CLINICAL LECTURES
ON
STRANGULATED HERNIA.

Delivered at St. Bartholomew's Hospital.

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LECTURE III.—(*Concluded.*)

IF the reduction be accomplished without opening the sac, you will have attained the best immediate object of the operation; but remember that fallacies of reduction are possible here as well as in the cases in which no operation has been done; they are, however, less mischievous, for, if the stricture be completely divided, there will be no strangulation of whatever remains in the sac. Especially you must have no fear if, as commonly happens, after returning intestine, some omentum remain in the sac. This will do no harm; but if more than omentum have remained in the sac, and the signs of strangulation be not relieved or lessened, you must operate again and open the sac, regarding these cases in the same light as those of partial or doubtful reduction, of which I spoke in the last lecture. But suppose the sac opened, as it should be in nearly all bad cases, and in many which, though they are not bad, yet may be called difficult, here may occur the most difficult question of all, What is to be done with the contents of the sac? Of course, in most cases you are to return them; but in many you are not, and which are which?

Look first to the character of the fluid which, in most cases, you will let out of the sac. In most cases, not in all; for, in some small femoral herniæ, especially in very thin dry people, and in many umbilical herniæ, and in any that contain a large quantity of omentum, there may be no fluid, or too little to be distinctly seen; but if there be enough to judge from, you may deem it a good sign if the fluid is clear, and yellowish like serum, or, rather, like liquor sanguinis—for it will coagulate spontaneously. This indicates only such an exudation of fluid as may come from a simply congested piece of intestine, or from a piece not badly inflamed; and the cases would be very rare, if there can be any, in which intestine found behind fluid such as this might not be returned. The same may be said when with fluid such as this there are flakes or bands of lymph or fibrinous exudation; for these tell of only such inflammation as may safely be recovered from when the intestine is returned. I am disposed to say the same of the cases in which the fluid is clear, but more or less deeply blood-stained; for this exudation of blood-cells or blood-colour is not characteristic of any serious morbid change in either the sac or its contents. But when the fluid of the sac is turbid, brownish, muddy, it tells of more advanced changes in the intestine or in the omentum; and the further it goes in this direction the more carefully must you consider whether these are in a fit state to be returned. You will probably have to decide that they are not fit, when the fluid has a distinct fæcal or putrid odour; and of course they are not fit when the fluid has fæcal matter mixed with it.

I do not venture to say that the characters of the fluid contents of the sacs of strangulated herniæ are to be absolutely relied on as guides for practice; but they are good evidence to be taken into the general account, for they fairly represent the state of mere congestion or inflammation, or more or less advanced decay or decomposition, or giving way of the walls of the strangulated intestine and omentum.

Not rarely, when you have divided the stricture and returned the contents of the sac, fluid runs from the peritoneal cavity. I do not know any rule of practice but that you must let it run as long as it will, and, if it be of very unsound appearance, not close the wound till the fluid has ceased to flow, if even then.

As to the omentum which the sac may contain, and what to do with it: if there be a small quantity—say two or three square inches—and this be not adherent, and not more changed than by congestion or slight inflammation, there can be no question that you are to return it after the intestine; and if there be a piece of even very large size, and not more changed in texture, you had better return it if you can without much force or expense of time. But it sometimes happens, when the abdomen is tense with over-filled intestine, that you cannot return a large piece of omentum without much difficulty. What then? shall you cut it off or leave it in the sac? I advise you to leave it. I believe that the cutting off, with the necessary ligatures or other fastenings of vessels, adds to the dangers to life; while the leaving of omentum is only sometimes followed by greater difficulty in the fitting of a truss—a difficulty which is not great enough to justify any risk of life.

Still more may this rule of leaving omentum in the sac be observed when a large piece of it is hardened and thickened as by old disease. When a small piece is thus changed you may, I believe, return it.

When omentum is adherent to the sac, but in other respects fit to be returned, you should break the adhesions and return it, after stopping all bleeding. If it be not fit to be returned, leave the adhesions; and in any case do not break adhesions so near the mouth of the sac that their vessels are likely to bleed into the abdominal cavity.

When omentum is sloughing, or nearly sloughing, leave it, that it may cast its sloughs outward.

But the chief questions in these operations are concerned with the state of the strangulated intestine and the manner of dealing with it. You are to judge chiefly from the colour and the tenacity. Use your eyes and your fingers; sometimes your nose; very seldom your ears, for what you may be told about time of strangulation, sensations, and the rest, is as likely to mislead you as to guide aright.

As to colour, any tint, from the natural grey through various shades of rosy or ruddy pink, or redness, up to the deepest crimson, even verging to blackness, may be consistent with fitness for returning of the intestine, if the texture be good. All these tints may be due to congestion and stagnation of blood, or to extravasation of blood into the intestinal walls; and all these may have been without such inflammation as would spoil the texture of the intestine, and may not have endured long enough to kill it. I am disposed to say that you may return intestine of any colour short of black, if its texture be good; if it feel tense, elastic, well filled out, and resilient, not collapsed or sticky; and the more the surface of the intestine shines and glistens, the more sure you may be of this rule.

When a piece of intestine is thoroughly black, I believe you had better not return it, unless you can be sure that the blackness is wholly from extravasated blood. It may not yet be dead, but it is not likely to recover; and, even if it should not die after being returned, there will be the great risk of its remaining unfit to propel its contents, and helping to bring on death by what appears very frequent—distension and paralysis of the canal above it. But, indeed, utter blackness of strangulated intestine commonly tells of gangrene already; and of this you may be sure if the black textures are lustreless, soft, flaccid, or viscid, sticking to the fingers or looking villous. Intestine in this state should never be returned.

Colours about which there can be as little doubt, for signs of gangrene, are white, grey, and green, all dull, lustreless, in blotches or complete over the whole protruded intestine. I cannot tell why there should be so many colours in different cases, or sometimes even in the same case; but all are alike certain signs of gangrene, and they are always combined with loss of due tone and texture of the intestinal wall. Intestine with these marks, even though they be small, must not be returned.

Then, as to the texture of the protruded intestine: it should be, for safety of return, thin-walled, firm, tense, and elastic, preserving its cylindrical form, smooth, slippery, and glossy. The further the intestine deviates from these characters, the more it loses its gloss and looks villous; the more it feels sticky, and is collapsed and out of the cylinder form, the softer and more yielding, the more pulpy, or like wet leather or soaked paper, the less it is fit for return. And when these characters are combined with so many colours as I have described, the intestine must be taken to have perished, and had better be laid open, that its contents may escape externally and do no harm.

But short of gangrene, there may be ulceration of the walls of the intestine. The usual place for this is where the intestine is girt by the mouth of the sac, and it is most frequent in femoral herniæ long strangulated. In these it is especially the sharp hard edge of Gimbernat's ligament, which seems to cut into the intestine, thinning its wall and at last piercing it: and the chance of this having happened is enough to justify the rule that, where the strangulation has been sharp and long, the intestine should be gently drawn down after the stricture is divided, in order to see that there is no great injury of its walls where the chief pressure of the stricture has fallen on them.

Here, too, because they are similarly dangerous to life, I might speak of laceration of the intestine in too violent attempts at reduction, or wound of it in operation; but I have no personal experience of such cases, and can add nothing to what you may read in the best treatises on hernia—such as that of Sir William Lawrence, or in the chapters devoted to hernia by Mr. Erichsen in his *Science and Art of Surgery*, or by Mr. Birkett in Holmes's *System of Surgery*. These will

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 fæcal 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 diarrhœa, 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 diarrhœa, indeed, is often in inverse proportion to it, but is caused by the contraction of the pulmonic 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