fertile field where I have not ventured to trespass. And if you find, as doubtless you will find, that here the sword has proved more trenchant than the pen; that art also as associated with Plymouth has completely distanced literature in the race for precedence; that, in fact, with one or two notable exceptions, its literature is, comparatively speaking, a failure when contrasted with the bright array of its artistic talent, I would fain hope that your presence here may stimulate us to remedy this deficiency. Such an assemblage as the one before me must leave its mark on the future history of Plymouth, if it be instrumental in calling our attention to those shortcomings, of which we are wilfully ignorant or tacitly conscious. Once recognise our defects, and a most important step has been taken towards their amelioration. That there is nothing so successful as success is a truism we cannot refute; but success, be it remembered, is never so permanent as when it is evoked from the ruins, and established upon the basis of an admitted failure. If this result be attained by your co-operation, either in awakening our emulation or in giving an impetus to latent talent, as yet crippled and confined from its never venturing to soar beyond mere local associations, or from its shunning the friction which can only give it an enduring polish, it will add the element of permanency, though not an increase of cordiality, to the welcome which I now bid you to Plymouth.

ADDRESS IN MEDICINE,

BY

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NATURE AND ART IN THE CURE OF DISEASE.

MANY of those whom I have now the honour to address, will remember how great an interest was excited when, about fourteen years ago, the late Sir John Forbes published his book on *Nature and Art in the Treatment of Disease*. The author of that little volume, in clear and vigorous language, with pitiless logic, characteristic truthfulness, and fearless candour, pointed out the evils resulting from what he calls the over-active perturbative treatment and the mischievous polypharmacy which were then prevalent. Sir John Forbes lamented that in his day a purely expectant treatment of disease was rarely practised, except under other colours and under other names; and he referred to the results of homeopathic treatment, which he, with reason, looked upon as simply inert, in proof of the proposition that, "the power of Nature to cure disease is infinitely greater than is generally believed by the great body of medical practitioners and by the public. So great, indeed, is this power," he goes on to remark, "and so universally operative, that it is a simple statement of facts to say, that of all diseases that are curable and cured, the vast majority are cured by Nature independently of art; and of the number of diseases that, according to our present mode of viewing things, may be fairly said to be curable by art, the far larger proportion may be justly set down as cured by Nature and art conjointly." He elsewhere remarks that cases recorded by young practitioners in the medical journals afford additional evidence of a yet stronger kind, by showing not simply the power of Nature to overcome natural disease, but to overcome this and the artificial disease superadded by what he very unpolitely calls "the energetic ignorance of the practitioner."

To the same effect, many years before, Lord Byron, when half-dead of a fever in Greece, wrote what he intended to be his own epitaph:

"Youth, Nature, and relenting Jove,
To keep my lamp in strongly strove;
But Romanelli was so stout,
He beat all three, and *blew* it out."

After all, however, he recovered on that occasion, and he adds: "That Nature and Jove being piqued at my doubts, did, in fact, at last beat Romanelli, and here I am well, but weakly."

Since the publication of Sir John Forbes's book, and partly, no doubt, in consequence of that publication, our views as to disease and its treatment have undergone a very great change. A purely expectant treatment is now as common as then it was rare. It is now fashionable and orthodox to trust to the curative powers of Nature, and to doubt the therapeutic power of art. The pendulum has swung from one extreme to the other. At that time it was said, "that according to the vulgar notion, the function of the physician consists in little else than the prescription or administration of drugs, and the function of the patient in little else than swallowing them." Now, on the contrary,

that which was once said satirically, has come to be an almost accepted rule of practice; namely, that "the chief business of the physician is to amuse the patient while Nature performs the cure."

Now it is a very noteworthy fact that, simultaneously and side by side with this firm belief in the almost all-sufficiency of Nature and the impotence of art, there notoriously exists an extreme unwillingness to admit that any phenomena of disease can rightly be considered as having a conservative or curative tendency. The writer of an article "On the Pathology of the Microscopic Arteries," in the April number of the *British and Foreign Medico-Chirurgical Review*, had good reason for the statement which he there makes, that to speak of pathological processes as curative efforts of Nature, "is a mode of looking at the phenomena of disease always suspiciously cross-examined at the present day."

This statement suggests the inquiry whether a suspicious cross-examination is not uncalled for and out of place when the object is to arrive at truth, and not merely to gain a verdict. For the discovery and appreciation of scientific truth, the judicial impartiality of the bench is certainly more effective than the zealous and sometimes not over-scrupulous partisanship of the bar.

Surely, there is, *à priori*, good reason to believe that in the curative processes of Nature there is an orderly method of procedure, of which, by a diligent search, we may gain some useful knowledge. That marvellous reparative power which cures a fever or an inflamed lung as thoroughly as it heals a wound or mends a broken bone, must certainly work after a definite plan in each class of cases. The characteristic manner in which, during the progress of acute febrile and inflammatory diseases, the temperature rises, fluctuates, and finally falls, within a period which, for each disease, has a more or less exact limit, is one amongst other indications of such a definite plan and order. And as conservative surgery has been much advanced by a careful study of Nature's method of healing wounds and repairing injuries, so it can scarcely be doubted that a thorough analysis of the more obscure phenomena of internal diseases, and an investigation of their physiological sequence and relationship, may afford us great assistance in our endeavour to conduct our patients safely through the storm of a dangerous sickness. Indeed, I hope to carry you with me in my attempt to show that much knowledge of this kind is already available and ready for daily use.

We are all too apt to forget that disease is a natural, although an abnormal condition of the body; that pathology is, in fact, a department of physiology, and that the phenomena of disease result from the action of the normal structures and forces only modified by morbid conditions. To take an extreme case, the time, perhaps, is not far distant when we may be able to give a physiological interpretation of even that destructive deviation from the normal processes of nutrition which results in the disease recognised as cancer. Meanwhile, much aid to scientific pathology and practical medicine is to be expected from the improved teaching of physiology, to which so much attention is now being given. Unquestionably there is a great call for improved and extended physiological training; for there are manifest signs of a lamentable ignorance of the subject not only amongst otherwise highly educated laymen, but amongst medical writers who publicly discuss physiological questions, and who therefore subject themselves to public criticism. You will not have forgotten that a very influential newspaper lately endeavoured to excite the sympathy of its readers and to bring discredit upon an eminent professor, by commenting on the supposed sufferings of a decapitated frog, whose automatic movements were assumed to be the result of volition and to be indicative of pain. Then, if any one desires to test the physiological acquirements of certain writers on pathology, let him study, as I have done, the copious literature of cholera. He will find, amongst other anomalies, that one writer of great eminence considers that he has sufficiently explained the collapse of cholera when he pronounces it to be the result of "paralysis of the sympathetic nerve." Another writer on cholera having satisfied himself by experiment that the disease is communicated by the poisonous discharges from the alimentary canal, maintains that the poison does not become absorbed so as to enter the circulation, but that it has a purely local action upon the epithelial lining of the bowel.

Now the rapid absorption and diffusion of all known poisons as an essential condition of their operation is one of the best established of physiological doctrines. The interesting observations of Dr. Bence Jones demonstrated, not only the rapidity of absorption from the alimentary canal, but also the rapid diffusion of the absorbed materials out of the vessels even into the non-vascular tissues. These physiological doctrines, as well as the numerous facts which go to prove the absorption of the cholera poison, are ignored by the author of the theory to which I have referred, and if we accept his teaching we are required to believe that in the case of the cholera poison a general law of physiology is suspended.

I propose now to refer to some very obvious and well-known phenomena as examples of morbid processes having a conservative or curative tendency. Let us take, for instance, the case of a patient who is jaundiced in consequence of mechanical obstruction of the duct by a gall stone. His urine is charged with bile, the result of a vicarious excretion of bile products by the kidneys. It will scarcely be denied that the elimination of bile through the urinary channels is, on the whole, a wholesome conservative process, tending, as it does, to free the blood from impurities, the retention of which might be injurious, and even fatal. Yet, this vicarious excretion is not effected without some functional disturbance, and even temporary structural change in the kidney. An examination of bilious urine often discovers in it desquamated renal epithelium, tube casts, and sometimes albumen—pathological conditions which quickly pass away when the bile, resuming its natural course, ceases to be excreted by the kidneys.

Again, in a case of diabetes there is an abundant secretion of saccharine urine. Now, whatever may be the primary seat and essential cause of diabetes, it is certain that the free elimination of the excess of sugar by the kidneys is essentially a beneficial and life preserving process. Yet this continued secretion of sugar not unfrequently induces structural changes which ultimately lead to a fatal result. The persistent elimination of sugar by the kidneys has the effect of changing the structure and the vital properties of their secreting cells, so that, in adapting themselves, as it were, for the secretion of sugar, they gradually become unfitted for the discharge of their own proper function, namely the secretion of urine. The renal gland cells at length become opaque, and are found to contain a large amount of finely granular material with oily particles. The circulation through the kidney becomes impeded, the gland is congested, its secretion is albuminous, and suppression of urine is the immediate cause of death in a large proportion of cases of diabetes.

I believe that the familiar facts to which I have here briefly referred afford a good illustration of a principle which admits of a very extensive application to the interpretation of morbid phenomena. A large number of diseases are caused by, or at any rate associated with, some form of blood-poisoning. The morbid poisons and their products are eliminated through various excretory channels, more especially through the kidneys, the liver, the skin, the lungs, and the mucous membrane of the alimentary canal. In many instances the elimination of the morbid poison causes much functional disturbance and structural change in the excretory channels through which the poison escapes, and these structural changes constitute the most striking outward signs and diagnostic marks of the diseases with which they are associated. Thus we have the cutaneous eruptions of the acute exanthemata, the gastro-intestinal symptoms of cholera, the bowel disease of dysentery and enteric fever, the hepatic disease which results from excess of alcohol or from the influence of malarious poisons, the renal disease, which may also be caused by an excess of alcohol, by the poison of scarlet-fever, and by various other morbid poisons which I need not now stop to particularise.

Here let me say that I am perfectly well aware that by a certain class of pathologists the doctrine of the elimination of morbid poisons is ridiculed as a vestige of a by-gone unenlightened age, well enough adapted to please the fancy of old women and ill-educated laymen, but quite unworthy the acceptance of scientific physicians. One of this very advanced school lately intimated to me privately that, as a supporter of this exploded notion, he looked upon me as the priest of a decaying faith. Now it is a remarkable fact that the opponents of the doctrine in question not unfrequently render it ridiculous by misrepresenting it; the misrepresentation being, of course, unintentional and apparently an unconscious result of confusion of ideas in the minds of the critics. Thus, quite recently in one of the medical journals, an eminent provincial physician asks what we mean by the term disease, and he replies by saying,—"In old times people used to think that a disease was some actual entity or thing which had got into the body in some way and was there lying hidden and secreted, and was to be cast out. We know that there are a few instances of this kind of thing. For example, we know that some disorders depend upon the presence of a tape-worm in the intestines, and that to cure them we must hunt the worm out of the body. This idea, which we know to be true only in a few specific instances, was at one time general. The germ of this idea still runs through a great many of our conceptions of the nature of disease. This is more particularly the case in the pathological conceptions of ill-educated laymen, but many traces of it are still to be found amongst ourselves also."

I venture to suggest that the writer of the passage which I have here quoted confounds, as others equally eminent have done before, the idea of disease with that of a morbid cause. If anyone maintains, or ever did maintain, that *disease* is an entity having an existence apart

from the living body, I am not here to defend so manifest an absurdity. I apprehend that what we all understand by disease is an abnormal condition of the living body. The causes of disease are almost infinitely various. Some diseases have their origin in purely mental, emotional, or nervous influences, but the cause of many diseases is as certainly, if not as obviously, a separate entity as the ovum of a tape-worm. For instance, the disease which we call lead-colic is the pain with constipation experienced by a man who has been poisoned by lead. The colic, of course, can have no existence apart from the sufferer, but the metallic poison which causes the colic was introduced from without, and may be again ejected from the system. We know that small-pox and scarlet-fever can exist only as phenomena of living bodies, but the cause of each of these diseases is a material and portable poison, which, being thrown off from the bodies of the sick, conveys the disease to the healthy. The theory of contagion as generally accepted, and the practice of disinfection as commonly adopted, both imply a belief in the elimination of morbid poisons. This doctrine of the elimination of morbid poisons is, of course, quite consistent with a belief that in part they may be decomposed and destroyed within the system, although we can have no proof of the actual occurrence of this destructive process.

I almost feel that I owe you an apology for occupying your time by insisting upon such obvious elementary truths. It is strange that at a time when so much discussion is going on as to the nature of morbid poisons, when germs and dust and fungi, their nature and origin and operation upon the living body, and the best mode of preventing or neutralising their effects form the subjects of fierce controversy, there should occasionally be heard a confused utterance which seems to imply that the whole controversy is utterly unpractical and unmeaning; that while a tape-worm is an undoubted fact, morbid poisons are the phantom products of a disordered brain. As an illustration of this kind of unreasonable scepticism, I may remark that a recent writer on cholera in India, Mr. Macnamara, denies the existence of a blood-poison in that disease because Dr. Thudichum failed to obtain *chemical* evidence of such a poison. As if Dr. Thudichum had chemical proof of the existence of any of the morbid poisons of whose reality and destructive activity the evidence, other than chemical, is only too conclusive.

And now to return to the subject of curative efforts of nature and the conservative tendency of certain morbid processes. There are few diseases which afford more striking illustrations of the principle in question than the various forms of Bright's disease when traced through all their stages from their origin to their termination. Excluding from consideration those cases of albuminuria which are caused by a mechanical impediment to the circulation, the result, usually, of cardiac or pulmonary disease, it may be held as a doctrine generally true that the primary cause of Bright's disease in all its forms is a morbid condition of blood, and that the structural changes which the kidney undergoes are the result of a conservative effort to excrete noxious materials from the circulation. In cases of transient blood-poisoning, scarlet-fever for example, the structural changes in the kidney and the functional disturbance may be only temporary and the recovery complete. But in other instances, as, for example, cases of chronic alcoholism—a very frequent exciting cause of Bright's disease—the continued passage of noxious products through the secreting structures of the kidney gradually destroys the tissues, and the gland either wastes and contracts, or becomes so structurally changed as to be unsuited for the discharge of its functions. There is then a secondary blood-contamination, a result of retained urinary excreta, and this uræmic condition may in a variety of ways bring about a fatal result. In illustration of our present subject, it may be observed that when the renal disease has reached an advanced stage life is often prolonged for a time by the vicarious elimination of urinary excreta through the mucous membrane of the alimentary canal. It is probable, too, that the dropsical symptoms which often complicate renal disease are the result of a conservative effort to free the blood from an excess of water, whose retention within the vessels would be more detrimental than its accumulation in the subcutaneous tissue and the serous cavities.

I wish to avail myself of the present opportunity to protest against a purely theoretical, although professing to be an anatomical, division of cases of Bright's disease, which originated with Virchow, and which has been accepted by some subsequent writers apparently without inquiry, and certainly without proof. A German theory appears sometimes to be as irresistible in this country as a German invasion has been found to be elsewhere. The theory to which I refer is this: That there are three forms or classes of cases of Bright's disease; in one class of cases the uriniferous tubes are the seat of structural change; in a second class the blood-vessels of the kidney are primarily and chiefly implicated; while in a third class the essential structural change consists in what is eupho-

niously called hyperplasia of the connective tissue between the tubes. Now this is a question of anatomical fact, and I am prepared at any time to demonstrate, as I have often done before—first, that in every case of Bright's disease both the uriniferous tubes and the renal blood-vessels are always structurally changed; secondly, that in no cases of Bright's disease does the essential structural change consist in a so-called hyperplasia of the connective tissue between the tubes. There is one insuperable objection to the connective tissue theory, and it is this, that the best anatomists agree in opinion that there is no connective tissue between the uriniferous tubes of a healthy kidney. It is difficult, therefore, to believe that hyperplasia or overgrowth of that which has no existence can constitute the essential anatomical character of any form of Bright's disease. Moreover, this theory compels those who adopt it to disregard very obvious structural changes within the tubes, changes which are not explained by the theory, and which are inconsistent with it.

In further illustration of conservative morbid processes, let me direct your attention for a moment to certain changes of almost constant occurrence which are found in the heart and in the minute arteries, after death from chronic Bright's disease. In the first volume of the *Guy's Hospital Reports*, Dr. Bright pointed out the fact that in a large proportion of cases of chronic Bright's disease there is considerable hypertrophy of the left ventricle of the heart, even when there is no obvious mechanical impediment to the circulation, the cardiac valves and the large arteries being free from disease. Dr. Bright, not being infected by the modern heresy which forbids us to seek for design in morbid processes, believed that this hypertrophy of the left ventricle had a physiological significance, and he suggested as a probable explanation of the phenomenon that "the altered quality of blood might so affect the minute and capillary circulation as to render greater action necessary to force the blood through the distant subdivisions of the vascular system." About four years since I had the satisfaction of being able to demonstrate as an anatomical fact, that which Dr. Bright had long before suggested as a reasonable hypothesis. I will state briefly the steps by which I was led to this result. It has been conclusively proved that the force which propels the blood through the systemic arteries is derived from the contraction of the muscular walls of the left ventricle of the heart. The force of elastic resiliency in the walls of the large arteries is derived from the muscular contraction of the heart as obviously as the elastic power of an archer's bow has its source in the muscles which bend the bow. The elastic walls of the arteries, distended by the injecting force of the ventricle, contract when that force ceases to act. The resiliency of the arterial walls, reacting upon the blood during the diastole of the ventricle, gradually converts the intermitting jet of blood from the heart into a continuous stream in the minute arteries and capillaries, just as the net-covered India-rubber ball in the ether-spray apparatus converts the intermitting current of air into a continuous stream in the tube beyond. The larger arteries have their walls mainly composed of yellow fibrous tissue, with but a small proportion of muscular fibre; on the other hand, the walls of the smallest arterioles which terminate in the capillaries are mainly composed of muscular tissue. These minute arteries, with their muscular walls, under the influence of the vaso-motor nerves, regulate the blood-supply to the various tissues and organs. By the contraction of their walls the arterial canals are narrowed, and the blood stream is in a corresponding degree lessened; on the contrary, relaxation of the arterial walls enlarges the canals, and a fuller stream of blood is permitted to pass. The minute muscular arteries, therefore, perform the function of stop-cocks; they exert a regulating, but not a propulsive, influence upon the blood stream.

Now let us proceed to apply these well established physiological doctrines in explanation of the hypertrophied left ventricle in cases of chronic Bright's Disease. It occurred to me as in the highest degree probable, that if, as Dr. Bright long ago suggested, the morbidly altered blood is impeded in its passage through the smaller blood-vessels, the cause of this impediment must be an excessive contraction of the muscular walls of the minute arteries. Then, if this were so, the continued over-action of the arterial walls should result in hypertrophy of their muscular tissue, and the degree of over-action would be found registered in the thickened arterial walls. The next step was to search for this hypertrophy of the arterial walls in cases of chronic Bright's Disease, when *post-mortem* examination showed that hypertrophy of the left ventricle existed, without disease of the valves or the large arteries to explain its occurrence. Accordingly we sought for it, and found it unmistakeably present, not only in the minute arteries of the kidney, where I had often seen it many years before, but also in those of the pia mater, the skin, the intestines, and the muscles. It will probably be found to exist in the arteries of other tissues which we have not yet examined. There is good reason to believe that the undue contraction of the arteries which results in hypertrophy of the walls is excited by

the abnormal quality of the circulating blood, while the hypertrophy of the left ventricle is a natural result of the increased action required to force the blood through the resisting arterioles.

Now it is obvious that, in order to maintain the exact balance between the propulsive force of the heart and the regulating power of the minute arteries, there must be a nice relationship between the hypertrophy of the ventricle and that of the arterial walls in each tissue. It is manifest, too, that if the arteries in any tissue are relatively less hypertrophied and strengthened than those in other parts of the system, the capillaries of that tissue must be liable to be over-distended, and even ruptured, by the injecting force of the hypertrophied heart. In accordance with this view, we found that in one case of chronic Bright's Disease, complicated with cerebral hæmorrhage, the minute arteries of the brain were certainly less hypertrophied than those of other tissues in the same subject. In another case of hæmorrhagic effusion into the retina, the arteries of the retina presented no appearance of hypertrophy, while those in other tissues were as obviously hypertrophied as were the walls of the left ventricle.

In these days of specialism, when the tendency is to dis sever the various organs of the body, and to forget their physiological relationship to each other, it is interesting and instructive to find that the deterioration of the blood which results from the degeneration of one organ—the kidney—may induce hypertrophy not only of the walls of the heart, but also of the walls of the minute arteries in every tissue throughout the system. A good illustration this of the very ancient doctrine that, "if one member suffer, all the members suffer with it."

Time will not permit me to comment upon the various symptoms of Bright's Disease, which receive their explanation from the anatomical changes which I have described. There is, however, one phenomenon to which I must make a passing reference. A full, hard, resisting radial pulse, with increased arterial tension, as indicated by the sphygmograph, is commonly observed in cases of chronic Bright's Disease. The explanation is obvious, by reference to the two antagonistic forces, that of the strong left ventricle, and that of the equally strong, resisting arterioles. So that, when the history of a case and the chemical and microscopical characters of the urine are insufficient to determine the question whether the renal disease is chronic or of recent origin, the physical signs of hypertrophy of the left ventricle, with increased arterial tension, afford important confirmatory evidence.

Before I pass on to the next division of my subject, I wish to say another word in defence of the teleological argument as applied to the structure and functions of the living body, whether in health or in disease. It is sometimes asserted that we can learn nothing of causation, or purpose or design, and that our sole business is to study the sequence of phenomena. Yet it is notorious that some of the greatest discoveries in natural science have been made by men who have followed up, step by step, the evidences of design and purpose in the structure of organised beings. By this process of observation and reasoning, Harvey made the grand discovery of the circulation of the blood. Mr. Darwin, acknowledged even by those who differ from him to be one of the greatest and most philosophical of living naturalists, is indefatigable in his endeavour to discover and to explain the purpose and the design displayed in the structure of animals and plants. Thus, with reference to the wonderful ball-and-socket ornaments on the wing-feathers of the Argus pheasant, Mr. Darwin says: "No one, I presume, will attribute the shading, which has excited the admiration of many experienced artists, to chance—to the fortuitous concurrence of atoms of colouring matter. That these ornaments should have been formed through the selection of many successive variations, not one of which was originally intended to produce the ball-and-socket effect, seems as incredible as that one of Raphael's Madonnas should have been formed by the selection of chance daubs of paint made by a long succession of young artists, not one of whom intended at first to draw the human figure" (Darwin's *Descent of Man*, vol. ii, p. 141). He argues that those who deny that this beauty of plumage is designed to captivate the female bird, will be compelled to admit that the extraordinary attitudes assumed by the male during the act of courtship, by which the wonderful beauty of his plumage is fully displayed, are purposeless; and he adds, "this is a conclusion which I, for one, will never admit."

Now this surely is a teleological argument. Obviously then Mr. Darwin sees no inconsistency between teleological reasoning and the theory of evolution, of which he is so powerful a supporter, and so eloquent and able an exponent.

Again, the writings of John Hunter abound in the most ingenious suggestions as to the physiological purpose of morbid processes. For example, he gives a reason for the cessation of the circulation when the respiration is suspended. He says it depends upon "the sympathetic connexion which exists between the heart and lungs (one action ceasing, the other also ceases); which sympathy is established, because if

the heart were to continue acting, it would send improper blood into the body, by which it can be supported only a little while. The right auricle and ventricle also cease acting, although not so early, and for the same reason; because on the cessation of the function of the lungs, the blood cannot receive any benefit in passing through them. These actions and cessations are all dependent on life and the connexion of one action with another." (Palmer's *Hunter*, vol. iii, p. 78.)

We now know that the immediate cause of the arrest of the circulation following upon apnoea is the resistance to the passage of blood offered by the contraction of the minute pulmonary arteries, and not the cessation of the heart's contractions as Hunter supposed; but we may still believe that the final cause or purpose of the arrest is that which Hunter suggested—namely, that when respiration is suspended, nothing would be gained by sending the blood through the lungs, or by supplying the tissues with unacrated blood.

During the time which remains to me to-day, my purpose will be to show that, as practitioners of medicine, we have something more to do than to watch the phenomena of disease as passive spectators, and that in our endeavour to prevent, to mitigate, and to cure disease, we have a better guide than mere empiricism.

Obviously, one of the most essential conditions for the successful treatment of disease is an exact diagnosis. If diseases essentially different are confounded together, it is impossible to arrive at any trustworthy conclusion as to the effect of particular remedies or plans of treatment. Nor does it suffice for successful treatment that a disease be correctly named and referred to its right place in a nosological system. I need only refer to the group of cases which are designated epilepsy to illustrate this proposition. There is reason to believe that the immediate cause of an epileptic convulsion is, in all cases, identical—namely, a sudden and extreme anæmia of the brain; but the remote and exciting causes of this cerebral anæmia are numerous and very diverse. It may be a result of failure of the general circulation consequent on profuse and rapid hæmorrhage, or a plug of fibrine (embolism or thrombosis) in the pulmonary artery; or the pulmonary, and consequently the systemic, circulation may be rapidly arrested by one of the various forms of apnoea; or by the accidental admission of atmospheric air into the veins during the performance of a surgical operation at the root of the neck or in the axilla. But in most cases of epileptic convulsion the arrest of the circulation and the cerebral anæmia are probably due to a sudden and extreme contraction of the minute intracranial arteries. The exciting causes of this arterial contraction are very various. It may be the result of a purely nervous or reflex influence acting, through the vaso-motor nerves, upon the cerebral vessels. Examples of this form of epileptic convulsion are afforded by cases in which the determining cause is emotional excitement or mental anxiety, syphilitic or tuberculous inflammation of the brain or its membranes, painful dentition, the irritation of intestinal worms. In another class of cases, blood-poisoning is the immediate cause of the arterial contraction. To this group belong cases of uræmic convulsions—convulsions the result of alcoholism, and the convulsions which sometimes occur during the initiatory fever of the acute exanthemata. If the treatment of epileptic convulsions is to be successful, or even harmless, we must do something more than give the disease a name and prescribe bromide of potassium, valuable as that remedy is in many cases of this terrible disease.

We are sometimes told that our chief duty as practitioners is to treat symptoms, and to leave pathological theories to those who have nothing better to occupy their time and attention. A patient consults us for aphonia; shall we prescribe for this symptom without looking into the larynx to ascertain its cause? Loss of voice, more or less complete, may be a result of a transient functional nervous derangement; it may be caused by the pressure of an aneurism, or of a cancerous or other tumour, upon the pneumogastric nerve or its recurrent branch; it may be a direct result of a simple or a malignant tumour within the larynx, or of the swelling or ulceration which results from the various forms of inflammation to which the larynx is liable. For the practical purposes of prognosis and treatment, it is not sufficient to be able to say of a disease that it is laryngitis; we have further to ascertain to which of the three large groups of cases it belongs; namely, the catarrhal, the strumous, or the syphilitic form of laryngitis.

Again, when it is said that we are to treat symptoms, it is meant that we are to make it a rule of practice to endeavour to repress all the symptoms of disease without reference to their origin, their physiological significance, or their probable conservative tendency? Such a rule would lead to the repression of the eruption of scarlet-fever and small-pox by external cold, to the abrupt arrest of choleraic discharges by opium, and to the weakening of a strongly pulsating hypertrophied heart associated with valvular disease by low diet and depletion—modes

of treatment condemned alike by pathological science and by the results of experience.

In estimating the results of treatment, the ultimate appeal must obviously be to experience; experience, however, has notoriously proved, in many instances, a fallacious guide—*experientia fallax* is a maxim which we shall all do well to keep continually in mind.

But, of all the delusive guides in practice, a false theory of disease is the most to be dreaded. It was an erroneous theory that suggested the antiphlogistic treatment of delirium tremens and the narcotic and alcoholic treatment of cholera; and theory alone prevented the more speedy discovery and rectification of these practical errors.

A knowledge of the *causes* of disease, and of their *modus operandi* in the production of morbid phenomena, may be made practically useful in many and various ways. The evidence obtained within the last few years of the manner in which cholera and enteric fever spread through the contamination of the air and drinking-water by the intestinal discharges, has greatly increased our power of preventing these destructive diseases. The ascertained influence of comparative dryness of soil and of atmosphere, resulting from improved drainage, in diminishing the mortality from phthisis in various parts of America and in this country, forms another important contribution to practical and preventive medicine.

To discover, and if possible to remove, the exciting cause, may sometimes suffice for the cure of a disease of most formidable nature and of obscure pathology. In illustration of this statement, I am tempted to relate very briefly the following case. A boy suffering from tetanus came under my care in the hospital. The symptoms, which had existed for rather more than a week, had commenced about ten days after the infliction of a wound in the upper part of the thigh by a sharp piece of wood. The wound had healed; but I found that the cicatrix was remarkably hard and tender when pressed. While examining the case, I remembered two fatal cases of traumatic tetanus referred to by Dr. Taylor in his book on *Poisons*. In both cases, the wound had healed; but in one there was found after death, beneath the cicatrix, a piece of iron; and in the other a splinter of wood. Thinking it not unlikely that some foreign body might be present in my case, I gave the boy chloroform, and requested the house-surgeon, Mr. Whitmore, to cut through the cicatrix. In doing so, he found neither iron nor wood beneath the skin, but a piece of woollen stuff of about the size of a small pea. The stick which wounded the boy's thigh had pierced his cloth trowsers, and, carrying with it a piece of wool, had left it in the wound. The tetanic symptoms gradually subsided. Repeated small doses of chloral apparently assisted the recovery; but, if the foreign body had remained under the skin, it is probable that neither this nor any other remedy would have prevented a fatal result.

There is a numerous and important class of cases in which suppressed action of the skin by cold or by some unknown atmospheric influence is the exciting cause of disease in internal organs; for example, cases of acute renal dropsy, whether occurring during the progress of scarlet fever, or unconnected with that disease; cases of catarrh and bronchitis; cases of acute pneumonia; and some cases of acute rheumatism. Now there is a principle of practice common to all these cases, which is this: that if, at the very commencement of the disease, a free action of the skin can be promoted, the malady may often be cut short; or, if not entirely and promptly arrested, it will be greatly mitigated. It is notorious that an ordinary catarrh may almost certainly be cured by free diaphoresis at the *very commencement* of the attack. I have myself gone into a hot-air bath suffering from headache, pain in the limbs, and other indications of a severe incipient catarrh, which, if allowed to run its course, would probably continue for a period of from one to two or three weeks; and in the course of half an hour the symptoms have been entirely removed by the action of the bath. The same treatment by hot-air or blanket baths is certainly very efficacious in the *early stages* of acute renal dropsy, more especially when cold has been the exciting cause of the disease. The object of the diaphoretic treatment in these cases is not, as some writers appear to imagine, to sweat urine through the skin, but, by diverting a large amount of blood to the surface, to lessen the congestion of the kidney—as dry cupping over the loins does in a less degree—and thus to increase the secretion of urine. The objection which has been made to this sweating practice—that it tends to waste a portion of the water which is required to wash morbid products from the uriniferous tubes—affords a good illustration of the insufficiency of mere physics to explain physiological phenomena.

In connexion with this diaphoretic practice, I have a few words to say on the treatment of catarrhal pneumonia. With reference to the treatment of pneumonia, two very distinct stages of the disease must be distinctly recognised—first, a febrile prepuermonic stage; second, the stage of pneumonic exudation. The commencement of an attack which

results in pneumonia is usually marked by rigors, a high temperature, quickness of pulse and breathing, not unfrequently vomiting or diarrhoea, or both. Then follow the signs of exudation into the lung; and as a rule, in an uncomplicated case, when the lung-tissue is consolidated, the temperature quickly falls, and the pulse and respiration become less frequent. Then, more or less rapidly, the inflammatory exudation is expectorated, and the lung is restored to its normal condition. I maintain that pathologically there is an analogy between the pulmonary exudation of pneumonia and the cutaneous eruption of small-pox, in so far as the initiatory fever abates when the exudation occurs—in the one case into the lung, in the other into the skin. Both are febrile and eruptive diseases; in one disease the eruption being pulmonary, and in the other cutaneous. Now I believe that, by treatment promptly applied during the febrile prepneumonic stage, the amount of subsequent exudation may be greatly lessened. The treatment suggested by theory and approved by experience consists in the prompt restoration of the suppressed cutaneous secretion, by hot air or water or wet sheet and blanket baths, an emetic of antimony or ipecacuanha when there is nausea without vomiting, and a calomel and colocynt pill, followed by a saline purgative. I have no doubt that by these means, adopted at the very commencement of the febrile stage, the severity of the disease may be very much mitigated; whereas, after the pulmonary exudation has occurred, the time for active treatment has passed: rest and food are then the chief requisites. To discuss the treatment of pneumonia without reference to the stage of the disease, is to include, under one name, pathological conditions essentially unlike.

It remains for me now to sum up in a few sentences the main points which I have had the privilege to bring before you.

I have suggested that a belief in the power of Nature to cure all curable diseases is inconsistent with a disbelief in the existence of morbid processes having a conservative or curative tendency. I have indicated various pathological phenomena the conservative tendency of which appears to me indisputable; and I have endeavoured to show that, by a careful study of the functional and structural changes which result from disease, we may obtain most valuable indications for treatment—learning thereby both to do that which may aid Nature and to avoid such means as may tend to thwart and hinder the natural curative processes.

Again, I have intimated that it is difficult, and, as it seems to me, impossible, to reconcile a disbelief in the elimination of morbid poisons with a belief in the spread of disease by contagion. Confirmatory evidence as to the elimination of morbid poisons is afforded by the disastrous results of repressive methods of treatment. To take all possible precautions to exclude the cholera-poison from the system, and then, when once it has gained an entrance, to endeavour to retain it there by opiates and astringents, are practical modes of procedure utterly inconsistent with each other; unless, indeed, the object of this repressive treatment be to sacrifice the individual for the public good—to prevent the patient, at the peril of his own life, from scattering the seeds of disease and death amongst the community.

I believe that the success of our attempts to cure and to prevent disease depends mainly upon an exact diagnosis and discrimination of the various forms and shades and stages of disease; upon a correct interpretation of pathological processes and symptoms; a careful avoidance of erroneous and misleading theories; and, lastly, upon a prompt recognition of the exciting causes of disease, some of which may be avoided, some removed, while the influence of others may be in a greater or less degree counteracted by the timely employment of suitable means.

THE USE OF IRON IN SCARLATINA.

I AM desirous of drawing the attention of the profession to the use of iron in scarlatina. I have given it for the past two years with great success; so much so, as to induce me to believe that in it we have a powerful remedial agent for that disease. I have found, if it be given as soon as the disease makes its appearance, that not only does it shorten and lessen the severity of the attack, but it also fortifies the patient against the after-consequences—dropsy, etc. The form which I have mostly used has been the liquor of permanganate of iron, in syrup or glycerine, in doses of ten minims every three hours for children of from one to six years, increasing, according to age, to fifteen, twenty-five, or thirty minims. During convalescence, I have given citrate of iron and quinine, ammonio-citrate of iron, or syrup of phosphate of iron, according to circumstances. This, with the exception of warm fomentations to the neck in cases of scarlatina anginosa, is all the treatment I have adopted, and, as I have before stated, with better result than has followed any other. I hope soon to give cases; but in the meantime I ask my professional brethren to give my suggestion a fair trial, and I believe they will be well satisfied with the results.

RUSSELL ALDRIDGE, M.D.

CHOLERA:

ITS DIFFUSION, PROPHYLAXIS, SYMPTOMS, AND TREATMENT.*

By JOHN MURRAY, M.D.,

Inspector-General of Hospitals, Indian Medical Service.

As cholera is regularly advancing from India through Russia and the Baltic ports towards England, and as the disease has pursued this course on several previous occasions, its arrival here may be shortly anticipated. As August and September are the months during which the last three epidemics prevailed in England, it is now time for every precaution to be taken to guard against its attack. As I have had opportunities of seeing this disease in India every year since 1833, I have been requested to relate the results of my experience in that country; and I willingly do so in the hope that it may be useful.

It is important that information should be given regarding the early symptoms of cholera and its treatment, as a delay of two or three hours will in many instances allow the disease to advance to a stage where those remedies will prove powerless, which, if exhibited sooner, would have controlled the disease and saved the life of the patient.

Much of the difficulty in understanding cholera has arisen from writers only acknowledging the presence of the disease when it has reached the third stage or collapse. I beg to state that the opinions to which I shall call attention on most of the important points in this disease are supported by a very great majority of the medical officers now in India, as shown in a recent report submitted by me to the Indian Government, in which the opinions of 505 medical officers are carefully tabulated.

In no malady are there more marked and characteristic symptoms than in cholera; but the symptoms, which are only found in one stage, are so dissimilar to those induced by the poison in an earlier stage or mild attack, that the connection has by many been overlooked. The value and importance of the early detection of the presence of the poison cannot be over-estimated, as we all know that, after a certain stage, the vital organs are rendered, by the absent or diminished absorption of the stomach, little sensible to the action of remedies; their functions are paralysed by the presence of the poison. It is in preventing the supervention of this stage that most life has been saved; and the physician who can soonest detect the presence of the poison will be most successful in his practice.

The earliest symptoms that can be recognised are those of *malaise*, viz., depression of spirits, want of appetite, torpidity of the bowels, and desire for stimulants. That the *malaise* I have described is caused by the presence of the poison of cholera is an opinion strengthened by the fact that, during an epidemic attack, when this feeling exists, the action of a purgative—especially salts—will almost always be followed by the other symptoms of cholera, and the circumstance of the frequent occurrence in men who have left the infected locality in apparent health and have been attacked within one or two days.

To the symptoms of *malaise* succeed *diarrhoea*, nausea, and vomiting; the urine is scanty; the stools light-coloured, then colourless, like rice-water, with occasional cramps, heart-burn, and slight headache. The countenance is dark and the eye-balls congested. This is followed by *collapse*, great prostration of strength, burning in the epigastrium, congee or rice-water vomiting and purging, with cramps and suppression of urine; cold clammy perspiration, feeble pulse and cold breath, broken voice and shrunk and livid face. When reaction takes place, the burning pain in the epigastrium disappears, the restlessness subsides, the stools become coloured, urine is secreted, warmth returns to the palms of the hands, the colour improves, the pulse becomes stronger, and sleep ensues.

In many instances the disease does not progress beyond the stage of *malaise* or *diarrhoea*. The poison appears to be digested or eliminated by the *vis medicatrix nature* through the natural functions of the system—hence the great importance of supporting these, and avoiding their being overtaxed, exhausted, or depressed.

Cholera is a specific disease, caused by the presence of a specific poison in the system; it multiplies or is reproduced; it must be vital and amenable to the ordinary laws which regulate other specific poisons, modified by the peculiar structures of the body which are chiefly affected. It must enter the body through some of the ordinary channels. Before health can be restored, it must be eliminated either in a vital state or after being decomposed or digested. Although we are unable, in many instances, to trace the manner in which the poison enters the system,

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yet in others the channel is evident and the source clear. The instances where cholera has appeared after drinking water contaminated by cholera discharges from wells, streams, tanks, or vessels, are numerous and well authenticated. The most convincing case which has come under my own notice occurred during the Hurdwar epidemic of 1867, when a pilgrim arrived at Joogh Kullian in the Punjab, on the 28th April. He was taken ill with cholera near a well and died next day. He had bathed, and his soiled clothes were washed in an adjoining pond. Other parties who afterwards visited the well and the pond for water and ablution were attacked two days afterwards; and up to the 15th May, fifty-three were attacked in that village, of whom twenty-seven died. There had been no cholera there for the previous twenty-four years.

I have witnessed numerous cases and heard of many more where the poison must have entered through the lungs; those attacked having been in the vicinity of cholera patients, or of their evacuations. It is difficult to show that the skin is the channel of communication, because the poison may be disseminated in the surrounding air, and thus enter by the lungs. But there are many instances on record of the disease appearing after the wearing of clothes worn by cholera patients; and Dr. MacLean mentions that the last case that occurred in the attack at Malta in 1865, was that of a washerwoman who stole a soiled shirt and wore it.

The poison leaves the body through the same channels by which it enters; viz., the bowels, the lungs, and the skin. Its presence in the discharges from these organs is recognisable in most instances by the smell—a mawkish, sickening odour, well known to those who have seen much of the disease. In the earlier stages the poison appears to be destroyed, or digested, without exciting any active symptoms, and this is the safest way of nature getting rid of it. The first active symptom is diarrhoea; and here we can be useful, as the system during this stage is amenable to remedies.

I shall now allude to some facts that are important in a practical point of view, as tending to promote or restrain the diffusion of the disease. I carefully examined the Records in the Medical Department in Calcutta from the year 1814, and found that, with the exception of the Delta of the Ganges, where the disease is never absent, its appearance in an epidemic form is limited to certain seasons and extending over three months in the year, and that this period of the year varies in different parallels of longitude; but there were sharp occasional attacks at other seasons.

The period of observation in Europe is shorter. It is probable that the disease may have appeared in former ages under the general names of plague, black death, or sweating sickness; but all the ordinary symptoms of what are called sporadic cases have frequently been found. The epidemics of 1849, 1854, and 1866, occurred in August and September; and we may infer that the arrival of the disease which is now gradually advancing therefore will be at the same season, and it becomes our duty to prepare for its advent.

All experience of the course of the disease teaches us that it rages with the greatest intensity, and proves most fatal where people are collected in great numbers—where there is crowding and filth, defective ventilation and impure water; and that it is aggravated by want and bad food. It is here that the circumstances of civilisation bear on the propagation of the disease.

Great benefit has arisen from a careful attention to the laws of sanitation, which have been assiduously studied of late years by many zealous investigators of this branch of medical science. Sanitation is of the greatest importance in the precautionary treatment of this epidemic, in ameliorating the virulence of the attack, restraining its dissemination, or warding it off entirely. On sanitation there is no difference of opinion; and both the medical profession and the public are aware of its importance, and its direction is in good hands.

There is another point of no less importance in the precautionary treatment, on which the profession was not in former years so unanimous: I allude to the contagious nature of the disease, meaning thereby the transmissibility, directly or indirectly, of the specific poison from a sick to a healthy person. I am unable to state the exact proportion of those who consider cholera as non-communicable in Europe or America; but in India there were only five out of five hundred and five medical officers who stated that it is not communicable. There is a slight divergence of opinion regarding the channel through which the communication takes place, and the changes undergone by the poison after leaving the diseased person, but none as to a diseased person being a source of danger if admitted into a healthy locality. The precautionary measures indicated by this opinion consist in the exclusion and isolation of the affected, and the removal of the healthy from the infected locality.

There has been a great diminution of mortality from epidemic cholera in India, particularly amongst the European troops, and in the gaoles, since precautionary measures founded on these indications have been

carried out. In many cases, cantonments and prisons have remained free from attack whilst the disease has raged amongst the population of the surrounding districts, and the mortality has been materially diminished in most of the stations where it did break out; still, in several stations and gaoles where the sanitary arrangements have been carried out by most efficient officers, there have been very severe and fatal outbreaks of the disease under my own superintendence, particularly in the cantonments of Peshawur, Kohat, Allahabad, and Gwalior, and in the Central Prison at Agra, which has long been celebrated for the completeness of its sanitary arrangements.

There was great difficulty in carrying out measures for its exclusion even from the gaoles and cantonments; and it is questionable if they were ever strictly observed in large towns. In England the disease must enter by the sea-ports; and the admission of ships with the disease on board is more under control, and should be restricted, and the cases treated on board special cholera hospital-ships. I question the possibility of perfect isolation of any hospital on land, unless it be an island. When the disease has once appeared in a port or town, special cholera hospitals should be established, with which there should be no unnecessary communication. Small detached buildings, tents or huts, are best for cholera cases: the air is purer, and the depression from the sight of the suffering and death of other patients is diminished.

The removal of troops or prisoners is a simple process in India. The supply of tents and means of carriage is ample, and the surrounding country open. In small villages the inhabitants desert their homes and live in the open air; and many who have the means leave the large towns. The precautionary arrangements in towns have been limited to the construction of special cholera hospitals and the general distribution of medicines. People have generally a great aversion to leaving their homes and entering a cholera hospital; but these hospitals were generally filled with travellers and poor people. Restrictions were occasionally attempted in infected localities in the towns, but it is questionable if they did any good. The free use of disinfectants applied to the evacuations has been employed with marked advantage.

In thus prominently calling attention to the communicable nature of cholera, I do not wish it to be inferred that I think it as communicable as many other diseases, or that there is *very* great danger in attending on the sick, especially in Great Britain, where decomposition is comparatively less rapid than in India. I have attended thousands of cases, and only contracted the disease three times, which yielded readily to early treatment. The exemption of hospital attendants, where prompt treatment is available, is remarkable; and the number of relatives and friends who attend in private houses and escape, or only have very slight indisposition, is generally observed. This should encourage all to neglect no duty to suffering humanity, while at the same time all unnecessary exposure should be avoided and every means used to prevent the disease being disseminated to others.

I now come to the most important branch of the subject—the medical treatment, in which the practice has been most uncertain in late epidemics, whilst some are of opinion that it is of little or no use in the advanced stages. This latter statement is not correct, and has a most injurious tendency by depressing the spirits of the patient and destroying his confidence in the physician. This confidence is a most powerful means of supporting hope when life is wavering in the balance. I have seen a patient's life hang on the expression of my face when, in deep collapse, his pulse was imperceptible at the wrist.

My object in giving in detail the symptoms of this well known disease is to enable the early presence of the poison in the system to be recognised, as it is here that medical treatment is most efficacious. It is of little importance that analogous symptoms may be induced by other causes if remedies used be not injurious to health, and they check the further development of this disease, which in an advanced stage is almost beyond the control of medicine. It is our duty to assist Nature and to relieve pain. In the stage of *malaise* the poison is thrown off without any violent or very prominent symptoms by the natural functions of the system. Our task here is to support the strength, avoid indigestible food and depressing causes. The only medicine that I have found useful in this stage is a little quinine every day. The subsequent indications of the treatment are to remove the abnormal symptoms as they appear, of which the most early is *diarrhoea*. The first indication is to check this and restore the case to the stage of *malaise*, then remove the cause and restore the natural secretions. Irritating or indigestible food in the bowels is the most frequent cause of diarrhoea; and should this not previously have been discharged in the evacuations it should be removed and a recurrence of the looseness guarded against, as I have always found it the most powerful exciting cause of collapse. I have found this best carried out by a combination of opium with carminatives in the form of a cholera pill, composed of one grain of opium, two of black pepper, and three of assafoetida. It appears to check the

looseness and stimulate the secretions. This pill does no harm if needlessly administered. It should be repeated should the looseness continue. It will cure most cases, and in all restrain the symptoms until regular medical advice can be procured. This is a most important point in the use of this simple remedy. It may be distributed to every house and be available in a few minutes, whereas the delay of a few hours may allow the disease to advance beyond control. I know no better remedy for this stage. These pills have been distributed in tens of thousands in the towns and villages of India with most satisfactory results. Some surgeons prefer red to black pepper, and others add camphor to the opium and assafoetida, and report favourably of the combination. They are distributed in the dispensaries, and are placed in the charge of the police in India. In this country, similar arrangements might be made.

In collapse, our power is limited by the circumstance that the vital organs are insensible to the ordinary action of medicines. Experience shows that opium, astringents, and alcohol, lie inert in the collapsed stomach, though these are the ordinary remedies for pain, looseness, and debility. It is also my experience that the free use of these remedies at this stage causes death, either by preventing reaction, or by causing local complications should reaction appear.

There is another cause of death which is not generally understood, but which it is in the power of all sufferers or attendants on the sick to check or to prevent. I allude to the extreme danger of assuming the erect posture, or even of sitting up in bed, during collapse, or the earlier stage of reaction. I have seen myself, and I have heard of many cases, where fatal syncope instantly followed sitting up in bed or rising to go to stool.

It must be borne in mind, while indicating the treatment in this stage, that the poison of the disease is contained in the congee evacuations in an active form, and also that the first sign of reaction is coincident with the appearance of bile in the evacuations. The dilution of the irritating contents of the bowels and the restoration of the watery particles of the blood are indicated and best fulfilled by frequent small quantities of cold water, to which a little soda or carbonate of ammonia may be added with advantage. In protracted cases I have seen decided benefit from the use of Liebig's extract of meat, made fresh and given frequently. I have also seen most marked benefit from the exhibition of hot saline enemata given after each motion. In some instances it has acted like magic, the symptoms subsiding after one injection, but in many others they have been powerless. I have thought that the artificial supply of Nature's own remedies in the stage of *malaise*, the secretion of which is suspended by the action of the poison as the disease advances to collapse, might be useful, and the results in a few cases in which they were used previous to my departure for India were highly satisfactory; seven out of nine having recovered, and the two fatal cases having been pulseless and dying before the remedies were used: these remedies were gastric juice and bile, in the form of acidulated pepsine, fifteen grains, and inspissated bile fifteen grains, given alternately every hour. The first dose of bile was followed by vomiting; but bile soon appeared in the evacuations, and mild reaction set in gradually. Shampooing with warm turpentine liniments gives relief to the cramps, and mustard poultices on the epigastrium restrain the vomiting. I think I have used a little quinine with advantage when Nature made an effort at reaction.

I have only time to allude to some of the other remedies which have been highly recommended. I found bleeding unsuccessful in 1833, and I have seen no reason to change my opinion. The practice is generally condemned in India. Calomel I have found inert in collapse, both in large and small doses, and consider that the benefit attributed by many to its use arises from its being employed instead of spirits or strong remedies. There is danger of its being accumulated in large quantities when reaction takes place. Sulphuric acid and acetic acid are less dangerous; but I have not seen decided benefit from their use. I have not found advantage from ammonia, except when added in small quantities to the cold water. I found, in 1833, the transfusion of saline fluids into the veins caused most hopeful reaction; but it was only temporary, and this is the general result of numerous trials made by other medical officers in India. Brandy I consider dangerous in proportion to the quantity given in the stage of collapse, and opium as decidedly poisonous in this stage. Chloroform, though it may give temporary relief, tends to induce dangerous head symptoms on reaction. Astringents are not beneficial. Purgatives are dangerous in the earlier stages, and not useful in collapse; they are generally condemned in India. Heat has been extensively tried by warm baths, but the fatigue entailed is dangerous. It has been tried in the form of hot-air baths, but the result has not been encouraging.

When reaction takes place, rest and careful nursing will complete the cure where collapse has not lasted long; but in protracted cases,

in addition to these remedies, medical treatment may be required for low fever, uræmia, or local complications, regulated by the ordinary rules.

One of the inducements which have led me to write this paper is the hope that the confidence in the treatment of cholera, particularly in its earlier stages, which my long experience has given to me, may be imparted to others through this National Association; and be extended to the people of this country, and help to ward off panic when the disease appears amongst us. I also wish to condemn the use of violent remedies in cases that appear to be hopeless, knowing the dangers they induce, and to recommend the milder expectant treatment which has in many instances been followed by favourable results.

REPORT OF THE COMMITTEE ON THE OBSERVATION AND REGISTRATION OF DISEASE.

THE following report was read at the annual meeting of the British Medical Association at Plymouth, August 1871.

The past year has been full of promise of the ultimate success of the efforts of this Association to bring about a national registration of disease. At the general meeting, held at Newcastle-on-Tyne, on August 12th, 1870, it was resolved—"That a deputation from the Association should seek an interview with the President of the Poor-law Board, to represent to him the views of the Association with respect to the registration of disease, and that Dr. Rumsey, Dr. Sibson, Dr. A. P. Stewart, Dr. Morgan, and Dr. Ransome be requested to form the deputation."

In accordance with this resolution, the above named gentlemen waited upon Mr. Göschen, in conjunction with a deputation on the same subject from the Poor-law Medical Association, and several other gentlemen interested in the question. After a careful inquiry into the views of the deputation, Mr. Göschen said that the arguments in favour of the registration of disease were very clear and strong, and that every one must accept them. The chief points to be considered were by what machinery it could be carried out, how rapidly, and at what cost. He thought, however, that the question was one for the Government, and not for his department, and he remarked that it would be undesirable to go into the matter until the report of the Royal Sanitary Commission had been considered by the Government.

The report of this Commission appeared in the spring of the present year, and contained very important and conclusive arguments for a registration of disease, apparently in every respect identical with that proposed by this Association. We append an extract from this report, from which it will be seen that the Commissioners were fully impressed with the importance of the subject, and with the necessity both of obtaining speedy returns of new cases of diseases occurring amongst large bodies of the population, and of making these returns immediately available for the suppression of epidemic disease. It does not appear, however, from the recommendation of the Commission, that any definite plan for obtaining these objects is put forth, nor any arrangement made for transmitting the records through a local officer of health. Your Committee recommend, therefore, that a memorial be addressed to Her Majesty's Government, praying that in any measure of sanitary reform this most valuable aid to the suppression of disease should be fully provided for, and that arrangements should be made by which health officers could make immediate use of the returns of disease before they are transmitted to the central offices of the Government.

Your Committee regret that they have to announce the discontinuance of the returns made for the last four years by the Northumberland and Durham Medical Society. Although those made at Manchester and Salford, St. Marylebone, and Birmingham still continue, the fact that this influential body is unable to carry on the undertaking is only another proof of the need of a truly national registration of sickness.

But, however complete the registration of deaths may be, it cannot give a fair estimate of the grief and poverty occasioned by sicknesses that are not fatal; it cannot indicate where or how these are to be prevented or remedied; it cannot tell the cost which is worth incurring for their diminution. To these ends, the first step must be a registration, so far as may be practicable, of all the cases of the most prevalent and injurious sicknesses among such portions of the population as may suffice for an estimate of the general state of the public health, and especially of the health of the working classes, and of those for whom sanitary arrangements are most urgently needed.

All the witnesses examined by us on the point have spoken strongly of the need of this registration of sickness. Among the chief purposes it would serve are these:—
1. It would keep the public, and especially the Central Sanitary Authority, constantly aware of the state of the public health in every part of the country. A certain amount of knowledge of this kind is obtained from the registers of deaths, and it would be hard to over-estimate the good service they have done; but the great advantage they can supply is, not only scanty, in comparison with what is wanted and can be had, but is often too late to be useful. In nearly all cases of epidemic and contagious diseases time is lost before the deaths, few in comparison with the cases, begin to attract attention. In many instances weeks have elapsed,

before the existence of widely prevalent and preventable diseases has become known to any efficient Sanitary Authority. Thus, the best opportunities have been lost both of ascertaining the origin of epidemics, and of preventing or limiting their spread. The chances of suppressing an outbreak of disease are in direct proportion to the speed with which it becomes known to a Sanitary Authority; and it is only by a systematic registration of all cases, whether fatal or not, that the speediest information can be attained. The advantage of obtaining such information has been proved in Ireland, where the medical officers of districts, in which epidemic or infectious diseases appear, are bound to report them to the Poor Law Commissioners in Dublin, who at once send down inspectors and, if necessary, assistant medical officers.

2. The registration of sickness would teach more than we can yet guess of the magnitude of the grief and poverty caused by disease. This is very incompletely shown by a register of deaths. For example, in an epidemic of scarlet fever, the deaths are rarely more, and often less, than ten per cent. of the cases. But, reckoned by its money cost, a non-fatal case may be more costly than a fatal one; for, in the one, the cost ends with burial, in the other it has to be borne through the whole period of sickness, and of the often tedious convalescence. Moreover, after every epidemic of scarlet fever, whether it has been very fatal or not, thousands are left disabled for long periods, or for life, with diseases of the ears, or bones, or joints, the glands, or other parts; and the great majority of these are a burden on the working power of the healthy.

What is thus true of scarlet fever is equally true of other epidemics. Their mortality is no sufficient measure of the importance, whether for the sake of humanity or of economy, of preventing or limiting them. In an epidemic of relapsing fever not more than one or two of every hundred cases may appear on the register of deaths; but the cost of the other ninety-eight in every hundred must be enormous, seeing that every one of them has to be expensively maintained for many days or weeks.

Moreover, it has often been observed that an epidemic, even when very fatal, does not add largely, if at all, to the total mortality of a few years. When many die of one epidemic, fewer die of others, or of all other forms of disease. But there is no such counterbalance for the cases of sickness and disability that are left as the residue of an epidemic. These are costly so long as they continue; they are a dead loss.

Terrible as epidemics are, when we count the thousands that have died quickly in them, and guess at the misery consequent on the deaths of those who worked for others, they would seem much more terrible, if we could count the consequences of the necessity of maintaining the many more thousands, who are disabled for months or years from working either for themselves or for others.

3. It is probable that the registration of sickness would show more urgent need than is yet felt for diminishing, if possible, many diseases that, being neither epidemic nor often fatal, are commonly regarded without concern. Ague is very rarely fatal, it counts for little on a register of deaths, yet ague in all its various forms, and its long abiding, is a great hinderer of work, a great burden on the wealth of the nation, a burden which good sanitary arrangements could remove, and probably would remove if it were shown to be worth the cost.

And so of scrofula, of rheumatism and bronchitis, and of many other disabling maladies. We cannot doubt that a registration of sickness would show that not only the personal misery, but the public loss caused by these diseases is enormous. Thousands suffer either constantly or through a large portion of the year. Many of them suffer and are thus disabled year after year, and during all their sufferings they are maintained wholly or in part by the labours of others. It is probable that, on the whole, these diseases are much more costly than the most terrible epidemics; certainly they are so, if we reckon their influence on the progeny of those who suffer. And yet to what amount they are costly, or what cost ought to be incurred for their diminution, we can scarcely guess without a registration of sickness.

4. Another class of cases, of which the death registers do not nearly indicate the importance as affecting the working power of the country, are those of diarrhoea, influenza, and other widely prevalent maladies which, though often trivial to the individual, are very impoverishing in their effects on the people generally. The deaths caused by them are not felt as causes of poverty, for they are fatal to very few except children and invalids and old persons; they are often too few to attract any attention, and every year thousands suffer and are unobserved by any Sanitary Authority. Yet the cost of these diseases in loss of work is so great that it can scarcely be estimated.

5. The registration of sickness would further show whether and in what degree legislation is needed for the prevention of diseases dependent on occupations and social habits. Much has already been done in this direction, by the inquiries directed by the medical officer of the Privy Council, but the effects of many occupations on health are still unknown.

The effects of social vices and vicious habits of life, whether any of them are sources of such sickness, poverty and decay, as to justify on public grounds a considerable expenditure of money and of executive force, cannot be known without registration.

6. In all these, and in many other instances, a registration of sickness would supply a comparatively full knowledge of the prevalence and magnitude of diseases which are imperfectly indicated in the register of deaths, but of which a fuller knowledge might be expected to teach the means of prevention. Equally it would correct some errors which are scarcely avoidable in deductions from the death registers. In these, for instance, each death is registered at the place at which it occurs, whether the fatal illness commenced there or elsewhere. Yet it is obvious that, for the prevention of disease, we ought to know where it began, rather than where it ended. The registration of sickness would tell the former—that of death tells the latter.

The errors hence arising are not trivial. Large classes of persons, including domestic servants and "workers on premises," usually leave the places in which they reside as soon as they fall severely ill. The places in which they die are thus made to appear more unhealthy, and those which they leave less unhealthy, than they truly are.

Similarly, the insane, and many other persons who have been long invalidated with one disease, often die of another, and this is registered as the cause of death. Yet that which it was more important to prevent, because it was the more costly, and wasted, though it did not quite destroy life, was the earlier and longer disease which would have appeared in a register of sickness, but takes a subordinate, if any, place in a register of deaths.

7. A registration of sickness is essential as a test of the fitness of institutions for the reception of the sick and poor. The registration of deaths in various hospitals and workhouses is important; yet it may tell little more than the proportion of cases of every severe disease admitted into them. That which ought to be known is the number and character of the cases of sickness that begin in each hospital, as, for example, of erysipelas, &c., springing up among those admitted for other ailments. 8. And it may be hoped that a registration of sickness would bring to light not only many unknown liabilities to disease, in various places and among various classes, but some immunities, from the study of which means of improving the public health might be derived. There is reason to believe, for instance, that workers in brass are

insusceptible of cholera, that people bred in rural districts are less liable, than those who live in towns, to the blood-infections following injuries. And, if these are facts, it is not likely that they are the only facts of the kind; there must be many more, which a registration of sickness would help to detect.

On all these grounds we recommend that authority and sufficient means should be given to the Registrar-General for a registration of sicknesses, under the direction of the Central Sanitary Authority. But questions may arise as to how far the registration should be carried.

It would be unwise to attempt to register all cases of sickness in the whole population. The number of cases severe enough to disable from work, or to require medical help, is probably in England and Wales not less than thirteen millions a year. The cost of registering so vast a number of facts, however simply, would be far beyond its value.

It would be impossible to register all cases in private houses.

The diseases constantly registered should be those which affect large numbers of the population, which may be considered as, in a greater or less degree, preventable by sanitary arrangements, and which are usually not difficult of discrimination. The registration of other diseases than these should be by a single entry of the total number of "other diseases," but an exact registration of any of them might be from time to time directed by the Central Sanitary Authority.

The registration would, in the first instance, include only or scarcely more than those among whom sickness is, for various reasons, already registered. These would be sufficiently numerous for a general and speedy estimate of the prevalence, in every part of the country, of the diseases which are the greatest causes of misery and the most likely to be prevented by due care. They would include all the sick attended under the poor law; the inmates of nearly all charitable institutions; patients at hospitals, asylums, and dispensaries; the workmen in some mines, factories, and other large establishments in which great numbers of persons are under the care of medical men who are obliged to make returns of all the sick they treat.

In all these cases some sort of record is made of sicknesses; but, however useful the records may be for private or local purposes, they are useless for any general or national purpose, being without uniformity, and therefore unfit for either summary or comparison. In many instances these local records include more particulars than would be necessary for a general registration; such as the name of the disease in every case, the name, residence, occupation, &c. of the patient. Details of this kind would be usefully preserved for local purposes; but the general purposes of the registration of sicknesses would be sufficiently served by a central registration of only the few most important facts, and by the Central Sanitary Authority and his officers having access to all local registers.

The method of registration may be safely left to the department of the Registrar-General. The subject has already been carefully considered in his office; and a member of his staff, Mr. James Lewis, has shown that returns for such a registration as suggested may be collected from all parts of England and Wales, and arranged and published weekly, so as to be at once accessible to both the Central and Local Authorities.

The practicability of such a registration is proved by its having been carried out by the medical authorities in the army, and by private exertions in Newcastle-on-Tyne, in Manchester and Salford, and, for a time, by the London medical officers of health. The system maintained, for the registration of the contagious diseases of animals, in the veterinary department of the Privy Council, a registration, not only of the first occurrence, but of the progress of every case, may be taken for a model of that which it may be hoped, will some day be maintained for the registration of preventable diseases among men.

If the registration of sickness be added to the work of the Registrar-General's Office, it will make the relations of that office to the public health so much larger than to any other subjects, that it would be advisable that the office should be a department under the proposed Central Sanitary Authority.

DRAFT REPORT OF THE PARLIAMENTARY BILLS COMMITTEE OF THE BRITISH MEDICAL ASSOCIATION.

YOUR Committee have to report that, owing to an oversight of the Committee of Council, they were not duly constituted in accordance with the resolution of the annual meeting until the greater part of the parliamentary session had passed. Prior to this, however, the Metropolitan Counties Branch had endeavoured to partly fill the blank by nominating its Parliamentary Bills Committee as formerly. Thus most of the measures introduced into Parliament affecting public sanitary questions or the interests of the medical profession, have in some form received consideration from officers of the Association.

Energetic opposition was made, through the intervention of Mr. Ernest Hart, to clauses in the Coroner's Bill threatening to deprive Poor-law medical officers and others of their fees for *post mortem* examinations conducted for inquests. That Bill was withdrawn. The Committee had great pleasure in warmly seconding Sir Dominic Corrigan's amendments to the Lunacy Regulation Bill (Ireland), and preventing the imposition on the Poor-law surgeons of Ireland of onerous duties without fee or reward. They warmly approve of Sir Dominic Corrigan's Lunacy Bill introduced this session, proposing to afford just remuneration to the Irish Poor-law medical officers for certifying dangerous lunatics, and for improving the superannuation of the medical officers of Poor-law medical asylums. This measure will be reintroduced early next session; and your Committee propose in concert with its author, to give it their warmest support, with whom they are in cordial communication. They have likewise thought it right to give their support to the principle of the measure introduced by Mr. Donald Dalrymple, M.P., for the Restraint of Habitual Drunkards; and Mr. Dalrymple attended a meeting of the Committee, and explained the

scope and provisions of his Bill; and your Committee was represented at the deputation which waited on Mr. Bruce, and elicited the promise of a Select Committee early next session to consider the whole subject.

Your Committee have also supported the appeal for the consideration of measures intended to lessen the present destruction of infant life, and especially the regulation of Baby-farming. The subject was referred to a Select Committee, whose deliberations were much influenced by the disclosures made in the *BRITISH MEDICAL JOURNAL* three years since, and the remedies then suggested. Among the most important evidence taken was that of three members of the Select Committee—Mr. Ernest Hart (the editor of the *JOURNAL*), Mr. Curgenvin, and Mr. Benson Baker; and your Committee rejoice to add that the report of the Select Committee generally concurs in their views and adopts their suggestions. It is believed that the Government will introduce a measure based upon these recommendations early next session.

The amelioration of the condition and the improvement of the system of the Poor-law Medical Service have largely engaged the attention of your Committee. They supported Mr. W. H. Smith's motion, which had at least the effect of bringing on an interesting debate, and obtaining an important declaration from the Government of their intention to consider the propriety of introducing considerable reform or amendments. Mr. F. S. Corrance, M.P., has attended a meeting of your Committee, and has expressed his intention of introducing, on the first opportunity, a measure for the purpose of applying to England some parts of the Irish Poor-law medical system. He was accompanied by Dr. Rogers, the President of the Poor-law Medical Officers' Association. That Association seconds Mr. Corrance's intentions; and your Committee, after consideration of the scheme advocated, believe that it will be beneficial, and propose to support it at the proper time.

The larger questions of Poor-law Medical Reform, and of Medical Reform properly so called, as well as of State Medicine, belong to separate Committees of the Association, and have not, therefore, been dealt with by this Committee.

Mr. Stansfeld's Bill for consolidation of the public departments of Medical Relief, Poor Relief, and Public Health, came under consideration. Some objections were urged from quarters deserving respect. Your Committee, however, felt it their duty to support the measure as one of immediate and obvious advantage.

The Pharmacy Acts Amendment Bill and the Licensing Bill came under consideration on two occasions. Both have subsequently been withdrawn; but your Committee made some representations to Government as to necessary amendments, which were promised due consideration, and which will be repeated, if necessary, should those measures be revived.

As to the Vaccination Acts Amendment Bill, the Committee objected to the principle that any one should be allowed to purchase immunity from the operation of the Act by payment of a small monetary fine. They rejoice to find that the Bill proposes to authorise district medical officers to vaccinate those liable to take small-pox in houses where the disease is present, and to make payment to them as though they were public vaccinators. This clause was pressed upon the Government prior to the passing of the Bill by a deputation of the Poor-law Committee of the Association, acting in conjunction with a deputation of the Poor-law Medical Officers' Association.

Thus the session has afforded ample scope for the activity, and evidence of the usefulness, of this Committee of your Association. There is no other body possessing a similar representative character charged with the duty of watching over legislative measures affecting public sanitary interests and the interests of all classes of the medical profession. Your Committee, recognising the importance and responsibility of the task, will continue, if reappointed, to perform the duty with care and diligence. They wish to impress upon the associates at large, and upon members of the profession generally, the importance of freely communicating with them upon all measures introduced into the legislature affecting individual or collective professional interests.

DRAFT REPORT OF THE POOR-LAW COMMITTEE OF THE BRITISH MEDICAL ASSOCIATION.

YOUR Committee have to report that, having been convened at the request of the Chairman of the Poor-law Medical Officers' Association, they have met the Council of the Association in conference on two occasions, and have made representations to Ministers which have not been without effect.

On October 14th, they met to consider a scheme of reorganisation of the Poor-law Medical Service which had been carefully considered by Dr. Rumsey, Dr. Ransome, and other authorities, bearing partly upon this general question, and partly upon the question of Registra-

tion of Disease. This memorandum they presented, through a numerous and influential deputation, to Mr. Goschen, then President of the Poor-law Board. Dr. Sibson, Dr. Farr, Dr. Ransome, Dr. Morgan of Manchester, Dr. Phillipson of Newcastle, Dr. Stewart, and other influential members of the Association, expressed their views. The result of the deputation is reported at length in the *BRITISH MEDICAL JOURNAL*, October 22nd, 1879. It was highly favourable to the general views of the Association, and promises substantial results.

On a second occasion, your Committee met the Committee of the Poor-law Medical Officers' Association in conference on the subject of the Vaccination Regulations, and had a lengthened interview with Mr. Simon, Medical Officer of the Privy Council. The special subjects discussed were the consolidation of vaccine statements, the regulations as to gratuities, and the desirability of relaxing the rigid rules as to stationary vaccination. The deputation was received with great courtesy by the Medical Officer of the Privy Council, who discussed with them in detail the points at issue. As results of this deputation, they may point to the clause in the Vaccination Amendment Act authorising district medical officers to vaccinate in houses where small-pox is present, and to claim payment for the same. The medical officer also announced his intention of codifying and publishing the regulations concerning gratuities to public vaccinators.

Your Committee regret that they have not been constituted and called together in a more regular manner. They believe that it will be to the advantage of the profession that they should be reconstituted, in order that the Association may continue to lend its powerful aid to the officers of the Poor-law Medical Service.

They regard with satisfaction the present organisation of the English and Irish Poor-law medical officers, and trust that the Association will continue to assist with all its influence the efforts of those Associations to elevate the status of the service and increase its public usefulness.

ERNEST HART.

REPORTS

OF

MEDICAL AND SURGICAL PRACTICE IN THE HOSPITALS OF GREAT BRITAIN.

ST. BARTHOLOMEW'S HOSPITAL.

CASE OF HYDATID TUMOUR OF THE LIVER, WITH ICTERUS.

(Under the care of Dr. DUCKWORTH.)

THE notes of this case were kindly supplied by Mr. G. Bagot Ferguson.

Emma R., aged 25, married five years, was admitted on May 1st, 1871. She had one miscarriage three years ago. She was born in, and had always lived in, London. She could lie without discomfort in any position. The face was considerably jaundiced, as also was the body generally, the chest being most deeply stained. The jaundice commenced about four months previously, increasing, with intermissions, up to the time of admission. The ocular conjunctivæ were deeply stained. The lips were dry; the tongue clean and moist, fissured longitudinally. There was no œdema of the feet and legs. Her liver was palpably much enlarged, the right lobe reaching within about three inches of the crest of the ilium in the mesial line to about two inches above the umbilicus; the left lobe reaching far into the left hypochondrium, and about four inches below the cartilages. The upper limit of the liver was normal. The abdomen over the liver was tense; and, on percussion, the so-called hydatid fremitus was evident, chiefly over the enlarged left lobe. The abdominal tension was greatest, and the prominence most marked, in the mesial line, about two inches below the ensiform cartilage. A rounded mass at the lower right border might possibly have been the distended gall-bladder. Her girth in the widest circumference was thirty-two inches. There was no ascites, and no enlarged abdominal veins were visible. The appetite was fair; no thirst; bowels open. The urine was extremely dark in colour; it had been thus only a few days. The catamenia were regular and normal. She complained of pain at the seat of the tumour, and a cramping sensation and feeling of weight. She slept badly, and suffered much from nightmare. The tumour had increased mostly during the last three or four years; but she thought it was there, though small, as long as seven or eight years ago, because of uncomfortable sensations of weight in its present site, and from the great tendency which she had to nightmare. Its growth had been attended by no actual pain. At a place where she lived as housemaid six years ago, three dogs were kept, which she petted much; she did not know if they had worms.

May 14th. She was tapped by Mr. Holden with a Thompson's