hope that this glimpse of deliverance from this heavy infliction on humanity may act as a stimulus to Science to continue her search after some certain and available relief from physical pain, possibly a visionary search, but still one for which human nature will never cease to yearn."*

Having discussed one or two objections which have struck me on perusal of reported cases, I will avail myself of this opportunity to protest against the unfair treatment to which Dr. Meggison has been exposed in some recent comments upon the late fatal case at Newcastle. There is no part of Dr. Henry Bennet's paper which has given me more pain than that in which, falling into error as to facts, he abets Dr. Simpson in inflicting a great injustice on a brother practitioner. Dr. Simpson, with more ingenuity than success, laboured to show that Dr. Meggison's patient "did not die from the effects of chloroform, but from the effects of the means used to revive her." In other words, that Dr. Meggison killed his patient by improper interference. Dr. Bennet pleads for the credit of chloroform at the expense of Dr. Meggison in a similar manner, but makes him destroy his patient by different means. It appears to Dr. Bennet that the fatal event was probably owing to the neglect of the precautions of using a thin pocket handkerchief for the chloroform, and allowing the patient occasionally to breathe atmospheric air. Assuming that the chloroform was poured upon a table-cloth, and that this was kept closely in contact with her mouth and nostrils, Dr. Bennet infers that death was principally owing to asphyxia caused by the want of oxygen in the lungs. An appeal to the elements of physiology will expose the absurdity of Dr. Simpson's attempted exculpation of chloroform; an appeal to facts will set aside Dr. Bennet's. John Payne deposed at the inquest as follows:—"Dr. Meggison held a pocket-handkerchief to her mouth and nose; he kept it moving at times." An appeal to common sense will confirm the verdict of the jury founded on the competent evidence of Sir John Fife and Dr. Glover, that the "deceased died from congestion of the lungs produced by chloroform."

It is surely riding a hobby somewhat too hard, when all its mishaps are saddled on those who use it. The history of this unfortunate case may, however, furnish a useful lesson to those who are inclined to submit their judgments to the new obstetric dictator. Should they, governed by his authority, rashly adopt his practice, and, disappointed in the results, be visited with public censure, let them not hope that Dr. Simpson will come forward to protect them. He will direct against them individually the blame that should be imputed to his

own advice. While listening to Dr. Simpson's persuasions to use chloroform in every case of natural labour, it should be remembered that his credit is pledged to its universal adoption. Before the result of twelve cases was known to him, he had already drawn the conclusion, that it ought to be resorted to in every case of natural parturition. He had enforced his own premature conviction, by declaring that medical men may oppose for a time the superinduction of anæsthesia in parturition; but they will oppose it in vain, for certainly our patients themselves, and their friends, will force the use of it upon the profession." The profession may rest assured that this *argumentum ad crumenam* is no better founded than Dr. Simpson's other arguments. The almost universal feeling of the well-educated women of this country is resolutely opposed to the idea of abandoning the highest prerogative bestowed upon mankind, for the sake of flying from physical pains, when by so doing they may heedlessly run into other and more serious evils that they know not of.

Gloucester-terrace, Hyde-park, 1848.

ASIATIC CHOLERA: ITS PATHOLOGY AND TREATMENT.

By E. V. MAINWARING, M.D., Bournemouth.

WITH your permission, I will answer Dr. Armstrong's letter, he having mistaken the effect of disease for a cause.

His letter in *THE LANCET* (p. 375) says: "The black blood in cholera is probably caused by the spasm of the diaphragm and other muscles of respiration preventing the lungs from carrying on their functions, and consequently impeding the process of arterialization."

I will ask the doctor how he accounts for the "spasm of the diaphragm and other muscles"? If it be not produced by a poisonous gas, by what is it produced?

The doctor seems to despise tartarized antimony as a remedy in cholera; but as I have observed undoubted good effects from its use, I do not hesitate to recommend the treatment to others, even as a sedative. Tartar-emetic may relieve the "spasm of the diaphragm and other muscles."

The counter-irritation, opium, and stimulants, with one or two moderate doses of calomel—treatment which Dr. Armstrong prefers—I have known to fail in more instances than one.

The doctor says, "as far as his experience goes, Asiatic cholera is no respecter of persons." In this country it appears to have been more discriminating, its attacks being principally to the ill-fed, ill-lodged, and intemperate portion of confined the inhabitants.

The cholera in India, being more general in its attacks, may be accounted for by observing that the towns and cities are generally built on swampy ground, and near large rivers, having inefficiently ventilated houses and streets, with bad drainage—a combination of evils, in a hot climate, enough to affect the high and the low, the rich and the poor; and if we take into consideration the enervating habits of the higher classes in India, it will be easily understood why "cholera is no respecter of persons" in that country. An individual may be "a regular liver," as far as eating and drinking are concerned, but eating and drinking are not the only causes of debility, and consequently are not the only evils to be avoided. A ride in the hot sun, distress of mind, over-exertion, &c., or a single excess of any kind, will produce a liability to the disease when the poison is rife in the air.

In conclusion, I beg to observe, that the treatment mentioned in a former paper having proved successful in this country, it is its best recommendation; and in the event of a recurrence of the Asiatic cholera, I think it deserving a fair trial.

Bournemouth, Hants, 1848.

ON A NEW PRINCIPLE IN THE TREATMENT OF ASPHYXIA FROM SUBMERSION IN WATER OR PURE CARBONIC ACID.

By WILLIAM REID, M.D., Liverpool.

I BEG leave to suggest, as an improvement in the method of treating the apparently asphyxiated from submersion, the admixture of a certain amount of chlorine gas, or perhaps nitrous oxide—though I would give the preference to the former—with the atmospheric air used in insufflation of the lungs, or artificial respiration. This I am led to propose, on the prin-

* Lecture on Inhalation of Ether in Obstetric Practice, *THE LANCET*, March 27th, 1847.

principle that the chief object is, independent of restoring the circulation and temperature, (which, by the way, will naturally return on the respiration being duly carried on,) that the dormant irritability should be subjected to an extraordinary stimulant. While the common atmospheric air, or even oxygen gas perfectly pure, has no action, the irritative action of this gas, (the chlorine,) so well known in the living subject, will, in a corresponding manner, be more energetic in the lungs of the apparently drowned. In numerous experiments made on the healthy subject, and also on different invalids, we have found that in the former, on an average of about 200 individuals, one cubic inch of chlorine diffused through 200 cubic inches of air (common air) could be respired without exciting any irritation; but when the quantity of atmospheric air was reduced, the amount of chlorine remaining the same, more or less irritation was induced. It is unnecessary to remark on the different effects on invalids, as these varied very much, and it would be impossible to make nice applications of the sort in cases of asphyxia.

One cubic inch of chlorine may be diffused through fifty cubic inches of air for this purpose, or even one cubic inch in twenty-five cubic inches of atmospheric air. The respiratory movements once being excited, the amount of chlorine may gradually be reduced. Scarcely any bad effect will supervene which cannot be relieved by ammonia and hot water. The chlorine is readily obtained from the common chloride of lime.

Liverpool, 1848.

ON THE

INFLUENCE OF VOLITION OVER REFLEX AND OTHER INVOLUNTARY ACTIONS.

By WILLIAM FREDERICK BARLOW, Esq., M.R.C.S.

IN THE LANCET of Jan. 11th, 1840, I directed attention to the influence of volition over the excito-motory function of the spinal cord. There is a note by Dr. Baly, in his "Translation of Professor Müller's Physiology," p. 800, which I beg leave to cite.

"According to a third opinion, that of Volkmann, (Müller's Archiv., 1838, p. 32,) and of Mr. F. Barlow, (THE LANCET, Jan. 11th, 1840,) the excited or reflex movements of paralyzed limbs are the result of the unrestrained action of a property constantly possessed by the cord in the same degree, though ordinarily controlled by cerebral influence."

I consider this statement accurate, the words "in the same degree" being omitted: experiment seems conclusively to demonstrate its correctness. I knew nothing of the views entertained by Volkmann before I saw the remarks by Dr. Baly. In reference to the production of reflex actions by experiments on animals, many observers must have come, quite independently of each other, to the conclusion, that the withdrawal of the will is an indispensable step to success in producing them. I dwelt upon it particularly in a lecture which I delivered in 1839, and to which Dr. Marshall Hall made reference in a memoir read before the Royal Medico-Chirurgical Society in 1840.

Mr. Grainger, however, as I have very recently observed, had, before the publication of the remarks of Volkmann and myself, plainly stated that volition can "stop the effect of the impressions transmitted by the incident nerves;" and, speaking of experiment, he instanced the opposite effects of stimuli on limbs connected with the brain, and on those deprived of its influence. (See his work on the Spinal Cord, p. 115, 1837.)

The great Hunter glances at the influence of volition over involuntary action, when he says, in treating of respiration,— "Fresh air was necessary for our existence, and it was therefore necessary that it should be regulated by some other principle than that of the will; for it is necessary when we sleep, and also when we will. Therefore the will has its limits of power over the involuntary actions, and the involuntary have also their limits over the actions of the will; each therefore can only go a certain length in opposition to the other." (Croonian Lectures on Muscular Motion, Palmer's edition.)

The nature of the actions regulated, (for among them are both direct and reflex movements,) as well as the extent and degree to which the will prevails as a restraining, moving, or superseding power, has seemed to me to need pointing out. It was my desire to contribute a little towards this end; for I am persuaded that the strong and widely-pervading energy of volition is very far from being taken into full and accurate account, either by physiologists or physicians. In a recent discussion at the Royal Medico-Chirurgical Society, (March 29th,) Dr. Baly, with his acknowledged impartiality, referred

to the paper of Volkmann; and since then he has been so kind as to translate some passages for me *vivâ voce*. Some of the instances brought forward by that physiologist and by myself are quite similar. I may refer to some remarks upon sleep and the application of them. But (and here I borrow Dr. Baly's own words) "Volkmann refers the prevention of reflex action, in certain cases, not to the will, but to the state of the mind which he calls "attention."

ON THE

NATURE AND RESULTS OF THE PUTREFACTIVE FERMENTATION OF ANIMAL & VEGETABLE MATTERS,

AND THE MEANS OF COUNTERACTING THEIR INJURIOUS EFFECTS.

By PHILIP B. AYRES, M.D. &c., London.

THE investigation of the phenomena of the putrefactive fermentation is of the highest importance in relation to sanitary measures, because these measures must be based either on the removal of the putrefying animal or vegetable remains, or on the arrest of the putrefactive fermentation. The disgusting and laborious nature of such an inquiry, together with its apparent inutility up to the present time, are the causes which have prevented chemists from ascertaining the phenomena with sufficient precision. Still much information has been collected by a number of chemists, which is scattered through the publications of the past century.

Putrefaction can only occur in substances of complex chemical constitution, the elements of which are in a loose state of combination, and retained in union chiefly by the controlling action of "vitality;" and it is necessary that these substances should contain nitrogen as one of their elements. Unlike the salts and other inorganic compounds which have for the most part a binary constitution, organic substances consist of three, four, or even five or six elements in direct union; the force of attraction between elements so numerous is necessarily weak, and they are spontaneously resolved into more simple and permanent compounds. The spontaneous decomposition of organic compounds containing nitrogen, sulphur, and phosphorus, being accompanied by the evolution of foetid gases, has received the name of putrefaction.

The following conditions are required for the development of putrefaction:—

First. The control of the vital force must be removed, or, in other words, the organic substance must be dead.

Secondly. Water must be present in sufficient quantity to permit the free motion of the particles on each other, and their mutual reactions; for organic substances, when perfectly dry, undergo no chemical change.

Thirdly. The temperature must be neither too high nor too low, for putrefaction cannot go on below the freezing point, or at a greater heat than 182° Fahrenheit.

Fourthly. Oxygen, atmospheric air, or at least some gas, must be present, partly to aid in the decomposition, and partly to carry off the gaseous products of putrefaction.

Fifthly. No powerful chemical agent must be present, which, by combining with the animal matter, can produce a permanent compound, and thus control the tendency to decomposition.

When animal substances, such as fibrine or albumen, which consist of oxygen, hydrogen, carbon, and nitrogen, associated with sulphur and phosphorus, are placed under the favourable circumstances I have described, their colour usually disappears, or changes to green or brown; they soften if previously solid, and a foetid liquid exudes; their texture is loosened, and an odour, at first faint and disagreeable, afterwards foetid and almost insupportable, is evolved. If kept in a confined quantity of air, the ammonia developed by the putrefactive fermentation mixes with the putrid odour and somewhat diminishes the foetor; but this is only a temporary change, for if the ammonia be permitted to escape, the putrid odour still remains, and continues, with greater or less intensity, through all the phases of putrefaction. With the progress of putrefaction the softening increases, the liquid becomes turbid, and the whole is resolved into a jelly-like mass called putrilage. Throughout the whole process, the putrefying substance alternately swells and sinks from the accumulation and bursting of bubbles of gas generated within it, and it is the extrication of these gases, and their diffusion, which produce the foetid odour. After the termination of the process of putrefaction, a small quantity of solid matter remains, resembling vegetable mould in appearance, and retaining a slightly foetid odour. The length of time re-