

mortality by virtue of its own merits, or in consequence of defective diagnosis. Some discussion took place in the year 1832, when Dr. Ayre first promulged his plan, and reported his cases, about the accuracy of his statements, and when doctors differ upon the correctness of diagnosis, who shall decide? At this distance of time I certainly cannot undertake to pronounce an opinion. Nevertheless, this suspicion damages the evidence. Granting that Dr. Ayre's cases were genuine cases of the disease, does the calomel exercise a positive influence over the malady, or is it wholly inert? I should not have thought of following out the inquiry in this close manner, if Dr. Ayre had not himself invited examination by repeatedly publishing his opinion that, during the stage of collapse not a particle of the calomel was absorbed, and yet that its efficacy was peculiarly shown during this stage. He mentions, in a communication to the Secretary of the Central Board of Health, the case of a woman, 91 years of age, among others, who recovered from the stage of collapse by calomel, and yet informs us, in the next sentence, that "during the period of the collapse no absorption of it takes place." If this be true, what merit was due to the calomel for this woman's recovery? Either Dr. Ayre's patients recovered from the stage of collapse by the aid of the *vis medicatrix nature* alone, and no thanks to the calomel, or his explanation of its action is unsound. How can a medicine cure that has never entered the system? It is plain, that if the patient has rallied from the state of collapse without or in despite of medical aid, that the claim set up for calomel as a curative remedy is invalid. Dr. Ayre's theory proceeds upon the necessity of exciting a flow of bile, and yet he informs us this result cannot be expected in cases of collapse until the patient has emerged from that state, and is, therefore, virtually cured. I would defend Dr. Ayre from his own theory, and be content with the facts he has stated, if I could be quite satisfied that they were facts. I think that the evidence he has brought forward demands attention, and the institution of new sets of experiments with the view of testing the efficacy of his plan of treatment. I really believe that if not altogether so successful as he states, it will be found far more useful—less dangerous, perhaps, I should rather say—than opiates, stimulants, and other very active remedies. Dr. Ayre allowed his patients to take as much cold water as they desired, and he objected to stimulants. Much of his success might have been owing to this auxiliary remedy.

The treatment of cholera by the administration of large doses of calomel has been also recommended. I have had some experience of this mode of practice in typhus fever, and in the early stages of that disease have found it to exercise the greatest efficacy. In cholera it is said to be equally beneficial, but the facts, as observed in England, do not support the assertion, so far at least as they have been accurately reported. I am reluctant to give too much prominence to opinions, even though emanating from judicious men. Calomel, in one or two scruple doses, acts as a sedative, and tends to arrest the vomiting that is so trying to the patient. Some authors say that the vomiting is a curative process; I shall refer to this hereafter. I have relieved obstinate vomiting in other cases by a large dose of calomel; but it is a dangerous weapon when fever is absent, and occasionally produces excessive salivation. Calomel in large doses does not purge. There can be no doubt that the administration of calomel, in small or large doses, induces a very different therapeutic action; but I do not so clearly see that this difference can obtain when small doses of calomel are so frequently repeated, in accordance with Dr. Ayre's plan, as to throw into the stomach, within an hour, as much as would be introduced if given in a single dose. Small doses may be conceived to have a better chance of absorption; but Dr. Ayre says, that during collapse they are not absorbed. I doubt this, for I have observed, that in typhus fever, at least 24 hours elapsed after the administration of a large dose of calomel, before any change could be observed. An immediate effect should not be expected in cholera. On the whole, it appears to me, that the evidence respecting the utility of calomel in this disease, is very equivocal; and that it is still questionable, whether or not it exercises its specific action to control the disease. Unless as a specific it has no claims; and it is upon this ground alone, so far as I can ascertain, that it has been employed. I have great doubts of its beneficial influence, though I would not discourage further trials, and should be glad to have my doubts removed. It is said, that calomel in large doses, has been wonderfully efficacious in India; but what are the facts; the subject requires examination. In India, small

doses are useless, and large doses cure; in England, large doses are injurious, and small doses cure! Surely a man may be forgiven if he venture to doubt. During the anterior and subsequent febrile stages, which frequently characterise the disease in England, calomel would doubtless be found of use. Under these circumstances, the disease should be treated by the ordinary rules of practice.

*Opiates and Stimulants.*—Any one who has taken the pains to review the history of the epidemic cholera, and carefully to compare the opinions that have been expressed in reference to its treatment, cannot hesitate to condemn the employment of either of these remedies for the cure of this terrible disease. By a reference to the foregoing tables, we find a gradually increasing rate of mortality under these several plans with their various combinations, and it would seem that the mortality, when calomel was employed, was augmented in due ratio as it was combined first with opium and then with stimulants. Under the stimulating system the mortality was highest, with the exception, perhaps, of the routine combination of calomel and opium and stimulants, when the mortality was literally murderous.

*Venous Injection.*—Of venous injection I shall merely, at present, say that its results are not sufficiently propitious to warrant any confidence in its remedial agency.—*Medical Times.*

*On the Acidity and Alkalinity of certain of the Human Fluids in the state of Health and Disease.* By M. ANDRAL. —In their physiological conditions, each of the humours of the body presents a certain degree of acidity or alkalinity; and the spontaneous transformation of a naturally acid fluid into an alkaline one, or *vice versa*, never takes place in the healthy organism. The utmost that can occur in this respect, is the rendering the fluid temporarily neutral by great dilution, as in the case of excessive perspiration—the water then being abstracted from the blood in larger proportion than the other principles. However this may be in health, the opinion is very generally entertained that in disease such chemical change in the humours does often take place; and the object of this paper is to investigate its accuracy.

Of all the fluids of the economy, the *serum of the blood* is the most decidedly alkaline; and whatever the nature of the disease or its duration, in which M. Andral has examined this fluid, he has never found the intensity of this reaction sensibly vary. Vogel quotes a case of *meto-peritonitis* from Scherer, in which the serum of the blood is said to be perfectly neutral, but adds, that he himself had never met with anything similar. If blood is examined after death, any acidity then found is the result of decomposition, and not the effect of disease. In examining the condition of fluids formed from the blood, it should be borne in mind that upon the same surfaces liquids possessed of different reactions may be found; so that the accidental predominance of one of these fluids may easily be mistaken for a change in the reaction of another. Thus the *sweat is acid*, but the *sebaceous matter is alkaline*. In the very various conditions in health and disease, under which M. Andral has examined the sweat, he has found it generally acid, sometimes from dilution neutral, never alkaline; out at the same time, at some parts of the skin, where sebaceous follicles abound, as the axilla and other hairy parts, an alkaline reaction may exist. It is evident, then, that the sweat is not a simple escape of the serum of the blood, charged with certain of its principles, for then it would be alkaline; and if the skin be irritated by blisters and the like, the fluid consequently effused will be found decidedly alkaline. So is the fluid found in herpes, eczema, pemphigus, &c., vesicular diseases preceded by more or less congestion of the skin; and it is remarkable that the contents of *sudamina*, which unlike these are preceded by no congestion, are acid, being also destitute of albumen, which is found in the others. Although *sudamina* are usually accompanied by excessive sweating, cases of typhoid fever are sometimes met with where this is not the case.