the catheter, when that instrument was used for the purpose of relieving the distended bladder. As to anæsthesia, it was present from the nipples downwards. On the arms, however, a strange phenomenon was apparent; sensation was nearly lost as low down as the middle of the forearm, on each side, but below that spot it was preserved as far as the hand, where it again was absent. The patient was ordered to be placed on one of Mr. Luke's elastic beds, the efficacy of which is being tried at this hospital with hitherto very favourable results; it was well calculated to forestall any ill effects of pressure on the spine.

[We may mention here, that Mr. Luke had lately to treat one of those unpromising cases which, with the greatest care, seldom do well,—viz., a fracture of the neck of the thigh-bone in a very aged woman. It was impossible to enforce the necessary amount of uniform posture, to obtain at least partial union; but by using the elastic bed, along with cleanliness, the sad complication of bed-sores was completely avoided, and the woman is on the eve of leaving the hospital.]

Towards the evening, the trifling amount of voluntary motion and sensation remaining in the arms was gradually lost, the bladder continued paralyzed, and priapism persisted. The next day the patient was found very weak, the bronchi became naturally clogged with their secretion, as the expiratory efforts were so feeble. The urine drawn off presented a very high colour; priapism and emission of semen were again prominent symptoms; the breathing became very laborious, and the patient sank about two days after admission.

On a *post-mortem* examination, effusion of blood, surrounding the phrenic nerve, was found between the fasciculi of the scalenus muscles, on the left side, no such appearance being discovered on the right side, and the jugular veins were dilated. For the sake of careful examination, a portion of the spine, comprising three dorsal and four cervical vertebræ, was removed and dissected; a fracture was then seen, commencing in the centre of the body of the sixth cervical vertebra, passing obliquely upwards and backwards towards the articular surfaces, the latter, however, not being fractured, but slightly separated from the articular plates of the fifth cervical vertebra. On removing the spinous processes, the medulla spinalis was found enlarged, and softened opposite the fifth, sixth, and seventh cervical vertebræ; there was no effusion on the surface of the cord, or between the latter and the theca, but on an incision being made near the fracture, a clot of blood was found in the centre of the cord, about the size of a small bean. Apoplectic clots in the substance of the cord are not of frequent occurrence as a consequence of fracture and displacement, but surgical writers have mentioned the circumstance, especially Sir B. Brodie, who says that such an extravasation within the substance of the spinal cord may, according to its situation, be productive of the most dangerous symptoms. The immediate loss of motion and sensation in the lower extremity may fairly be attributed to the concussion the medulla sustained, and the phenomena which took place subsequently—viz., the gradual extension of the paralysis and anæsthesia were very probably caused by the extravasation, disintegration, and softening, which the cord suffered. How slowly motion may be lost by gradual softening is shown in a case related by Dr. Watson in his lectures, where the palsy travelled down the arm so slowly, that the man could write after he had lost the power of raising his arm or bending the elbow.

We see by this case, that an interruption to the healthy action of the cord above the origin of the intercostals, but below the origin of the phrenic nerves, destroys life almost as certainly, but not so rapidly, as when the origin of these nerves is involved. When extravasation, disintegration, and softening, take place, there can be no sort of hope of saving the patient, for the function of respiration suffers so materially, that life cannot continue long. The question, then, naturally arising is, whether attempts at reduction and extension are advisable in fractured spine. We should be inclined to answer this in the affirmative, for the symptoms generally presented in these accidents are not sufficiently pathognomonic as to point out that the spinal marrow is suffering disintegration and softening, or merely compression; therefore it would be hardly fair to the patient not to afford him a chance, by acting upon the presumption that the injury is but slight, and admits of repair. This opinion receives much weight from the fact, that, by pursuing this course, we can hardly add to the mischief already existing. This view is advocated by Mr. Luke, and this gentleman has had occasion to reduce a fracture of the spine, where the effect of replacing the bones was perceived immediately. The fracture was situated near the middle of the dorsal vertebræ, and the patient was extended on his

back, upon a bed, and a firm pillow put under his loins, as also a jack-towel, by the aid of which he was subsequently raised a little from bed during extension. His feet were fastened with bandages to the lower extremity of the bed, and a second jack-towel was carried over the chest, and then under each axilla, to his back. By this, four assistants made powerful extension, while (the loins being raised, as before mentioned) pressure was made upon the sternum. In three or four minutes a crepitus was felt, immediately the deformity disappeared, and the ridge of spinous processes again became continuous. The man expressed himself at once free from pain, without being questioned, and felt sure that the bones were replaced. On inquiry being made, it was found that the patient had more feeling in his abdomen, and a tingling sensation in his thighs, and was now aware when pressure was made on the pubis, motion and feeling having been impaired in these parts by the spinal injury. The man died of erysipelas, and the fracture was found traversing the seventh dorsal vertebra, unattended with any displacement. The medulla was softened opposite the seat of the fracture, and for some short distance from it; by making a longitudinal incision, a small drop of pus escaped from its centre, and, at this part, its whole texture seemed disorganized, so that a recovery from the mischief which the spinal marrow had suffered was not probable, although the means used had removed all pressure from surrounding bone, and had placed the medulla in as favourable a situation as the circumstances of the case admitted. Mr. Luke very justly remarks, that this case shows the feasibility of the practice adopted, and the capability we possess of removing pressure from the medulla; but it also shows, that a mere replacement is not sufficient for the recovery of the patient, while the injury which the spinal marrow has sustained at the time of the accident continues unremedied. If, however, pressure on the medulla should take place, without material injury to its organization, it is not at all improbable that the practice here pursued would be attended with effectual relief.

Medical Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

TUESDAY, JAN. 22, 1850.—DR. ADDISON, PRESIDENT.

CHEMICAL RESEARCHES ON THE NATURE AND CAUSE OF CHOLERA.

By ROBERT DUNDAS THOMSON, M.D. Glasgow.

(Communicated by SIR B. C. BRODIE, Bart.)

IN the first part of the paper the author details the results of chemical analyses of the blood, urine, and intestinal discharges, in the cold, or "lymphatic," stage of cholera; and in the "biliary," or febrile stage. The main results arrived at are—1. That in the cold stage of the disease the specific gravity of the blood, and of the serum separated from the clot, is increased; that the proportion of water is less than in health by at least nine per cent., and in some cases by as much as seventeen per cent.; that both the organic and the inorganic components of the blood are proportionally increased in amount; but that the increase of the insoluble salts is much greater than that of the soluble salts. 2. That the intestinal discharges, in the cold stage, when of the true "rice-water" character, resemble closely, in their chemical composition, the fluids of hydrocele and hydrocephalus; that their flocculi are formed of epithelial scales, and the watery part of water, containing a small proportion of organic matter (albumen) and salts, (chloride of sodium, carbonate of soda, earthy phosphate, alkaline sulphate, and some lime.) 3. That the small quantity of urine sometimes found in the bladder, in this stage, presented no apparent aberration from an ordinary standard. 4. That in the biliary, or febrile stage of cholera, the blood soon regains its normal proportion of water, or even an excess of it; and that the other constituents resume their natural relation to each other. 5. That the urine, in the biliary stage, in several cases contained albumen, but presented scarcely any other deviation from the urine of health, except in the amount of urea, which at first was deficient. In the second part of the paper the author describes some experiments, instituted by him, with the view of determining whether any poison could be detected in the atmosphere. In one series of experiments it was ascertained that no solid matter existed in the air; but ammonia was obtained from it in the proportion of 0.319 grains of caustic ammonia, or 0.731 grains of carbonate of ammonia to 1000 pounds of air. By another series of experiments it was determined that no carbon or hydrogen

existed in the atmosphere, except in the states of carbonic acid and water; while carbonic acid was obtained in the proportion of one volume to 6650 volumes of air. In his concluding remarks, the author argues that the cause of cholera is not a specific, tangible poison, introduced into the body from without, but rather a vicarious transference of the cutaneous excretion to the intestinal mucous membrane, dependent partly on an atmospheric influence, and partly on a predisposing state of the system, in those who are affected with the disease.

Dr. SNOW said, that the analyses of the blood and evacuations, detailed in the paper, confirmed those previously made by Drs. Garrod and Parkes in almost every particular. The experiments on the atmospheric air, having been very carefully conducted, afforded additional proof that cholera did not depend on a poison diffused in the atmosphere. The author of the paper had given an account of the fanciful reasons which had led medical men to attribute cholera to the presence of a poison diffused in the atmosphere, but he did not seem to have emancipated his own mind from the atmosphere as a cause of the disease; for although he did not attribute it to a special poison existing in the air, he supposed that it depended on some condition of the air which interrupted the cutaneous and pulmonary exhalations. Now, the physical conditions of the atmosphere, which influence the functions of the skin and lungs, were accurately known, and if the state of the air was such when the cholera prevailed at Glasgow, being cold and foggy, as to check the cutaneous transpiration, it was just the reverse in London, in August and the early part of September last, being warm and, for the greater part of the time, dry. The truth was, that the cholera prevailed at one place or other under all conditions of the atmosphere. The analyses of the blood showed that it was so drained of its water, that it must be rendered thick and not capable of circulating freely through the capillaries: this state of the blood he (Dr. Snow) considered was the cause of the collapse, and the chief symptoms connected with it. Dr. Thomson had drawn a parallel between cholera and influenza. He (Dr. Snow) considered that both these epidemics were propagated by a poison generated in the human body, and that the poison was in each case applied to that mucous membrane which was the chief seat of the disease. Influenza was in the last century considered to be contagious, but latterly it had ceased to be looked on in that light; and as it bore, in the progress of several epidemics, a certain relation to cholera, this analogy was used as an argument that cholera was not contagious either. It was said that influenza spread too rapidly to be contagious, but if it was communicated by the breath, from one person to others, this difficulty disappeared, for influenza did not spread so swiftly as a piece of bad news, also communicated by the breath. As the cholera poison had to be swallowed to produce its effects, this disease necessarily spread more slowly; and although the communication of cholera could, in numerous instances, be traced, yet, in many others, from the very nature of the method of communication, it could not. But it was found that it spread most extensively where there were the greatest facilities for the swallowing of the excretions. Thus, want of personal cleanliness, which was well known to assist the spread of cholera, rendered those about the sick person liable to partake of the evacuations along with their food, from their unwashed hands; and some recent tables, published by the registrar-general, had confirmed the statement, that the epidemic had been most fatal in those districts of London supplied with water obtained from the Thames in the neighbourhood of the chief sewers, and which must, when cholera is prevalent, contain the evacuations of the patients. There were also many spots where the malady had been very prevalent, in connexion with the contamination of local supplies of water. For instance, it had been very fatal in the neighbourhood of Bridge-street, Blackfriars, around which spot the inhabitants used to send to St. Bride's pump for their drinking water; and this pump had since been closed, at the instance of Mr. Hutchinson, surgeon, it having been ascertained that the well was contaminated, by a sewer running into the Fleet-ditch. The cholera, in many places, had been so circumscribed within certain limits, that it was impossible to attribute it, in any way, to the atmosphere, which, together with all the conditions connected with it, was extremely moveable.

Dr. COPLAND was astonished that the analyses made by Dr. Thomson were not more numerous, considering that the experiments were undertaken by direction of that redoubtable body, the Board of Health. He thought the experiments were too few to draw any definite conclusions from. For instance, it appeared that the experiments on the blood were confined to three patients, and yet no account of the previous

state of health of those persons was given. Looking at the difference which often existed in the blood of individuals, the experiments were incomplete. (Dr. Baly said the experiments were made on eight persons, three being the mean.) No statement was made regarding the state of the secretions of the patients previous to the disease; and though the faeces had been analyzed, no account of the medicines taken had been given. No allusion had been made in the paper to the fluid coating the mucous membrane of those who died in the stage of collapse. The paper, so far as it went, however, confirmed other experiments. It appeared, also, that the atmosphere of one room only had been examined, that room being in the University of Glasgow; the experiments on this point, therefore, were merely negative. One would have supposed that the atmosphere of the disease-ward would have been examined, or, at least, the sick-room, containing the emanations &c. of the patient. In all the particulars alluded to the paper was a complete failure, and furnished us with no information, or, if any, of a negative kind. Dr. Copland then made some remarks on the speculations regarding the cause of the disease as stated in books and pamphlets by the emissaries of the Board of Health, and considered them of little weight, inasmuch as they were mere evidences of a foregone conclusion respecting it. That Board, however, had lately modified its opinion, and was now in favour of the doctrine of contagion. He then made a statement regarding the evidence adduced by the Board of Health commissioners, and mentioned one instance in which cholera was stated to have made its appearance spontaneously in an hospital; whereas the fact was, that three persons had been brought in with the disease; two other persons caught it. These facts had been suppressed.

Dr. BALY explained, with reference to Dr. Copland's remarks, that the author's words were: "The following investigation was commenced at the instigation of the Board of Health;" and that the experiments on the atmosphere consisted of two series, one occupying nine days, the other ten days. Dr. Baly then remarked that the author's hypothesis of the cause of cholera was not supported by the phenomena of the disease. For not only would the suppression of the cutaneous and pulmonary exhalations, and the transference of the fluids normally excreted by the lungs and skin to the intestinal mucous membrane, be inadequate to produce the enormous discharges which drain the system of its fluids; but it is a fact that the cutaneous exudation is, in many of the worst cases of cholera, greatly increased, a watery fluid being in these cases poured out by the whole surface of the skin, as well as by the mucous membrane of the intestines. The draining away of the watery part of the blood was certainly the main phenomenon of the disease, all, or nearly all the others being secondary to and dependent on it. It undoubtedly was the cause of the coldness of the body. But it prevented the development of animal heat; not, he thought, by rendering the blood thick, and thus incapable of circulating quickly, as suggested by Dr. Snow, but by rendering impossible those chemical changes which normally go on in all parts of the capillary system, and which are the source of the animal heat. The circulation in cholera is undoubtedly much retarded; but the rapidity with which the nervous system of the patients is affected by chloroform inhaled into the lungs, shows us that the motion of the blood would still be sufficient to allow of some evolution of caloric, if other necessary conditions were present. But an essential condition for the normal processes of nutrition, and the chemical processes on which the temperature of the body is dependent, seems to be the presence of a certain proportion of water in the blood, and it is most probably because this is wanting that the body becomes cold. To the same cause are obviously to be ascribed the arrest of the natural secretions of the liver and kidneys, and the depressed state of other organic functions of the body. And even the remarkable condition of the mind, and of the sensitive and motor functions—their state of comparative integrity—not only affords evidence that the nervous and muscular systems are in a great measure independent of the watery part of the blood, so essential to the organic functions, but is itself a result of the loss of that part of the circulating fluid. Under ordinary circumstances, if the respiration continued so imperfect, and the secretion of urine were suspended for a long period, as they are in the cold stage of cholera, the blood would become charged with carbonic acid and urea, and coma would result. In the cold stage of cholera the natural chemical processes of the body are brought nearly to a stand-still by the loss of the water of the blood, and neither urea nor carbonic acid is formed in large quantity; the powers of the mind and of voluntary motion are therefore left comparatively unimpaired. Dr. Baly concluded his

remarks by referring to some interesting observations which had been made by Mr. Frederick Barlow, on two other remarkable phenomena of cholera—viz., the muscular movements and elevation of temperature after death.

Mr. W. F. BARLOW said, in reference to the cause of cholera, that he could not discover how failures to demonstrate any poison in the atmosphere could properly be construed into inferences of its non-existence. Probably it was so subtle as to defy all scrutiny. Could the poison of small-pox or scarlatina be demonstrated in the atmosphere? Yet, was there any doubt that the air could be made infectious by, and could communicate, those diseases? Looking at the lateness of the time, he would only just touch on one or two matters respecting which Dr. Baly had done him the honour of suggesting that he should make a remark. He had paid some attention to the state of the muscles in cases of cholera. Nothing in that disease was more marked than the extreme excitability of the muscles during life, and their extraordinary manifestation of irritability after death. The facts which related to these points were most obvious, but as to the causes of them, there were so many difficulties in the way of satisfactorily accounting for them, that he begged the few words which he had to offer might be viewed, as stated, in a suggestive, not a positive, manner. The condition of the blood must be considered, if we would even approach a rational conjecture on the state of the muscles. It seemed pretty clear that there was a great defect in the formation of carbonic acid, in cases of cholera, and therefore in the production of animal heat. He had listened with interest to what had fallen from Dr. Baly, respecting the failure of chemical changes in the blood, and their consequences. There appeared a broad difference between death in the collapse of cholera and death by asphyxia, as ordinarily considered; in the latter, noxious blood circulated, and impaired the irritability; but in the former, carbonic acid could not be carried freely through the system, or coma would follow, and the heart's action would cease even more quickly than it was observed to do. The state of the muscular fibres after death reminded one of what was seen in animals destroyed by hæmorrhage, a case in which the muscles were simply in great part deprived of blood, not poisoned by it. A great distinction was to be drawn between those instances wherein, before death, impure blood freely circulated through and damaged the muscular fibres, and those in which it did not, or acted to a much less extent. Why muscles should contract as they did after death by loss of blood was difficult to explain, but it was a fact that they did so, and by the side of this should be placed instances, if not altogether, at least somewhat similar. The suddenness of dissolution, on the contrary, was always to be borne in mind in regard to post-mortem contraction. The strange elevation of temperature which happened not rarely after death by cholera was another most interesting matter. How was this to be accounted for? He had heard it said, that it might possibly result from the degree and violence of the contractions. But these, he thought, perfectly inadequate to the explanation of the phenomenon; and such an opinion was made altogether untenable by the fact, that there was, not seldom, great heat where there were no muscular movements whatever. Dr. Green had kindly communicated to him the particulars of a case which had occurred at Bristol. It was remarkable, from the long retention of heat after death, and excited no little public sensation during the late epidemic. In that instance there were no muscular contractions seen. He could not help thinking that changes might occur in the blood after death, such as usually happened during life-time, and so cause maintenance or increase of heat. Might not carbonic acid continue, in certain cases, to be formed awhile? He spoke only suggestively, but he had as yet heard no satisfactory explanation of the fact why a body cold, before death should become not only warm but unusually warm afterwards. Dr. Dowler, who had made some instructive observations on yellow-fever subjects, said, that in one case the heat of the dead was, "for seven hours, from 102° to 111°." That poisons would continue to take effect long after the cessation of the respiration and circulation, was shown by an experiment, which was published in the *Philosophical Transactions* for 1811, by Sir Benjamin Brodie, who found that an animal poisoned by infusion of tobacco exhibited spasmodic movements half an hour after it had ceased to breathe.

Dr. WEBSTER remarked, in reference to the effect of atmosphere alluded to by the author in the production of cholera, that an instructive illustration of its action came within his knowledge during the recent epidemic in London. It occurred at the Knightsbridge-barracks, where the Life Guards were then quartered. Cholera having broken out among the men, of whom some died, the regiment was therefore moved

to Canterbury. Immediately afterwards, not a single new case appeared, and the regiment has ever since continued free from the disease. However, several wives and children of the soldiers having slept in the barrack dormitories after the men's departure, two individuals among them were attacked by the epidemic, which evidently was produced by the local atmospheric influences affecting the men previously.

Dr. BASHAM wished to recal the attention of the fellows to the analyses of the blood in cholera detailed by the author. He did not collect from the paper, that the different salts were specified in their proportions given, but the author inferred that the total saline ingredients was greater than in health. 8.56 parts in 1000 were given as the mean of several analyses; and this proportion was compared with Lecanu's standard of six parts in 1000, as the mean of healthy blood. It was generally admitted that Lecanu's standard was below the mean; Simon had placed it as high as about eleven; and if the mean of many analyses of healthy blood by various chemists—Enderling, Marchand, Becquerel, Simon, and our own English chemists—were taken, the average proportion of salines in 1000 parts would not be below 11.5 to 12. He had never found the saline matters of healthy blood, on an average, lower than 10 to 10.50. Individual cases would occasionally present a lower proportion. In two cases of cholera-blood examined last August, the saline matters were considerably below the mean; this was also the case with regard to the fibrine, and these results agreed with Simon's analysis. The author had, however, estimated this element above the average, but he did not state whether this increase was absolute or only relative. The deficiency of water in 1000 parts would cause an apparent increase in the whole solid contents; but was the fibrine augmented in relation to the amount of albumen and blood corpuscles? He (Dr. Basham) was surprised that no reference was made to the presence or absence of urea in the serum of the blood; he had obtained it, in several instances, during the late epidemic, from the blood both during life and after death. The statement, that there was no departure from the natural standard of the urine in a small quantity taken from the bladder, was very contrary to the results of numerous examinations of the urine made during the prevalence of the disease. In every case, the fluid found in the bladder was more like an alkaline solution of albumen than urine; it contained no urea, no phosphatic salts, traces only of chlorides; and these qualities were observed even in the urine of those who, for many days after the collapsed stage, passed scanty urine, but who ultimately recovered.

In reply to Dr. Basham, Dr. Baly said that the author had given a statement of the proportional quantities of the different soluble salts in cholera blood, and had also quoted Enderlin's and Marchand's "Analysis of Healthy Blood," as well as Lecanu's.

Dr. COPLAND regarded the heat of the body after death as the result of the incipient decomposition. In all cases of death from cholera, the stomach and intestines were easily torn. This was also observed in cases of yellow fever, and in the malignant form of puerperal fever. In these diseases there was a great increase of temperature after death, and the tissues were easily torn.

The next meeting of the Society will be held on Tuesday, the 12th inst.

PATHOLOGICAL SOCIETY OF LONDON.

MONDAY, JANUARY 21, 1850.—Dr. LATHAM, PRESIDENT.

Mr. PRESCOTT HEWETT presented a well-marked specimen of ANEURISMAL DILATATION OF THE LEFT AURICLE, WITH THICKENING AND CONTRACTION OF THE LEFT AURICULO-VENTRICULAR OPENING.

The dilated auricle, of the size of a large orange, presented, when cut into, the appearance of an aneurism, its cavity being filled with firm, fibrinous coagula, of a grey colour, and for the greater part distinctly laminated; towards the centre, however, the coagula were soft and broken down, but still quite distinct from the blood, which had coagulated in the auricle since the death of the patient. The layers of coagula, in contact with the walls of the auricle, were adherent to them in several places, and after their removal several small spots of calcareous matter were found deposited under the serous membrane of this cavity. The mitral valve was much thickened, and the auriculo-ventricular opening contracted, so as only to admit of the middle finger being passed through it. The fossa ovalis, which had assumed a round shape, was of the size of a half-crown piece, and its membrane, very much attenuated, was nearly transparent. The right auricle