supply you not only with their authors' experience, but with what they have gathered and set in order from the writings of others. Limiting myself to what I have studied in my own cases, I must omit many things besides ruptured and wounded intestine; such as the various complications of strangulated hernia with hydrocele and misplaced testicle, with varicocele, and with accidents of the operation, such as hæmorrhage from the epigastric or the obturator artery. Some of these things I have never seen; others I have seen only once or twice, and have learned concerning them nothing but what you may learn by reading the works to which I have referred you.

But, as to the treatment of sloughing and ulcerated intestine of which I was just speaking, I will only say that it has always seemed to me more prudent to incur a great risk of having a permanent external fecal discharge by leaving the intestine at the wound, than to add to the risk of life by returning any thing which it may seem possible to repair by suture or any such means. Of course, these means are not to be thought of if the sloughing or ulceration be of more than very small extent; but even in the smallest, unless in some very rare cases, I would not add to the inevitable risks of life by returning the damaged intestine. In cases of hernia, the saving of life is so much more important than anything else, that we ought not to incur a risk of life for anything less than the highest probability of saving a patient from some life-long distress.

CROONIAN LECTURES

ON

DISEASE AND ITS MEDICAL TREATMENT.

Delivered at the Royal College of Physicians, London.

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LECTURE III.—(Concluded.)

BUT it has been assumed, partly from the tendency which many of these affections have to be attended at about the time of their greatest severity with an attack of diarrhoea, an outburst of perspiration, or a large flow of urine (discharges which, from their frequent relation to commencing convalescence, have been called "critical"); partly from the fact that, in the larger number of them, a rash forms on the skin, or some corresponding condition affects some of the mucous membranes, and that in these affected surfaces the contagium of the disease chiefly resides; and partly from the well-known fact that many inorganic poisons received into the system pass out of it again by certain selected routes; that their natural cure depends on the elimination, by the skin or the mucous membranes, of the poisonous or contagious material which has developed within the body, and that hence their cure can be promoted by the encouragement of that elimination.

The theory of elimination is an old one; but it has acquired a special interest during the last few years from Dr. George Johnson's application of it to explain some of the symptoms of cholera, and the principles of its treatment. His views may be epitomised as follows. "Cholera is due to a morbid poison which has been received into the blood, which multiplies in the blood, and, ere long, escapes from it at the mucous surface of the bowels, causing in its escape the copious liquid evacuations which are so characteristic of the disease. This process is a curative one, and as such it is dangerous to check it, beneficial to promote it. The choleraic collapse is not dependent on the diarrhoea, indeed, is often in inverse proportion to it, but is caused by the contraction of the pulmonary arterioles, which is in its turn excited by the poisonous matters in the blood, and prevents the passage of that blood into the pulmonary capillaries." With some parts of this theory I have no present concern; and, as regards the rest of it, it must, I think, be fully admitted that, if cholera depend on a poisonous something which multiplies in the blood, and, by its presence there, causes the characteristic symptoms of the disease, and that, if recovery from the disease depend on the separation of this poisonous something from the blood at the mucous surface of the bowels, it would probably be injudicious to adopt any plan of treatment calculated to arrest this eliminative process. But I must confess that I am not at all prepared to admit that this chain of hypotheses is sound in all its links.

I have already, in my first lecture, briefly considered the modes by which the organised particles which constitute the contagia of infectious diseases become introduced into the blood, and the modes by which their numbers may be supposed to be augmented or kept up in that fluid. I have pointed out, also, that their residence therein must be of marvellously short duration, since they cannot be detected in the circulating blood in the most virulent fevers, even at their most infectious stage. They cannot (I believe) be recognised therein by the microscope; and the blood from patients suffering from infectious disorders is rarely, if ever, capable of inoculating the disease upon healthy persons. The same fact holds good, as I have shown, in respect of carcinomatous and other forms of malignant tumours, and probably also of tubercle; the germs of which, developed in diseased lymphatic glands, are thrown off from thence into the blood, and are conveyed by the blood to distant organs, where they take root and produce secondary tumours, and yet are never discovered in the blood itself. The remarkable freedom which the blood of carcinomatous patients, and that of persons who are suffering from infectious diseases, manifest from the specific poisonous matters which are the respective causes of these diseases, renders it, as it seems to me, most improbable that their characteristic phenomena are due to the presence of their respective poisons in the blood, or to the direct operation of these poisons on the blood. As regards carcinoma, it is certain that, so long as its germs are retained within the blood, they cause no symptoms—at least we have not the slightest evidence to show that they do. It is true that Mr. Simon, many years ago, propounded the ingenious hypothesis that a carcinomatous tumour is an eliminative tumour, that it becomes developed as the result of some antecedent cachectic condition of system, dependent on the formation of some poisonous material in the blood, simply and solely to act as an organ for the elimination of that supposed poison from the system. But that hypothesis has never been accepted; and Mr. Simon himself has, I believe, long ago given it up. Again, the syphilitic poison—at least so far as I know—has no important effect so long as it is retained within the blood-vessels. It is generally acknowledged, too, that even when the cause of inflammation is a poison carried by the blood, the changes which constitute inflammation commence only in the tissues outside the blood-vessels, and, consequently, after the diffusion of that poison amongst them. And, I may add, as regards unorganised poisons which gain an entrance into the blood, that all those the action of which can be best traced by characteristic symptoms, obviously produce these symptoms neither by their action on the blood nor while they are in it. I do not, of course, for one moment deny that poisons act upon the blood; still less do I deny that, in the course of inflammatory, exanthematous, and other diseases, the blood becomes materially affected, and perhaps so much affected as to be incompatible with the maintenance of life; but I contend that these obvious and indeed coarse affections of the blood are not (except in a very slight degree) specific, but are due mainly to the rapid destructive processes which are going on in the tissues, and to the insufficient removal from the blood of the products of these processes. Indeed, it seems to me clear, not only from the facts of infectious fevers themselves, but from the analogies afforded by allied diseases and morbid processes, that the characteristic symptoms of these fevers do most certainly not arise so long as their respective specific poisons are merely circulating in the blood.

That poisonous or foreign matters which have entered the circulation tend rapidly to leave it, is a fact which has now been quite clearly established, not only for those which are inorganic, but for those which are organic, and even for the contagia of infectious diseases; but, whether after their escape from the vessels their operation on the solid tissues is beneficial, or injurious, or *nil*, depends necessarily on a variety of circumstances. Thus some of the medicines called diuretics escape rapidly through the kidneys, simply carrying with them an increased quantity of the water of the blood; others, however, like cantharides, cause irritation and inflammation of the organ in their passage through it. Again, the salts of silver become deposited, in the form of an insoluble sulphuret, in the walls of the capillary vessels of various organs and in other tissues, and remain there permanently and perfectly inert; but lead causes colic when it is deposited in the muscular parietes of the bowels, and dropped hand when it is precipitated among the extensor muscles of the fore-arm and hand. And how does mercury or iodine cure a syphilitic gummatous tumour, but by acting directly upon its tissues? Again, the germs of malignant neoplasms and of syphilitic growths develop secondary tumours only after they have escaped from the blood. They then infect the solid living tissues, irritate them into specific proliferation, and produce growths which are entirely extravascular, or rather, perhaps, outside the blood; but which, yet, acting in conjunction probably with similar growths in other parts, react on, or affect secondarily, the general nutrition of the body

in a very remarkable way. And is it not the same precisely with, at all events, many of the infectious fevers? A susceptible person inhales the contagium of measles, small-pox, or scarlet fever: for some days after he has received the poison, he remains apparently still in the best of health; and yet we know that during that time important morbid processes are going on, processes which are occurring probably partly at the seat of inoculation, partly in the lymphatics, partly in the blood. Then almost suddenly there is an increase of temperature, and an accession of symptoms which are more or less characteristic; and, in a day or two, characteristic inflammation of the skin and of some of the mucous membranes. Now surely here, as in carcinoma and syphilis, all that renders each of these diseases characteristic, all that renders them dangerous to the individual who is affected, and nearly all (if not all) that renders them sources of danger to others, is in intimate relation, not with the processes which are going on within the vessels, but with those which are occurring external to them, those which are dependent on the escape of the contagia from the vessels.

All that has hitherto been urged may probably be regarded as compatible with the theory of elimination. It might be admitted that, in the affections to which this theory relates, there is a poison in the blood which, so long as it remains in the blood, produces little or no injury, and that all the symptoms of disease manifest themselves only when this poison has left the vessels; and yet it might well be maintained that the poison, having entered the blood, must, in the nature of things, depart from it, and hence that the symptoms which attend its departure must be encountered if the patient is to recover, and that, the more rapid this departure can be made, so much the better for him. I am not, however, quite prepared to admit the force of this argument, even if it could be shown that we have to deal in each case with a certain measurable quantity of poison, which, having been manufactured in the blood, can, without quantitative change, be separated from it. But, believing as I do that the chief multiplication of the poison takes place in the solid tissues of the body during the eruptive stage of the exanthemata, and during the corresponding stage of those allied diseases which are not characterised by a specific rash, I must confess that the argument seems to me to be of no weight. And here again, in support of my view, I must appeal to the analogies afforded by other kinds of disease, especially those in which the morbid processes have a tendency to become generalised. In carcinoma, in sarcoma, and in syphilis, I have already shown that this so-called "elimination", so far from being curative, is, in fact, at each point where it occurs, the starting-point of new outgrowths. It seems to have been proved conclusively by Cohnheim and his followers that the escape of white corpuscles or leucocytes from the small veins and capillaries forms an essential part of the process of inflammation; and the proliferation and the suppuration which attend inflammation would thus seem due (to a considerable extent at least) to the escape and action of these bodies. But does any one suppose that this elimination of white corpuscles is in the least degree curative or beneficial, or that it hastens the progress of the inflammation? Is it not, indeed, obvious that after their escape changes occur which it would have been far better to have prevented had it been in our power to prevent them? But I will pass from these affections to one of the exanthemata—small-pox. What happens here? The patient receives the poison, and after a certain time peculiar symptoms reveal themselves, followed by a rash. The rash is first papular, soon becomes pustular, and at the end of about a week attains its full development. It then scabs, and, after a certain period, the scabs fall off, and probably somewhere about the same time the patient is well or far advanced in convalescence. On the eliminative theory, it is of course assumed that the poison of small-pox, discharged from the blood, produces the cutaneous inflammation as a necessary step towards its final discharge from the system. But obviously that is not the fact. No doubt the contagium of small-pox does escape from the vessels, and no doubt it produces inflammation in the skin; but it is at least equally certain that the skin thus inoculated by the virus received from the blood becomes itself a most active manufactory of the variolous poison. It can hardly be seriously argued that all the variolous contagium of each variolous pustule is simply separated from the blood; that each variolous pimple is a mere organ of secretion acting until the blood is purified; that the theory that cancer is an eliminative growth (long since proposed and discarded by Mr. Simon) should be applicable to the pustules of small-pox! The answer, however, to any such argument is plain. You inoculate a patient with small-pox, and within the course of a week a variolous pustule becomes fully developed at the point of inoculation; and a little before the time at which the primary pustule becomes perfected, the rash commences in a papular form over the general surface of the skin. The primary pustule goes through precisely the same stages as the pustules which subsequently appear in consequence of the generalisation of the disease. Now there can be no

doubt that in this case the primary pustule is due to the direct multiplication of a portion of the poison inoculated at the spot; and it follows almost as a matter of course that the generalised pustules of the later date are due to the elimination of a similar poison from the blood, and to the fructification (so to speak) of this eliminated poison. But if this be true of small-pox, surely it must be equally true of scarlet-fever and of measles; and there is no good reason why it should not be equally true of whooping-cough and mumps.

And let me ask whether this and all the preceding arguments are not applicable with equal force as an explanation of the phenomena of cholera. I admit that in this disease a specific poison gains an entrance into the blood; I admit that it has a marked tendency to become eliminated from the blood at the alimentary mucous surface, and that this process of elimination is an essential step in the progress of the disease. But I do not think that there is any proof that the poison acts injuriously while it is retained in the circulating fluid; and I do think (judging mainly from analogy) that it produces its characteristic effects when it escapes from the blood and acts on the solid tissues. That the poison of cholera undergoes development after its escape from the blood, is indeed proved by the fact (which seems to have been clearly established, both by the experiments which have been made upon mice and other animals, and by the experience which was gained with respect to the behaviour of cholera in our last and in the previous epidemics) that choleraic evacuations become more and more effective in producing cholera up to the time at which (after the end of a few days) they are rendered inert by the advance of putrefactive changes.

But a further argument in favour of the view for which I contend—namely, that the contagious particles which are received into the blood, and which multiply either in the blood or in the organs which supply the blood, sow themselves, as it were, in certain tissues external to the vessels and there germinate, rather than merely eliminate themselves—is furnished by a fact which is special to the exanthemata (using this term in its widest sense); the fact, namely, that the foci of contagion which they evolve never reinfest the individual in whom they are evolved. Thus the primary pustule of small-pox (resulting from inoculation) throws out effective contagious particles into the system; but the precisely similar pustules which form at a later date, and it may be in close proximity with that which was first developed, have no such ulterior influence. Absorption by the lymphatics is a more passive unselective act, so that we have no right to doubt that absorption actually does take place from the secondary pustules, just as it originally took place from the first; but whatever thus becomes absorbed, fortunately for us, withers and remains inoperative. It may be said that this fact is connected with the protective influence which an attack of any one of these diseases exerts against any future attack of the same disease; and that, of course, is true: but neither fact explains the phenomena; and both of them show that an essential feature in these diseases is, that their duration is limited—a limitation which is determined not by any tendency for the vastly multiplied poison to pass off like urea, or iodide of potassium, or chloroform, by certain emunctories, but by the fact that the development of the poison exerts some occult influence on the system, which gradually renders it unfitted for the further growth or maintenance of the poison; and hence that the cessation of the disease is not dependent on the elimination of the poison, but on the supervention of a condition of system which renders the poison no longer operative.

The conclusions to which my long, I fear tedious, argument has led me, are incompatible with a belief in the value (in reference to diseases dependent for their origin upon contagia) of eliminative treatment. But, theory apart, I cannot think that the results of practice are at all clearly favourable to it. In several of the exanthematous diseases, the chief development of contagium takes place at the surface of the skin, and there, if anywhere, elimination is taking place, and ought to be encouraged. Now what is better adapted to encourage proliferation of tissue than warmth and moisture? What is more likely than these to promote the rash of small-pox, or that of measles or scarlatina? There was a time, indeed, when patients with these diseases used to be shut up in hot close rooms and packed in bed-clothes, but the advancing enlightenment of our ancestors proved to them the impropriety of this treatment; and now, I suppose, every physician, as a simple matter of course, treats his patients on exactly opposite principles, and thus wisely discourages elimination. Has any one ever thought of thus trying to eliminate the inoculated cow-pox? Is it not certain that such attempts at elimination would be attended with an increased development of the pustule, and that the process of elimination and of increasing the disease would in this case be identical? Or, again, has anyone ever attempted to eliminate mumps by promoting saliva or semen? or diphtheria by encouraging the development of its characteristic false mem-

brane? And yet, if the theory of elimination be true, I conceive that this should be the proper indication for treatment.

I must add, in conclusion to this part of my subject, that I cannot recognise any sufficient grounds for regarding the discharges of cholera as any more curative than the eruption of small-pox or the membranous exudation of diphtheria; and that, just as I should look upon the specific exudations of these latter two diseases as a source of danger to the patient, and should do all I could to moderate their development, so I should look upon the alvine discharges in cholera as detrimental, and should wish to restrain them. It does not appear to me that the fact (which is abundantly proved), that many of the most rapidly fatal cases of cholera are fatal without the previous occurrence of much, or even of any, intestinal evacuation, in any sense proves either that such discharges are not a source of great danger, or that they are to be regarded as eliminative. Such cases, so far as I can see, have simply the same relation to the ordinary run of cholera cases as those terrible cases of small-pox, or of measles, or of scarlet fever (in which the patient is struck down at once by the intensity of his attack, and dies within a day or two and before the appearance of the rash), have to the more common kinds of cases, in which the diseases take their typical course and present an orthodox and well developed rash. And I distinctly, therefore, range myself on the side of those who believe, on theoretical grounds, in the uselessness of the purgative treatment of cholera, and who fail to recognise in recorded experience any evidence of the superiority of this over other plans of treatment. I venture, indeed, to go further, and to record my entire disbelief that you can in the so-called "premonitory" stage of cholera prevent the subsequent full development of the disease, either by purgatives which eliminate, or by astringents which restrain; or that you can, when the disease is thoroughly established, influence its progress in the least by remedies, whether they be stimulants or sedatives, astringents or purgatives, administered by the mouth; or that, lastly, there is the least benefit to be derived, as a rule, in the later stages of cholera from the employment of opening medicines to remove poisonous matters retained in the bowels. In the premonitory stage, the poison is in the system and not in the bowels; and no purgative is more likely to expel it thence than, in the corresponding stage of variola, a diaphoretic is likely to eliminate the poison of that disease by the skin. In the second stage of the disease, when the congested mucous membrane of the bowel is pouring out, or tending to pour out, enormous quantities of fluid, and when, therefore, absorption at that surface is utterly impossible, it is obviously useless to give medicines which only operate after they have been absorbed; and it is surely about as reasonable to aim at checking or preventing the discharge by remedies which act locally, as to endeavour to hasten or retard the downward rush of a mountain torrent by throwing your astringent or your purgative medicines into that. As regards a later stage still of the disease, I would rather limit myself by no definite rule: I would only repeat that the belief in the beneficial effects of purgatives, then, seems to me based on the erroneous notion that the effete matters which are contained in the bowels, and which might be poisonous to others, are equally poisonous to the individual—a belief which has, I conceive, no more solid foundation than that which could assign similar self-poisoning properties to the squamæ of scarlet fever and the scabs of variola. If the essential object in the treatment of cholera be to promote fluid discharges from the bowels, and thus eliminate the poison, would it not be better attained by injecting fluids into the veins, and thus feeding the alvine discharges, than by administering drugs by the mouth, and thus launching them on a journey which must be utterly barren of result?

It was my intention, when I commenced the preparation of this course of lectures, to devote a portion of the time allotted to them to the discussion of the value, in a remedial point of view, of certain agents or measures employed in the treatment of diseases, and more especially of the value in this respect of counter-irritation, bleeding, alcohol, and diet. I regret that I have treated of other matters at so great a length as to be compelled on that account to forego this intention.

It will have been observed, I hope, that, although in the course of my last lecture and of this I have dwelt chiefly upon the fact, as I believe it to be, that a very large number of the diseases which we are called upon to treat, inclusive even of such as have a natural tendency towards recovery, are absolutely incurable by our art, I have at the same time (though much more briefly, and much less systematically and infinitely less forcibly than facts warrant) pointed out that in almost all of them the skilful physician has numerous opportunities of exercising his skill for the patient's benefit; that, when recognising the nature of his patient's illness, he can guard against and avert threatened complications; and that during its progress he can treat, and treat successfully, many of these complications and many symp-

toms which distress his patient or endanger his life; and that by such means he may often be enabled, when he has to do with diseases which are dangerous but yet tend towards recovery, to cure his patient, although incompetent to cure the disease under which he is labouring; and further, that I have pointed out (although this has formed only an incidental part of my subject) that there are many diseases—especially among those which may be denominated functional—which it is absolutely within our competence to cure.

It will also have been observed, I hope, that in the course of my observations I have more than once dwelt upon the extreme importance of endeavouring to maintain or promote the patient's general health and strength, by regulating the action of his emunctories, by soothing his mind and nervous system, by improving the tone of his digestive organs by means of such tonic medicines as are admissible, and by the regulated administration of food. Of course I know that in a large number of cases we fail to effect any of these objects, and that in many we effect them only in a very limited degree; but if we effect them at all, there is no doubt that we materially benefit the patient. And indeed it will be universally admitted, that the more we can improve our patient's health and strength generally, the more likely he is to battle successfully with his disease, and the less likely he is to suffer from many of its complications and sequelæ. We know, for example, how apt dropsical accumulations, the result of incurable organic disease, are to come on prematurely, under adverse conditions of health, and how often they disappear as the health of the patient becomes improved, and how often they may be kept in abeyance under a continuance of the measures which have brought about this improvement. We know that it is mainly under the influence of such treatment that phthisis and many other chronic and exhausting diseases become arrested in their progress, and in some cases finally cured; and we know also that it is due largely to the adoption of such measures, and especially to the judicious administration of appropriate nourishment, that many a patient suffering from inflammation, and many a patient suffering from fever, is saved from death during the severity of his disease and launched into safe convalescence.

In bringing this course of lectures to a conclusion, I cannot but express the fear that much which I have said in them has been little likely to command the sympathy or acquiescence of those whom it has been my privilege to address. It is easier to pull down than to build up; and there are many more persons in this world who are competent to do the former than there are persons who have sufficient genius to effect the latter. I am afraid that I belong, both by ability and by inclination, to the larger class. I trust, however, that in giving utterance to my own opinions, I have used no expressions which might seem disrespectful or contemptuous towards those of others. I certainly entertain no such sentiments towards the opinions which I have most freely criticised; and I fully acknowledge to myself that those who hold them are at least as much entitled to hold them as I am to hold mine, and that at all events in this matter I have no claim to be the judge. I cannot pretend to deny that I am more sceptical than most physicians in regard to the value of drugs; but I declare from the bottom of my heart that I am not sceptical in regard to the medical skill of my colleagues in this college, and that I am not sceptical in regard to the dignity and importance of the profession to which we all belong. I am thankful, indeed, to acknowledge that there are in this country many educated medical men, to any one of whom I might, while suffering from dangerous sickness, trust myself implicitly; who, however much they might differ from one another and from me in respect of belief in the efficacy of certain drugs, would yet certainly not fail to grasp all that was of essential importance in my case, and to restore me to health, if such restoration were possible.

POISONING BY BELLADONNA PLASTER.

SOME months since I prepared a small belladonna plaster, about the size of a crown-piece, by spreading the extract with my right thumb upon a portion of adhesive plaster. Whilst on my round, I was seized with dizziness and dimness of vision, especially in the right eye. I suspected that these symptoms were produced by spreading the small belladonna plaster. I at once returned home and examined my pupils with a mirror, although with a little difficulty. I then found the right pupil much dilated, whilst the left pupil was of the usual size. These symptoms in a short time passed off. I should observe that there was no wound or abrasion of the thumb. The inference to be drawn is, that there is a great susceptibility to the influence of belladonna, when applied externally, and that the application of a belladonna plaster, even of moderate size, might be followed by dangerous symptoms.

C. A. HEMINGWAY.