REMARKS ON THE CONDUCTING PATHS BETWEEN THE CORTEX OF THE BRAIN AND THE LOWER CENTRES IN RE- LATION TO PHYSIOLOGY AND PATHOLOGY.

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ONE of the primary points to be considered in tracing the connection between the cortical so-called centres and those lower down, is the course and attachments of the fibres entering into the structure of the corpus callosum.

It is, I think, almost universally believed at the present day that this body is a commissure; that anatomically it unites equivalent areas in the two cerebral hemispheres, and that physiologically it serves to bring these into functional harmony. Some years ago, when working at the pathology of the brain, I came upon certain appearances which tended to shake my belief in the commissural theory, and which led to an inquiry, part of the results of which are embodied in this paper. The appearances to which I refer are to be seen in the brain of any animal possessing a corpus callosum, when it has been hardened in Müller's fluid, but best in those in which this body is of large size, as in man. It was in man that I first noticed the appearance, but I found afterwards that it is common to all callosal mammals. The Müller's fluid in the case of the human brain, or in any brain of large bulk, must be injected from the large vessels at the base, in order to cause it to penetrate properly.

If such a brain, when completely hardened, be simply cut into a series of perpendicular transverse segments, each of about half an inch in thickness, the following can be readily seen with the naked eye or a simple lens:

Coming out of the corpus callosum, at each side, is a large arched mass of fibres, which, leaving the corpus callosum and continuous with it, turns upwards, outwards, and downwards, in the centrum ovale. The arched mass varies somewhat in shape at different parts of the brain. Thus, anteriorly it represents an almost complete semicircle, while posteriorly it becomes more pointed. The fibres entering into the composition of the arched mass subsequently pass into the inner and outer capsules. The greater bulk of them enter the inner capsule, and in its anterior limb this mass of white matter is almost entirely composed of them, while a considerable portion also seem to run into the outer capsule, constituting the inner of the two layers of which it consists. Their further course and attachments to underlying parts will be considered afterwards.

This arched mass is very large, quite corresponding in bulk to half of that of the corpus callosum, and, in order to get at once at the gist of the arguments I intend using to explain its nature, I shall start with the postulate that it is mainly constituted of callosal fibres which have arisen in the opposite cortex, have crossed in the corpus callosum, and, instead of turning upwards to become attached to points in the opposite cortex equivalent to those from which they have sprung, are now turning downwards into the two capsules, to become subsequently united with the nasal and other ganglia presently to be enumerated.

In a former paper (*Journal of Anatomy and Physiology*) I have named this mass of fibres the crossed callosal tract; and, as all my work since then has tended fully to bear out the view I then entertained as to its nature, I propose still to retain this nomenclature.

The great difficulties heretofore experienced in investigating the nerve-centres have been, first, the want of a method of preparation by which their gross anatomy could be thoroughly sifted and exposed; and, secondly, of a method by which bundles of nerve-fibres could be distinctly traced or their relationship to nerve-cells indicated. In the endeavour to collect anything like reliable data from the records of lesions of the human brain, it becomes abundantly evident that until some much more efficient method of exactly localising lesions be adopted than those generally employed at the present day, little can possibly be added to the knowledge we already possess. Our concep-

tion of the functions of some of the most important parts of the central nervous system is as yet so obscure, and, in many cases, so contradictory, that it seems perfectly clear we must be erring in our notions of their physiological and structural significance.

Before continuing the attempt to demonstrate the functions of these obscure regions by the purely empirical means that have been employed of late years, it would, to say the least of it, be desirable to know something about their connections. Would it be considered a scientific method to cut down, for instance, upon the tissues of the axilla, to irritate or partially excise these, and to find that certain phenomena ensued in the upper extremity without, in the first place, endeavouring to differentiate the nerve-fibres of this region, and to display their connections with the parts below? In our present state of knowledge of the construction of the central nervous system we are in a like position. The greater part of the human brain is in fact a *terra incognita*, and I feel convinced that the prevailing conceptions of its mechanism are utterly erroneous. We have attempted far too wide generalisations in times gone by, and the idea of founding a system of psychology upon data such as we are possessed of, certainly to anyone who knows anything about the matter, savours of the absurd.

The methods of preparation I now employ for demonstrating the gross connections of the organ are, with certain modifications, essentially the same as the "gelatine-potash" process I have elsewhere described. My great difficulty in employing Weigert's hæmatoxyline copper stain for medullated fibres has been that of combining it with the method of cutting large preparations in the freezing microtome. I have now completely succeeded in doing so, and have reduced this most serviceable process to great simplicity. Space will not allow of me entering into these technicalities at present, but I intend before long to publish a full account of them.

Could one combine with Weigert's stain for nerve-fibres the aniline-black stain of Sankey and Lewis for nerve-cells, nothing better could be desired as a means of demonstrating the relationship of the conducting nerve-fibres to them. There is this difficulty, however, in the way of complete success, namely, that the aniline-black dye will give its proper reaction only when the brain is perfectly fresh, whereas the hæmatoxyline will act on the nerve-medulla only when it has been hardened in a chrome salt. I have, however, already succeeded in partially combining the two, and see no insuperable difficulty to fully accomplishing it.

If what I have postulated regarding the crossed callosal tract be true, namely, that it represents the fibres derived from the opposite cortex, which have passed over in the corpus callosum, and which are now turning down to the two capsules, the following data ought to admit of verification.

1. The crossed callosal tract should be capable of being dissected out;
2. It ought to be co-extensive with the corpus callosum; and,
3. It should be possible to trace the fibres microscopically as they turn downwards.

1. Foville long ago showed that an arched ridge of fibres could be exposed by simple dissection turning downwards at each side of the corpus callosum, and he has figured appearances in his atlas which, allowing for a certain amount of artistic embellishment, substantially represent what actually exists. I lately exhibited to the Royal Society of Edinburgh a brain previously hardened in Müller's fluid, in which this dissection had been made, and in which the arcuate mass of fibres was distinctly displayed. At that meeting I further showed this arcuate mass in horizontal gelatine-potash preparations of the centrum ovale, as clearly mapped out as if it had been drawn with a pencil. Its fibres have a more or less transverse direction, totally different from that of the fibres descending through the medullary white substance from the surrounding cortex.

2. That the crossed callosal tract is co-extensive with the corpus callosum can be proved by dissecting it out, or by examination of it in a horizontal gelatine-potash preparation.

3. It is only to be expected, of course, that the fibres should be capable of being traced microscopically, curving downwards from the corpus callosum. This is not so easily accomplished as might be supposed, owing in great part to the fibres running in different planes throughout their course from their points of origin, down to those of their termination.

It has been asserted by Meynert that he was able to trace a single fibre from the cortex of one side through the corpus callosum into the cortex of the opposite side. Anyone who has given even a minor amount of attention to the subject will have discovered that this must be a gross misstatement. The individual fibres diverge and run so obliquely after crossing, that it is utterly out of the question, even

in small animals, to think of tracing an axis-cylinder continuously from side to side in a single section.

If the brain be cut perpendicularly in an oblique antero-posterior direction, however, a very considerable part of the course of the bundles of nerve-fibres can be followed, and the most essential part, namely, from about the middle line down to the two capsules, can be traced with precision. Various methods may be employed for staining the fibres as they turn downwards, but that which I have found by far the best is Weigert's hæmatoxyline-stain modified as previously indicated. By this means, it can be demonstrated that the fibres coming out of the corpus callosum all, I believe, turn downwards to the outer and inner capsules, instead of up to the cortex, as generally asserted. In no case have I been able to see a single bundle of fibres run upwards after emerging, but the whole mass of fibres seems, after having crossed, to turn downwards into the two capsules, and to form the greater part of their bulk.

In a late number of *Brain*, Dr. Beevor has taken exception to this view as originally enunciated by me in a communication to the Royal Society, and in papers which I subsequently published in the *Journal of Anatomy*, in *Brain*, and elsewhere. He says that, in the marmoset, he has been unable to see the fibres turning down in the manner I have described, and gives a drawing which he considers demonstrates that my view is wrong, and that the old idea of the fibres passing from cortex to cortex is correct.¹ He asserts that this drawing is not a diagrammatic scheme, but an actual representation of a preparation in his possession. He further states that he has made oblique sections as I had directed, but still has been unable to see what I had described.

When I read this criticism, I felt certain of two things: first, that Dr. Beevor had not examined preparations cut in the oblique direction I have recommended; and, secondly, that the drawing above referred to was not an actual representation of the preparation from which it was said to have been taken. If nerve-fibres of the corpus callosum, when magnified, as he says they were, have the thickness of the nerve-fibres depicted in this illustration, they certainly differ from any callosal nerve-fibres which it has been my good fortune as yet to meet with. I was fully convinced that what he had endeavoured to depict consisted in reality of the fibres passing into the corpus callosum, and that he had entirely failed to see, as had happened to so many before him, those which were issuing from it, owing to his having cut the brain transversely, instead of obliquely. Those entering the body can be readily distinguished from those leaving it by the fact that, while the former are spread out in a regular fan-shaped manner, each fibre distinct from its neighbour, the latter are all aggregated into bundles, from the fact of their having been condensed while passing through the corpus callosum. In justice to Dr. Beevor's statements, however, I resolved to adopt a course which he well might have followed before being so perfectly sure that I was in error, namely, to see his preparations for myself, and to hear his explanation of them by word of mouth. I am constantly being reminded by so-called critics that they are still sceptical of my statements, and the most ardent are those who have never taken the trouble to examine my work, nor really to work at the subject for themselves. The matter is not one which can be settled in an off-hand manner, but requires the most careful scrutiny. If it had been easy to demonstrate what I have recorded, it would long ago have been done. I have never yet failed, however, to convince an unbiassed and competent person that every statement I have made in regard to the corpus callosum is substantiated and fully borne out by the preparations which I possess, when that person has done me the honour of examining the facts for himself.

My anticipations in regard to the basis on which Dr. Beevor's criticism was founded were more than realised. I emphatically state that the drawing of the corpus callosum given in his critique in *Brain* is very far from being an actual representation of the preparation from which it was taken. The continuity of the fibres is not such as that he depicts, for immediately at the outer margin of the corpus callosum there is a break in the preparation caused by a large number of fibres having been cut off abruptly, which is not represented in the drawing. The fibres so cut across constitute those I have described as turning downwards. They have been severed, because they do not lie in the same plane as those entering the body. I further found that the oblique preparations he had been examining had been cut in an entirely wrong direction, in a direction which was calculated to divide the crossed callosal fibres, instead of rendering them more apparent. As I have elsewhere stated, he has not followed the directions I have so explicitly given in various of my published papers, and until he does

¹ Ferrier, I find, has somewhat hastily reiterated this statement in the latest edition of his work on *The Functions of the Brain*.

so it is useless to argue the point. If he will harden the human brain, or that, say, of a sheep, by the method I have recommended, and cut this perpendicularly in an oblique antero-posterior direction, he will see what I have described. If he fails to do this, he will still continue in unbelief. I have over and over again exhibited preparations before competent judges, stained by my modification of Weigert's process with the invariable result that they have confessed to the downward course of the crossed callosal fibres being indisputable. Individual axis-cylinders it is impossible to trace throughout their entire course, as I have already indicated, but the continuity of individual bundles between the corpus callosum and the capsules can be demonstrated in all the mammals I have examined. The great drawback to tracing the course of the crossed callosal fibres, I believe, rests on the fact that, after crossing, those which lie anteriorly run obliquely backwards, while those which lie posteriorly run obliquely forwards, the point to which they all tend to converge being the knee of the inner capsule.

It consequently happens that, cut the brain in whatever plane you choose, the fibres will be divided at some point. In a completely transverse perpendicular section the crossed callosal fibres are usually divided, and, being represented only by small fragments, are very apt to be overlooked in the dense mass of nerve-medulla lying in their neighbourhood.

Such being the course of the fibres as they issue from the corpus callosum, let us next consider where those which enter it are derived from.

Of late years a good deal has been written of the most interesting plexus of fibres which exists in the cortex of the cerebellum and cerebrum, by Exner, Weigert, Butzke, Gerlach, Rindfleisch, Boll, and others. It seems likely that since the discovery of these fine cortical plexuses our whole notions, as suggested by Hill, of what are known as nerve-centres, and of the communication which exists between nerve cells and fibres, will shortly be revolutionised. The plexuses to which I refer can only be seen when certain methods of staining are employed. Exner, who is generally regarded as having discovered the plexus in the cerebral cortex, employed perosmic acid and ammonia, but since then (1881) the reagent used for the purpose of demonstrating it has almost exclusively been Weigert's hæmatoxyline stain previously referred to. This brings out an appearance in the cortex, both of cerebrum and cerebellum, which is truly marvellous. In the cerebellum it is particularly dense, but I have also found it in all parts of the cerebral cortex I have examined.

What I would specially wish to direct attention to, however, is that this plexus not only prevails in the cortical grey matter, but appears to intertwine itself round the nerve-fibres throughout a great part of the white. The large medullated fibres from the cortex run into the white matter, but almost immediately become surrounded by a dense padding or casing of this fine plexus of nerve-fibres. At first it might be supposed to be simply connective tissue, and it has in times gone by been always regarded, when indistinctly seen by other methods of demonstration, simply as the branching neuroglia. The plexus I refer to as pervading the white matter of the brain is, however, nothing more or less than a true nerve-apparatus of extreme complexity, and that which is found in the cortex of the cerebrum and cerebellum is simply an extension or outcrop of this. It also exists in certain of the ganglia, such as the thalamus and lenticular nucleus, but there is one part of the brain in which it is probably more highly developed than in any other, namely, in a little comma-shaped body which lies in the angle formed by the caudate nucleus and the corpus callosum. This body, whose presence I have never seen even referred to, is one mass of a dense and complicated nerve-plexus, without nerve-cells, so far as I am able to discover. It is contiguous to the caudate nucleus below, but the tissue of the one is separated from that of the other by a sharp line of demarcation. It passes for a short way underneath the corpus callosum; and the fibres constituting the plexus of which it is composed are directly continuous with the fibres of the plexus in the white matter of the brain, to which I have already referred. It is only lately that I have made out the true nature of this mass, and as yet I am at a loss to give it a name. Let us call it for present purposes the plexiform nucleus.

The large masses of nerve-fibres coming in from the cortex are therefore immersed, as it were, in a dense plexus of nerve-fibrils, and those which are callosal take the following course in entering that body.

The most of those which come down from the vertex appear to run directly into the corpus callosum, and interlace in doing so with those leaving it, which are turning down to the two capsules. Those which are derived from about the lower third to a half of the cortex extending between the Sylvian fissure and the great longi-

tudinal fissure do not appear to run directly into the corpus callosum, but pass first of all into the plexiform nucleus just described. Shortly after issuing from the grey matter, they become united into strands which penetrate through the fibres of the crossed callosal tract, and which finally pass into the plexiform nucleus just described. They seem to lose themselves within this by breaking up into its reticular network. From the callosal end of this plexiform nucleus fresh fibres appear to arise, and to enter the corpus callosum. In all probability, these turn downwards on the opposite side into the two capsules as fibres of the crossed callosal tract. This plexiform nucleus would thus represent a meeting-point for many of the callosal fibres before they proceed to cross. The individual fibres lose their identity within it by splitting into an anastomosing common network, and from this network again fresh fibres arise, which travel across the corpus callosum to the opposite side.

The fibres which enter the body are chiefly derived from the region of the motor centres which in man have been found to preside over the muscles of the tongue and face, that is to say, the lower parts of the ascending frontal and parietal convolutions, and it is quite possible that the function of the plexus contained in it is to correlate and associate the action of the various fibres coming from these parts, so that they may exert a combined influence.

After passing into the inner and outer capsules, the arched callosal fibres just described become united into dense bundles; and the next point to consider is their subsequent distribution. A very large proportion of them certainly lose themselves in the thalamus opticus. The excessively fibrous appearance which the thalamus presents is due to these fibres passing into it. They probably break up into a network, in the meshes of which are intercalated the nerve-cells. Are these nerve-cells directly connected with the nerve-fibres entering the ganglion, or is the plexus referred to intermediate? The more I examine the cortex and basal ganglia, the more enamoured do I become with the idea that the union is not direct, but that a plexus intervenes in most cases between the two. I am not certain about the large motor cells of the cortex, but in the case of the other cells of this part of the brain, and in that of the cells in the basal ganglia, it seems much more probable that the actual termination of the nerve-fibres is in a plexus, and that this plexus simply surrounds the nerve-cells. I am not at all convinced, even, that the nerve-cells in all cases are directly connected with the plexus by their processes. How do we know that nerve-energy generated in cells may not exert its influence upon nerve-fibres in ways other than by direct continuity? Is it not conceivable that it may be transferred to the coils of a dense plexus through the liquid and neuroglia which fill up the intervals in the tissue, and that conversely peripheral stimuli may thus be conveyed to a nerve-cell? I think this is quite possible, and the idea has of late been entertained by several physiologists, both in this country and abroad.

Few, if any, callosal fibres end in the caudate nucleus, and, curiously, as if supporting this observation, the plexus in the caudate nucleus seems to be very scanty. The lenticular nucleus may receive through the striæ medullares a considerable number, and probably some of the fibres connected with the red nucleus may be also callosal. A large number appear to end in the pons and medulla oblongata, while there is a probability of certain of them even penetrating down to the spinal cord.

This paper, however, is concerned with the connections between the cerebral cortex and the centres lower down, and as yet I have referred to only one set, namely, those which are callosal and which cross from the opposite side. There are others, of course, which run down directly, and of these the motor fibres are among the most important. These direct motor fibres lie to the outside of the crossed callosal tract, and, like it, bend somewhat outwards in circumventing the ventricle. Those derived from the marginal gyrus seem, at least in the sheep, to lie in very close apposition with the fibres of the crossed callosal tract. In man, I calculate that about one-third of the fibres entering the anterior two-thirds of the posterior limb of the inner capsule are direct while the remainder are crossed callosal.

From experiments made upon the cortex, it is evident that these are derived from a wide area, one, indeed, so wide that it comes to be a question how it is that the fibres are so few when they decussate in the medulla, and become connected directly, or through the intermediation of the spinal cord, with the peripheral nerves. The notion at present held by most physiologists is that, from the motor cells of the cortex, fibres issue which are continuously prolonged downwards to the spinal cord. But if we consider the matter for a moment, it must be evident that they must have suffered much in bulk by the time they reach the medulla, and that the pyramidal fibres of the medulla or cord cannot represent the whole of the motor fibres derived from

the motor area. How, then, is this sudden falling off to be accounted for? My present conviction is that the direct continuity of the process of a ganglion motor cell in the cortex and the pyramidal tracts of the spinal cord is a myth. Try to follow them from the cortex inwards, in the best stained preparation to be had, and you will find that, shortly after they enter the centrum ovale, you lose their direct continuity, and they appear to merge into the plexus, which I have just described as pervading the whole medullary white. My firm belief is that, just as in the case of many of the callosal fibres, the motor fibres break up into a plexus, from which again fresh fibres, those which enter the pyramidal tracts, take their origin. When the pyramidal tracts in the cord are affected by secondary degeneration, the implicated tracts are mapped out with the utmost precision, and the degeneration never overlaps them. Can the same be said of the degeneration further up in the centrum ovale? I do not think that it can. There is always more or less diffusion of the tract below the point of lesion, if that be cortical, and the explanation, I think, is to be found in the interposition of this plexus. The plexus is a means of reduction and association, a means by which the action of the many fibres coming from a particular cortical area may be combined and correlated in the few.

But the direct fibres entering the inner capsule are not all motor in their function. There are many other bands which enter it and whose function varies. Thus there is a large contingent of fibres which passes into its posterior limb from the parieto-occipital region, and whose function there cannot be much doubt is sensory. It has been shown, over and over again, that when it is destroyed hemianæsthesia results.

One of the main differences which exist between the brain of man and that of the lower mammalian types consists in the disproportional size of the white and grey matters. In man the white matter is relatively more abundant than in the brain of any other mammal I have examined, and the lower we go in the scale the greater the disproportion appears to be.

Now, the cause of this, I think, seems mainly to reside in the fact that this intertwining plexus which ramifies through the whole centrum ovale is vastly more abundant in man than in the lower animals, and hence, probably, the superiority of the human brain, as an instrument of association, may be accounted for.

Then there is a large band of fibres which comes from the prefrontal region, and which enters the anterior limb on its way back to the anterior nucleus of the thalamus, to which it becomes attached. The geniculate bodies and the pulvinar finally are connected by direct bands with the occipital region.

I have said that a large number of callosal fibres pass into the thalamus opticus. They lose themselves in it, apparently by becoming connected with a dense plexus. In conclusion, let me ask the question whether there are any fibres which leave the thalamus, and, if so, where do they go to? Do fibres descend from the thalamus into the cerebral peduncle, to enter ultimately into the spinal cord? I am becoming daily more and more convinced that, if such do exist, they must be small in number. The thalamus no doubt has certain bands of fibres entering it which are not callosal, but these seem to connect it with parts of the cortex associated with which experiment has shown are definite and well located functions. Thus there are the three so-called peduncles of the thalamus, uniting it respectively with the prefrontal region, with the nucleus amygdalis in the temporo-sphenoidal lobe, and with the hippocampus major. But these differ entirely in their nature from the fibres which are supposed to leave the thalamus and to pass downwards with the other descending cerebral tracts, and it seems to me that the latter, if they do exist, must be in small quantity.

How then is the thalamus connected peripherally, and what is its use? As yet any statement on this subject must necessarily be largely of the nature of a mere conjecture, but my researches incline me to believe that it is almost exclusively connected with the education of the brain through the optic nerves. Of all the nerves in the body the optics are those by which the brain is mainly educated. They are in constant use, imperceptibly opening up the cortical grey matter to impressions made upon the periphery by light vibrations. How is this brought about? What is the connecting link between the peripheral retina and the central cortex? Vision is said to be located in the occipital lobe, but I would ask if the optic does not equally subserve a most important function in educating other parts of the cortex as well as this small area? How is it that the motor centres, for instance, are educated to a particular complicated act, purely through the sense of sight?

What is the mechanism by which a sudden visual impression, accompanied by a sense of danger, will serve instantaneously to throw

the body into a complicated attitude of defence? This introduces far too wide a subject of discussion to take up at present; but it seems to me extremely likely that the thalamus is an educating optic ganglion, in the sense that the callosal fibres entering it have their functions concentrated within its substance, and that these fibres are the means of educating the opposite side of the brain through vision.

Where the corpus callosum has been destroyed in infancy, the invariable result seems to have been imbecility. There are certain congenital deficiencies of the body which have been recorded unaccompanied by any symptoms of note, more especially one described by Eichler. In these latter cases, however, it seems, from an examination of their records, that we have to do not with a deficiency in the actual callosal fibres, but with a malformation by which they have failed to decussate in the middle line, just as so frequently happens in the anterior pyramids.

On the supposition that the thalamus subserves the purpose of concentrating the fibres which educate the higher centres through the optic, I can quite easily see how, if it were destroyed in adult life, no very evident symptoms might follow. It has already subserved its purpose to a great extent. The higher centres have been educated, and are capable of discharging their functions apart from the channels through which that education has been imparted.

Nay, I could go so far as to grant that a large part even of the corpus callosum might be annihilated in adult life without occasioning any very marked phenomena. It has already played its part, so to speak, in infancy and youth, and may in a manner be now considered as functionally inert. The impressions made upon the cerebral cortex through it are quite possibly recalled by a perfectly different set of channels; for I do not see why in the brain-economy there may not be paths for educating the higher centres through vision, hearing, touch, and so on, and a whole set of other paths by which the results of this education may be brought into action. If such be the case—and I advance this simply as a surmise—I should regard the callosal system of fibres as the great educating system; while the other direct bundles to which I have adverted would constitute the means of adapting this education to a utilitarian purpose.

In the case of those born blind, this education, of course, could not go on through the optic circle of communications, but would take place by means of the nuclei of the nerves of special sense situated in the pons and medulla and the crossed callosal fibres connected with these.

AN ADDRESS

ON

SOME OF THE RARER SYMPTOMS PRODUCED BY GALL-STONES.

Read in the Section of Medicine at the Annual Meeting of the British Medical Association in Brighton.

By WILLIAM M. ORD, M.D. LOND., F.R.C.P.,
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THE occurrence of gall-stones is so common, and the symptoms of biliary colic are, in a general way, so decisive, that, in dealing with the effects produced by gall-stones, I shall ask to be allowed to pass over many points which, in a complete treatise, would be, necessarily, subjects of primary consideration. Therefore, I do not intend to discourse either on the structure of gall-stones, or on the way in which they come to be formed; nor to describe biliary colic as commonly seen; nor to enumerate all the possible wanderings of gall-stones, either to exit by one path or another from the body, or to intrusion into various organs; nor to discuss treatment, operative or not operative, seeing that this relation is to be discussed in another Section.

I will ask you to follow me in two lines of thought. Accuracy of diagnosis occupies, in all cases, the primary attention of the practical physician. In this aspect, it appears to me, on the one hand, that cases in which gall-stones have existed without producing the symptoms commonly indicative of their presence may be found worthy of consideration; that, on the other hand, cases illustrating uncommon effects due to the presence of gall-stones may be found to have a value, a very different value, of their own. Around this thread of diagnostic investigation winds the thread of

inductive or explanatory investigation, every turn whereof may send us to contemplate some fresh and important relation to the whole organism of the small area affected by disease. In this aspect we are rather philosophic observers than practical physicians. If we are to do all that is required of us, we must be both; and we may be sure that it will ever come about that in this duality each comrade will minister to the other. I will first call your attention to gall-stones which give rise to no symptoms.

Gall-stones Existing without Symptoms.—It is a common experience of the *post-mortem* room that gall-stones, small and large, solitary or in hundreds, shall be found in the bodies of persons whose medical history contains no record of suffering or disease such as our present knowledge would refer to gall-stone; and that, structurally, no evidence of serious local mischief determined by the presence of concretions shall be apparent. Are there, one is tempted to ask, people who pass renal or vesical calculi, without other signs than the migrations of the calculi into the chamber-pot?

Gall-stones Passed without Symptoms.—The occurrence of what is generally spoken of as biliary colic, with vomiting, with sequence of jaundice, and of the faecal and urinary signs of retention of the bile, constitute, I think you will allow, the most obvious signs of the existence of gall-stone. But gall-stone of large size may exist, and may find its way into the bowel, without producing any symptoms whatsoever of biliary colic. Here is a specimen which I showed some time ago at the Pathological Society of London. It was sent to me by my friend, Mr. Carr H. Roberts, of Kensal Town. I always call it the "Twin," because it was passed by a lady on the day after her confinement, with such difficulty as almost to constitute a second labour. This lady had, so far as I can learn, no precedent signs of gall-stone whatever, and it must be supposed that adhesion had taken place between the gall-bladder and an adjoining coil of intestine, and that a painless ulceration had offered an easy retreat to the concretion. But we all know that the obstruction met with in this case at the anus may occur in the intestine. Most of us have, no doubt, seen cases of the kind. I have seen several. One, which I saw with Mr. Bullock, of Isleworth, I should like to quote. A lady, aged between 60 or 70, was attacked suddenly with symptoms of intestinal obstruction. A careful investigation led to the belief that the obstruction was mechanical, and within the abdomen. Mr. Thomas Smith, of St. Bartholomew's, was accordingly asked to give his help. He first opened the abdomen at a point where most pain was felt. He then minutely examined the state of the intestines, bringing out through the small orifice the nearest coil of intestine, and working from this in both directions, a fresh coil being brought out after its predecessor had been examined and returned. A piece of jejunum, about 5 inches long, was at length extruded, in a state of intense inflammation, black and rough on the surface. Nothing abnormal could be felt within it, and we supposed that it had undergone some strangulation, which had been relieved by the operation. The patient died the next day, and a very limited *post-mortem* examination was allowed. A large gall-stone was, however, found, embraced strongly by slightly inflamed intestine, a little below the seat of inflammation already observed. The gall-stone had, of course, been moved on, without being felt, in the manipulation. Now, in this case, reviewed after the knowledge of the actual existence of the conditions stated, it may be observed that no ordinary symptoms of gall-stone had presented themselves. It follows that in considering the possible causes of any case of intestinal obstruction, we are not justified in excluding impaction of gall-stone, even though all story of previous biliary colic or of jaundice be wanting.

Gall-stones giving Rise to Pain, etc., without Jaundice.—The case just recorded leads on to the consideration of a fact of great importance, namely, that gall-stones may give rise to various kinds of pain and suffering to inflammation, and even to death, without the occurrence at any time of jaundice. Such cases are far from rare, and are to me of great interest. Let me quote two or three illustrative histories.

Some years ago, I was asked to see the wife of a clergyman in the East of London. I was informed that she was subject to irregularly recurring attacks of fearful pain in the region of the gall-bladder, with associated vomiting and faintness. She had never had jaundice, nor passed pale stools. Many physicians had seen her, whose diagnoses had ranged between uterine irritation at one pole, and cancer at the other. The absence of jaundice, and of the other signs of obstruction of the common bile duct, left the clinching of the diagnosis impossible, and the idea of duodenal ulcer was chiefly entertained. The patient died in an attack a few months later, and was found to have had a large biliary calculus, which had made its way through a perforation of the gall-bladder into the peritoneum.