STUDIES PHYSIOLOGICAL AND PRACTICAL.

By BENJAMIN W. RICHARDSON, M.D.

SUGGESTION FOR THE TREATMENT OF CHOLERA IN THE STAGE OF COLLAPSE BY THE ARTIFICIAL PRODUCTION OF PERITONEAL OR CELLULAR DROPSY.

[Report read before the East Surrey Medical Society for the Observation of Cholera, August 22nd, 1854.]

Up to the present time, the treatment of cholera has been, as a general rule, attempted in a manner so purely empirical, that when the Committee, appointed by this Society to report on this important subject, entrusted to me the physiological department, I felt myself placed in a position in some respects novel and serious. Having undertaken the duty, however, I have lent to it my best attention; and, at the request of the Committee, I now place before you the results of my investigations up to this date.

As a physiological study, the matter placed in my hands develops itself in various directions. The conditions that would give rise to the symptoms and effects of cholera furnish one point for investigation; the discovery of some potent measure for the arrest of those symptoms, another; not to mention others of vast interest or importance. But the point which has as yet most occupied my attention, and to which I wish to direct yours, is how to refurnish the system with the amount of fluid which it has lost, in cases where the patient lies in that desperate condition, called the stage of collapse.

And at the outset, I would state, that the more I have considered the matter, the more thoroughly have I been confirmed in favour of the now generally received opinions, that the symptoms of collapse, one and all, are due to the loss of fluid which the body has sustained; that the poison of cholera, whatever it may be, has, like a dose of a violent purgative, a limit to its action on the body; and that, if a patient, suffering from the disease, can himself sustain the loss caused by the dose of the poison he has received, or if he can be sustained artificially from that loss, his recovery is to a great degree secured.

In looking at the effects of loss of fluid from the system, we must not confine ourselves to the idea that a very large quantity of watery matter must of necessity pass away before serious symptoms shall result. It is true, that in some diseases, as diabetes and consumption, a great amount of fluid escapes without giving rise to results immediately fatal; but in these diseases, it must be remembered that the loss is gradual, and that the system for a time bears up against it, except in the last stages of consumption, when a rapid diarrhoea often terminates the scene: while in cholera, the main loss is so immediate, that the body is drained in a few hours, instead of weeks or months, thus affording no time for the commencement of those conservative processes which nature sets up in other diseases of the exhaustive type.

I say that a rapid removal of fluid must not be considered always in regard to quantity, because all physiological learning goes to prove that a certain balance between the densities of arterial and of venous blood must be kept up, to secure for the system many processes upon which its existence directly depends. The experiments of Dr. John Davy, and of other not less distinguished physiologists, have shown that, under all conditions of climate and habit, the venous blood of the healthy man is denser than the arterial; and the labours of our learned and excellent President prove to demonstration that upon this difference of density rests those osmotic changes, by which the transudation of the effete matters of the body into the returning or venous current is effected.* Now, in cholera, as the serous loss must be derived mainly from the arterial circuit, it is obvious that a very few copious discharges from the stomach or intestines lead quickly to the destruction of the balance that ought to exist, speedily arrest the course of the circula-

tion, and of necessity interfere with the respiratory process, the generation of animal heat, the functions of the nervous system, so far as they depend on the blood, and with every other process which we consider essentially vital.

Viewing the matter in this sense, the loss of fluid in cholera resembles closely the loss of blood itself. When blood is drawn largely and suddenly from the system, there is a rush of fluids from the various parts of the body to fill up the deficiency; and the collapse and muscular shrinking that result, arise in all probability from this cause. So in cholera, where the hæmorrhage is watery merely, the same results ensue; but, as the loss is continued instead of temporary, and as a portion only of the required constituents of the blood is removed, the symptoms of collapse are less rapid, though not less certain, in their course. In a patient, therefore, who has suffered some hours from the discharges of cholera, recovery is even more remarkable than dissolution, since every structure of the body has yielded up more or less of that fluid constituent upon which its shape, softness, and mobility so much depend. Hence, doubtless, the reason why the aged, in whom the body is shrivelled and already in part dried up, sink from the disease so much sooner than do those who suffer from it during the prime and vigour of life; and hence, also, the cause why young persons, suffering from a fatal attack of the disorder, become in a few hours such aged-looking, shrunken, and almost mummified objects.

In addition to all this, the elimination of water from other excreting surfaces than the bowels is of necessity suspended. The kidneys fail to secrete, and the saline substances which should be thrown off by them are retained; though how far this retention modifies the course and result of the case is as yet uncertain.

Now, in observing a patient in the collapsed stages of cholera, and bearing at the same time leisurely and calmly in the mind those modified physiological conditions which I have attempted to sketch out, and which are then going on, one sees at once why so much of incessant failure attends many of those remedial measures which are commonly adopted to rally the sinking system; in plain words, one learns negative practice, which in doubtful treatment is often one of the first and best steps at which the mind can arrive.

Thus we see that the application of external warmth over and above the natural animal temperature, and for the purpose of increasing the heat of the patient, is a proceeding at once useless and unphilosophical; since the body does not absorb caloric from without, but makes it from within, and diffuses it externally. And, as the coldness of cholera arises solely from the fact that the internal chemical process, by which heat is produced, is arrested, it is obvious that the only means by which a new supply of heat can be obtained, will be by endeavouring to rekindle the natural calorifying influence. It is of course strictly physiological to surround the body of a cholera patient with a temperature somewhat above the ordinary point of animal heat, or by a non-conducting material; for this is a conservative step, tending to retain such small amount of caloric as may be generated within; but to do more—to heat the atmosphere of the room, to shut off currents of fresh air, without which respiration is a nullity—is a system directly opposed to our knowledge of first physiological laws.

Again, no one who has observed cholera much, can fail to be convinced of the frequent inutility of drenching a patient with large amounts of simple liquids or medicines, during such times as copious vomiting and purging are going on. For there can be little or no absorption by a membrane, at a time when it is pouring out excreted matters from every point of its surface; nor can we suppose that, even in the last stages of the disease, when from sheer want of fluid to exude, the watery evacuations have ceased, the membrane from which they have all proceeded, and of whose epithelium immense portions have been washed away, is in a very happy condition for the imbibition of substances, medicinal or otherwise. And, indeed,

* On the Special Function of the Skin. By R. Willis, M.D. ASSOCIATION MEDICAL JOURNAL, June 30th, 1854.

we find in practice that even the most powerful styptic remedies prove of little avail in arresting the discharge, unless given in that early stage of the disease, when their retention is secured.

As, then the loss of fluid from the system is the cause of the collapse, as in many cases it cannot be replaced by the natural process of imbibition from the intestinal surface, and as the blood cannot circulate and reaction occur, without such replacement, there is open before us these plain and simple physiological problems—Are there any other means by which fluid can be introduced into the exhausted circulation? and, if so, which of these is the safest, and most calculated to effect the object desired?

We have seen that the mucous membrane of the alimentary canal, while carrying on an extensive excretory process, cannot invariably be made a medium for absorbing fluid to any great extent. Some practitioners have, however, believed that the administration of large quantities of fluid by the mouth, during the intervals of rest from vomiting, has been attended with benefit; and there surely can be no objection to a plan so reasonable, unless the swallowing process should itself excite vomiting, and especially as patients themselves almost invariably crave urgently for drink.

By other practitioners, and by one set of charlatans, the external application of water to the body, has been advocated; and this plan is also reasonable to a certain extent, for it is well known that, at a push, the skin will take on a modified form of absorption. Thus the body has been found in some degree to be nourished by the immersion of its parts in nutritive fluids, in cases where admission of liquids into the stomach is rendered impossible by disease: and in the voyage of Captain Bligh, after the mutiny of the *Bounty*, when the sailors were without drinking water for many days, and were exceedingly exhausted, their pangs of thirst were remarkably assuaged by dipping their clothes into the sea, and afterwards wrapping them round their bodies. I do not know from experiment to what extent the skin will absorb water; but it is quite certain that in cholera, where the loss is so sudden and extensive, and where a supply equal to the loss is required, no amount of immersion could be expected to do marked good, to say nothing of the inconvenience of such an application in cholera cases.

The most important step that has as yet been made on the human subject towards supplying the exhausted system with fluids in cholera, is that of throwing a considerable quantity of fluid directly into the circulation through the veins: and it cannot be denied that, in practice, this plan has been attended with a some amount of success. That it should not be generally successful, is, I think, strictly explainable on physiological grounds. In the first place, the plan is objectionable on the mechanical argument, that to throw a large amount of fluid through a vein directly into a heart itself exhausted, is sufficient to paralyse that organ to a serious degree; for I have found, by direct experiment, that a weakened heart often succumbs instantly from over-distending it with blood itself. To meet this difficulty, I have sometimes thought that a long narrow syphon tube, with one end fixed in the vein, and the other immersed in the fluid, would be much better than the syringe; since the suction power of the circulating organ would gradually take in from the reservoir the required element. I throw this out merely as a suggestion, unsupported as yet by experiment.

Another objection to the direct transfusion of water, or of water impregnated with saline or other materials, into the circulation, is, that such fluid is not the proper stimulus for exciting the heart to contraction; and that, even if the heart does contract upon it, and throws it round the pulmonary circuit, it is, again, not a substance upon which the process of respiration can properly act. When a saline substance, injected into a vein, accomplishes anything, it does so, I imagine, by dissolving the half-clotted blood in the cardiac cavities, and by carrying the blood, thus diluted, round the lungs. The mention of this idea indicates how

slowly fluids should be introduced into veins, and supports the notion of the syphon tube, alluded to above.

But the last and main obstacle to the success attending injection into a vein, is, that by this process no such competent amount of fluid can be introduced so as to produce a permanent effect: and, in truth, we too often find in practice that, after such injection, the patient only rallies for a brief period, to sink again under a continuance of the discharge from the intestines. I do not know what is the largest amount of fluid that has ever been injected into the vein of a cholera patient; nor yet the largest amount of fluid that has been known to be lost during the disease. Each of these questions ought at once to be ascertained, and might easily be so: the first, by each practitioner keeping a careful record of his treatment of cases; and the second, by weighing patients at the commencement and end of the disease, and by calculating the loss of weight sustained. In hospitals, this last fact could easily be learned, without teasing the patient, by having beds made as weighing machines; taking account, of course, of the alterations in weight produced by clothes or other appendages. Speaking from general observation, however, I may say that the largest amount of fluid I have ever heard of as having been injected into a vein in the course of a case is from seven to ten pounds, and the most I have ever seen injected is from three to four pounds; while I have known a cholera patient to pass off by one vomit from two to three pounds, and repeat this many times in the course of three or four hours. I speak on this matter with great caution, and strictly subject to correction; wishing only to convey to the mind the idea that, by the ordinary processes now in use, no sufficient quantity of fluid can be injected by a vein to make up a tithe of the serum lost, which is the point to be desired. I make no mention of any of the risks that may arise from injection into a vein, such as introduction of air or phlebitis; because these, though important in themselves, are of secondary consideration in extreme cases of cholera, when compared with the main object of diluting the blood and re-establishing the circulation.

In contemplating the subject of transfusing watery matter into the system after the modes I have described, the question occurred to me, whether we ought to confine ourselves to these modes; or whether any other means existed of introducing fluid matters into animal bodies in very large quantities, and in a manner which should secure their absorption. This thought led me to look back upon the pathological characters and treatment of those diseases in which serum is largely thrown out into serous cavities or cellular tissue. I recollected that, in cases of this class (dropsical cases), not only were several gallons of fluid often thrown out of the circulation into the system without immediate danger, but that patients thus situated could tolerate a degree of purgation which would absolutely destroy healthy individuals. This immunity must arise solely from the fact that the effused serum lies as a reservoir, from which the circulation is fed while the purging is going on. The idea furnished an important suggestion; and I consequently commenced a series of experiments on animals, which, as they are as yet in an imperfect state, I shall only describe in general terms, reserving the particulars for my next report.

The experiments up to this time have mainly consisted in exhausting dogs and cats by starvation and violent purgation with large doses of elaterium, and afterwards injecting either their peritoneal cavities or their cellular tissue with large quantities of distilled water, at the blood temperature. The results, in general terms, are as follows.

I find that either into the peritoneal cavity, or into the cellular tissue, a quantity of water, varying from a tenth to a fifth part of the weight of the animal may be injected with little risk. The effect of this is to induce a sleepy condition, which lasts from twenty to thirty hours, long before which time all trace of the injection is lost. If more than this is thrown in, the sleep, or rather torpor (for the animal only remains quiet, and rouses when spoken to), ends in death. This was the case in one dog

weighing ten pounds, into whose cellular tissue I injected three pounds of water. In animals that have been much exhausted, more fluid may be injected, and the absorption is greatly quickened. In animals which have died from the experiment, and have been examined immediately after death, I have never found any trace of the injected liquid; but the blood has always been exceedingly fluid, and has coagulated slowly, if at all. I should notice also, that in two cases, in which a cat and a dog were operated on while under the influence of an anæsthetic agent, they died; the one in four, and the other in forty-eight hours, without ever rallying fairly from the effects of the narcotic. I should mention, too, that I have met with one or two casualties in the course of these experiments, which have arisen purely from their novelty, and cannot be considered as bearing on the merits of the system. Thus, in two young cats of the same age, I injected a sixth part of their respective weights of water into the cellular tissue of one, and into the peritoneum of the other; the result was, that the one whose peritoneum was injected, after remaining apparently well until three or four hours previous to death, died about twenty-four hours after the operation. On opening the body, all trace of fluid was absent; but I found that, from having made too large an incision into the peritoneum, and from the animal having reclined on the belly, about two inches of intestines had slipped into the opening, and had become strangulated, and deeply congested. In another instance, where I inserted pins into two openings, erysipelatous inflammation came on in the surrounding skin; but not in another part of the body, where the wound was left untouched. In a third casualty which occurred to me, I learnt a very useful lesson. Into the peritoneum of a dog three pounds of water were thrown, and one pound into the cellular tissue of the thigh. The animal seemed pretty well for a short time after the operation, and the fluid was absorbed in the course of three or four hours, although it constituted a fifth part of the weight of the whole body. In a little time, the creature gradually became exhausted, and died in eleven hours. On opening the body, I found that all the fluid injected had disappeared; but in the peritoneum there lay about a pound of semi-coagulated blood, which had escaped from an artery that had been wounded by the trocar. The blood in the veins and heart of this dog was like water itself, and showed no tendency to coagulate. The heart was flaccid and collapsed. In this case, the rapid absorption of the injected fluid arose doubtless from the hæmorrhage, and from the too rapid and copious flow of fluid into the exhausted circulation. For, while the blood lost its solid constituents on the one hand, it gained too large an amount of water on the other. This animal had been reduced somewhat by purgation. Previous to the operation, he weighed twenty-six pounds; immediately after it, thirty pounds; and, at his death, a little more than twenty-four pounds. He must consequently have made use of all the injected fluid, and have lost somewhat more than one pound additionally, during the interval between the operation and death.

Setting aside, therefore, these casualties, I infer from my experiments as they now stand, that into the peritoneum or the cellular tissue of a patient in a proper state of collapse from cholera, water to the extent of at least a tenth, or even a fifth, part of the whole weight of the body might be injected with safety; and that the absorption would be almost immediate.

The performance of the operation for producing this artificial dropsy is exceedingly simple. The instruments required are simply a scalpel, lancet, or trocar, a common catheter, and an ordinary enema syringe. If the cellular tissue is to be injected, a deep puncture is made with a lancet in some fleshy part, the abdomen is the best, and the blunt point of the catheter is then introduced, and wedged sufficiently far into the cellular substance to allow the skin around it to be firmly grasped, so as to keep it steady in its place. The other end of the catheter being connected with the enema pump, an assistant very gradually throws in the fluid, and, as the tissue distends, gentle compression may be made with

the hand so as to force on the fluid under the skin. Into the leg of a dog of middle size, a pound and a half of water may thus be injected in a quarter of an hour; and the experiment may then be recommenced at some other point if desired.

For puncturing the peritoneum, I am now having made a special trocar, which I will describe at a future time; but the scalpel or lancet, if used with care, answers exceedingly well. A careful plunge into the cavity is made in the linea alba; and the blunt point of a catheter is introduced, and grasped as before mentioned. Fluid may in this case be thrown in with much greater rapidity for a considerable time; but I have observed that, as the body distends, the action of the diaphragm becomes somewhat interfered with, upon which the operation should cease for the time. I have injected three pounds of water into the peritoneum of a middle sized dog before producing this effect. The wound must be carefully closed with stitches; but a little of the water will always escape, and become diffused in the cellular tissue of the abdomen.

The steps of the operation in the human subject would not vary materially from those I have already described. The opening into the peritoneum should be made with a small scalpel through the linea alba on the left side, about two inches below the umbilicus: the blunt end of the catheter should then be introduced and steadily grasped, the fluid being meanwhile made to pass slowly through it, so as to prevent the injection of air. The freeness with which the fluid flows through the tube will show whether or not the peritoneum has been freely entered. For injecting the cellular tissue, some point of the body should be selected where the parts are loose, such as the abdominal parietes, or the back; in these parts the cellular substance is extensive, and would offer but little resistance to the passage of fluid.

I am not at this moment able to say which of the two kinds of operation is best, but I am strongly inclined in favour of injecting the peritoneum; for, although the absorption of fluid from the cellular structure seems most rapid, the difficulty of introducing water in full quantities into the peritoneal cavity is much less, and the pain attendant is considerably milder. In either case, too much care could not be taken to throw the fluid in slowly, to have it at a temperature not less than 92° Fahrenheit, and, if the peritoneum is the part injected, to be mindful not to distend the cavity to the extent of interfering with the action of the diaphragm.

As yet I have had no favourable opportunity of trying the suggestion I have here thrown out in cholera; but that it might be put into application in desperate cases and might prove advantageous, I have no doubts whatever. The advantages of it would be:

First. That an abundant store of fluid would be supplied, which the exhausted circulating system would rapidly and effectually take up.

Second. That as this imbibition would take place through the capillary system, the fluid would enter the veins freely charged with the solid constituents of the blood, and would pass to the lungs in a condition suitable for respiration.

Third. That the process would in no way interfere with the administration by the mouth of astringent solutions or such other medicines or liquids as the practitioner might consider indicated.

But, although I am thus sanguine on this subject, I wish it to be understood that the first trial of this process should commence in a case where the patient is in the last stage of collapse, where other remedies have failed, and where death is imminent. This, it is true, would scarcely be putting the new remedial measure to a fair test; but my ideas of the value of life are so great, that I would not, even in so important a disease as cholera, recommend the employment of a new remedy wantonly, or without that due care for the welfare of the sick man which every honest practitioner of medicine intuitively feels.

The subject once started, branches off in various ways, and opens many points for investigation. If simple water

can be taken into the system through the cellular tissue or peritoneal membrane, it remains to be ascertained whether nutritive or medicinal substances, which could not obtain admission by any other way, might thus be introduced. It is interesting and important to know, that the peritoneum has been found capable of absorbing some medicinal agents. I am informed by Dr. Cormack, that many years ago, he, and the late distinguished Professor of Anatomy at St. Andrew's, Dr. John Reid, threw into the peritoneal cavities of dogs various medicinal substances; and that the proofs of the absorption of these substances were speedily manifested. Dr. Christison, also, in his *Treatise on Poisons*, p. 29, thus remarks: "The serous membranes possess an activity of absorption which hardly any other unbroken texture can equal. Accordingly, many poisons act much more rapidly through the peritoneum than through the stomach. When oxalic acid is introduced under the same collateral circumstances into the stomach of one dog and the peritoneum of another, the dose may be so apportioned that the same quantity, which does not prove fatal to the former, kills the latter in fourteen minutes."

It may be urged that the act of injecting either the peritoneum or cellular membrane could not be performed without some risk of exciting inflammatory action. And that this would often be the case in *healthy individuals* I have no doubt. But in the collapse of cholera, when all the great processes of the economy upon which inflammation depends are in a state of abeyance, such a result is in no way probable; and if it were, it ought under the circumstances to be regarded as a matter of secondary consideration.

I should scarcely omit to refer, in conclusion, to the fact (though it is one pretty generally understood) that imposters in feigning diseases, have been known to inflate their own cellular tissue or that of their children with air, without any harm following; and the authors of the article, "Feigned Diseases", in the *Cyclopædia of Medicine*, make special reference to the statement, that some French conscripts were known to inject water into their own peritoneal cavities, to produce a factitious ascites; while numerous instances have lately been recorded, in which, after the operation of tapping, such irritating substances as iodine and brandy have been thrown into the peritoneum without leading to fatal results.

With these observations, I now close this report, offering many apologies for its incompleteness. My object has been simply to show that the treatment of cholera admits, as the Council of the Society have imagined, of much consideration from a rational and physiological point of view.

Mortlake, August 1854.

REPORTS OF SOCIETIES.

EAST SURREY CHOLERA SOCIETY.

This valuable Society held its second General Meeting in the Hawkesley Hall, Waterloo Road, on Tuesday, the 22nd instant, at 7 P.M. Dr. WILLIS, the President of the Society, was in the Chair.

REPORT OF COUNCIL.

The business commenced by the reading of a Report from the Council, of which we regret we can only give an outline.

The Report commenced by referring to the present outbreak of cholera. It then noticed the draft code of laws for the government of the Society, which had been approved by the Council, and would be submitted to the Society for consideration. The Council had been instructed to prepare a series of queries and printed schedules for the record of observations on cholera, choleraic diarrhoea, and the prevailing character of all diseases; and to prepare a report on the treatment of cholera. The absence of the disease had till recently rendered the issue of the queries unnecessary, and the materials for the report had not been received. The subjects of treatment, etc., had been entrusted to a Committee, with instructions to direct its attention to the following objects:

"Firstly, to prepare a full and careful digest of the whole

literature relating to the treatment of cholera, taking care to distinguish, as far as is possible, the varieties of cholera.

"Secondly, to prepare a schedule of queries on the treatment of cholera, to be addressed to every member of the East Surrey Cholera Society, and to all other members of the medical profession whom the Committee may think willing to afford information on the subject.

"Thirdly, to institute any physiological experiments which may appear to them calculated to throw light upon the subject of their inquiry."

The Committee had not yet been able to prepare a report on the literature of cholera. They had drawn up the following questions on the subject of treatment, which they recommended should be extensively circulated throughout the profession.

"1. Will you detail the method or methods of treatment which you believe to be most efficacious in cholera?"

"2. Have you employed in practice the method or methods named in reply to the above? If so, in how many cases? Under what circumstances? And with what results?"

"3. To secure uniformity of data, would you give, briefly but distinctly, your definition of cholera?"

The Report contained the following suggestions with reference to the treatment and history of cases.

"With reference to treatment, there are now before the profession several recognised plans; the real and relative values of which it would be highly desirable to ascertain. Every respondent will probably have some experience of the mineral acids; of opium; of the terebinthines; of camphor, creasote, and chloroform; and of the ethers. Many will have used metallic astringents, both by the mouth and by enema, and the benefit apparently obtained from each of these familiar remedies should be carefully observed and recorded. If these, by common consent, could be put in their right places, and if the circumstances in which they are indicated could be clearly known, the Society would indeed have much reason to congratulate itself on the results of its exertions.

"Your Council would, however, suggest, that in relation to the effect of remedies, the periodic phenomena of each case require to be carefully observed; it being quite certain that, in many instances, the natural remission or intermission of the disease has produced an erroneous impression in favour of some particular drug. This branch of the inquiry will involve some notice of the effects of quinine, whenever time for its administration has been afforded.

"With reference to the history of cases, the problem that chiefly requires their aid for its solution, is the nature of the relation between cholera and diarrhoea; or rather, in the first place, the difference between the diarrhoea which would lapse into cholera, and that which would not; and, secondly, the relations between the former "*premonitory*" or "*choleraic*" diarrhoea, and the cholera itself. These two questions meet and confound us on the threshold of all inquiries into the transmission of the disease, and at the dawning of every hope of successful prophylactic treatment. Until the obscurity which enshrouds them can be cleared away, our prognosis must remain uncertain, and our treatment must remain empirical. Your Council desires earnestly to impress the importance of these questions upon all who observe and record cases of cholera; and to suggest that an attempt should be made to answer them, on the part of each practitioner, by a distinct statement of the sense in which he understands and uses the words "cholera" and "diarrhoea" respectively. And as this sense will vary widely for different individuals—one man saying that he has had much cholera in his practice, and another that he has had none, each having been in attendance upon the same class of cases—your Council is of opinion that correctness will be more nearly approached by a redundancy than by a deficiency of returns, and hopes that an account of every case presenting a distinct affinity to cholera will be sent in, leaving it to the proper Committee to classify the materials thus furnished. It must be regarded as highly probable, that an analysis of many well-reported cases would lead to the discovery of some general principle, by which the relations above alluded to are governed and controlled."

On the motion of Mr. WHITEMAN of Putney, seconded by Dr. CORMACK, the Report was adopted.

NAME AND LAWS OF THE SOCIETY.

Some discussion took place regarding the draft of laws of the Society, brought forward by a Committee appointed at a previous meeting. Ultimately, the following code was unanimously adopted, on the motion of Dr. HOLMAN of Reigate, seconded by Mr. MITCHELL of Clapham:—