THE LETTSOMIAN LECTURES ON SYPHILIS AND THE NERVOUS SYSTEM.

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LECTURE III.—PART II.

A GENERAL survey of the treatment of syphilis, as well as the observation of individual cases, can scarcely fail to impress one important conclusion on an impartial observer. It is not easy to state this conclusion in language not open to misconception. Stated in its baldest form, the conclusion is that syphilis is an incurable disease. Stated in the form least liable to misinterpretation, and with the limits of exactness, it is this: there is no real evidence that the disease ever is or ever has been cured, the word disease being used here to designate that which causes the various manifestations of the malady. The statement that "syphilis is an incurable disease" is the shortest way of stating this fact, and it is legitimate if we recognise that "by incurable" merely we mean that there is no proof of cure. I do not put this opinion forward as in any way novel; indeed, I feel with regret that on this and on many other points in these lectures, I express opinions that are held by others, some of which have been well stated by others, and all of which, I cannot doubt, have been stated more than once. I regret that it has not been possible for me to ascertain the extent to which this and other opinions have been before expressed: hence, I feel that the only course that is just to others and safe for myself is to ask you to regard nothing in these lectures as having any claim to originality. As I said at the outset, my desire is to render more definite the knowledge that already exists, and not to bring new knowledge into existence. In current works on syphilis the fact of incurability is clearly recognised, although even in these it is not quite compatible with the language here and there employed. But the fact is certainly not accepted by the profession at large in the manner which seems to me to be warranted by the facts. It is, I think, most important for practical reasons that it should be understood by all, that, as far as evidence at present goes—whatever power we may hope and anticipate that the future will bring—no treatment, however thorough, will bring the disease to an end, so that the patient does not suffer again from any of its direct effects. In this sense, and it is the only proper sense of the words, the assertion seems to be open to no question, and yet the fact is very remarkable. It is strange that this should be true of a disease, the effects of which are so largely under control. We can, under favourable circumstances, remove most true specific lesions, we bring to an end most true specific processes, but the arrest or removal of the manifestations of a disease is a very different thing from its cure. We can, indeed, do a little more than remove the manifestations and effects of the disease, we can restrain the activity of the virus, whatever it be, that causes these manifestation and effect. But when we speak of the cure of a disease, we mean that its essential element, that which lies behind all its symptoms and consequences, that which is the persistent cause beneath the transient effects, is made to cease, is ended once and for all as morbid agent, so that it never again disturbs the system. In this sense, we cannot bring the disease to an end. Facts on all sides show this if we will perceive their meaning. I believe that it is literally correct to say that we have no evidence that syphilis ever is, or ever has been, cured.

I can scarcely doubt that so absolute a statement will seem, even to some here to-night, unjustified and perhaps mistaken. Some of you may call to mind cases that seem to contradict it—cases in which a patient was suffering from symptoms of the disease, and after treatment was free from any indication of the malady. Such cases do occur; they may be numerous. Far more frequent, however, in the experience of most observers, are cases in which the immunity that follows a course of treatment is not permanent, in which other symptoms reappear at a later period. In very few of the cases regarded as "cured" has the patient been kept under observation long enough to justify the positive conclusion that the disease was at an end. Nowhere has this fact

been recognised more clearly than in the admirable work of Hill and Cooper. Cases are common enough in which the hope of cure produced by several years of perfect freedom was ultimately disappointed. Of the patients with late syphilitic lesions that come under the observation of the physician, many, very many, have undergone thorough treatment for the early symptoms. It is difficult—I may say that to me it is impossible—to compare the history of the various sufferers from syphilitic lesions—to compare the course of the disease in cases in which treatment was early and thorough with those in which no treatment was employed—and not to question whether early treatment has yet been shown to have any appreciable influence in preventing the late symptoms of the disease. But, if this be so, how are we to explain the cases in which the malady has seemed to cease after a course of treatment—those in which it has ceased so far as observation has gone? I have already hinted at the explanation which I believe to be the true one. We saw in the first lecture that, if we compare a series of cases, we meet with many in which the primary affection received no treatment at all, and yet the later symptoms were of a trifling character. There is the strongest reason to believe that many other cases occur in which later symptoms are altogether absent. Some facts that have an instructive bearing on this subject come to light in the histories of patients taken in the medical wards of a hospital or in the out-patient room, who are suffering from diseases that have no relation to syphilis. It is not uncommon to meet with those who have unquestionably suffered from primary syphilis and from slight secondary symptoms, but who took little or no notice of them, and who have never been treated, and who have had no later manifestations. I have mentioned the series supplied to me by Dr. Radcliffe Crocker, and which I hope he will extend and publish, for their importance is very great. In this series there are many cases in which notable late syphilitic skin eruptions occurred without any recognised previous manifestations of the disease, and even many in which the primary lesion had been unnoticed by the patient and was certainly never treated. As I have said, there must be many more with a similar latency of the early stages, with a similar absence of treatment, who passed not merely six or ten years without anything to call the malady to notice, but who never noticed through life any manifestation of the disease to tell them that they were or had been its subject. Take, again, the other class of cases I referred to—the immunity of mothers of syphilitic children. They are certainly syphilitic, and yet many of them have never been treated and never suffer. I believe that the variations in the general course of the disease in those who have been treated find parallels in the course of cases that have not been treated. I suspect that, if a large number of each class were compared, the discrepancy between the two series would be found to be surprisingly small, and that it would not be difficult to find in each series a corresponding proportion of cases, on the one hand, in which the disease without treatment was brief and trifling, and, on the other hand, in which the disease with treatment was prolonged and severe. All the facts, impartially examined, seem to me to have one meaning, clear and unmistakable to those who choose to read it. The extreme variations in the natural character and course of the disease have not been sufficiently considered and allowed for by those who are under the impression that the cure of syphilis is certainly within our power. This impression is an illusion produced by the variations in its course. In some patients the disease is brief; the symptoms that bring the patient under treatment may be the expiring effort of the virus, and the spontaneous cessation of the disease is ascribed to the treatment which was adopted. In other cases the tendency to further manifestations at some future time, near or distant, may be so strong that, in spite of the most energetic treatment, long continued, many times repeated, wave after wave of activity brings to the unhappy subject year after year of varied suffering, and sometimes his life ends sooner than the disease.

I have put the matter strongly in order to emphasise the essential truth. But certain qualifications are desirable. They do not touch the fact itself. One qualification is that, while we have no proof that we ever can or ever have cured syphilis, it is possible that we do come very near the achievement, and even sometimes attain it. I said that some manifestations are probably—indeed, I might say certainly—the expiring efforts of the malady. It may be that in some others the symptoms that bring the patient under treatment are only the penultimate manifestations, and that some further consequences which would occur were it not for the treatment may be averted. Then we do arrest

the disease altogether. In such a case the malady may be said to be cured; but the cure is at best limited, and cannot be regarded as more than hypothetical. The second reservation is one rather of words and theory than of fact. There are some who hold that the duration of the disease is much more brief than that of its manifestations—that these latter symptoms are indirect consequences, either in their real nature or in the mechanism by which they are caused. The statement I have made is one that is independent alike of pathology that is known or any theory that may be held. Whatever may be the intimate and ultimate pathology of the later symptoms, from the point of view of practical medicine they are part of the disease, and no question as to their origin or their relation to the virus can alter the fact that the disease, as a disease to be dealt with and not merely to be thought about, is not, in the proper sense of the word, a disease that can be cured. Speculations as to the origin of the lesions and the essential nature of the malady at different periods have no influence, it seems to me, on the practical problems to which we must now confine our attention, although it may be that these practical questions have an important bearing on those speculations. Although the course of the disease in its relation to treatment justifies the doubt whether it is ever cured, except in this narrow and partial sense, the question arises whether the removal of the lesions and the repression of the activity of the essential elements of the disease has an influence on the course of the affection. It is difficult here to resist the seductive attractions of theory and of analogy. But the question can be answered only by facts, and we ought not to try to obtain the semblance of an answer from any other source.

The conclusion that the essential element in the disease resists treatment, and runs its course uninfluenced by our efforts, is in harmony with what we know of other specific diseases due to a poison introduced from without, and communicable from one person to another. There is not any fact whatever to show that a single disease of this kind can be cut short. The course of the acute exanthemata cannot be arrested by any means at our disposal at any stage of their course, and the same seems true of this chronic exanthematous disease. This is eminently true, also, of the disease that stands perhaps nearer to syphilis than any other known malady—leprosy.

Here we catch another glimpse of the strange duality of the disease. And yet the word "strange" is scarcely accurate. In syphilis we probably only see that which exists in all maladies of like nature, but in most others, to a large extent, escapes our notice. What is it that treatment influences in so remarkable a manner? What is it that remains in spite of the treatment—remains to assume from time to time a fresh activity, and to cause renewed manifestations? We say, in general terms, that treatment removes the effects of the disease, the local lesions it produces. Besides this, treatment seems to be capable of repressing, and even suppressing, the activity of the *materies morbi*. Take this one fact, evidence of which you will find presented with care in the work of Hill and Cooper. A syphilitic woman who would bear a child certainly diseased and probably dead, can by treatment be made to bear a child that, at the time of birth, shows no trace of the disease; yet even such an influence on the morbid process in the mother does not prevent her from again suffering. If we are able to hold securely the outline of a general truth only by means of the point of an hypothesis, we may consider that our drugs destroy the developed and developing organisms, and fail to influence the germs of these. But much remains obscure when this or any other hypothesis that we can frame has done its utmost to clear our view, and we must wait, perhaps for long, before we can understand the real reason why we are able to do so much, and yet can do no more.

The general treatment of syphilitic lesions does not come within the limits that I have set to these lectures, and, even if time permitted me to make an exception on account of the importance of the subject, I should still hesitate to occupy your attention with that which every student is taught. The few minutes that remain I may best spend in pointing out certain points on which we need to have a far firmer foundation of observed facts than has at present been supplied, and a more secure hold on certain truths.

One question on which there is some difference of opinion is regarding the respective power of mercury and of iodide of potassium, represented in its extreme forms by the custom of some physicians to give only iodide for the late lesions, and the opinion of others that mercury alone is effective, and that iodide has merely the power of bringing into an efficient activity the mercury that

may have been deposited and held inert in the various tissues. The only point really at issue, however, is the influence of iodide. I have, on this subject, to range myself with the majority. It seems to me impossible to see much of the true specific lesions of the nervous system, and not to believe that over most of them iodide has a most potent influence. The symptoms of a gumma, for instance, lessen and pass away under the influence of iodide of potassium as speedily and as completely as would seem, *a priori*, to be possible from any agent. I am inclined to think that when a lesion is distinctly influenced by either drug, if this is given freely, the effect of one is as great as that of the other; and that, in most circumstances, I doubt whether it is necessary to follow the iodide with a course of mercury. But this is only an impression; the physician cannot gain from his cases any real trustworthy conviction. We need more detailed observations by those by whom the effects of the treatment of lesions in the skin and other parts can be seen. We need observations as to the relative effect of different kinds of treatment, and the time needed for the complete removal of specific processes. We need details that would be useless for the observer himself, but are essential for us. And we need in such observations careful discrimination, as far as is possible, between the effect of the drug, and changes which result from that effect, but may need time for their full completion. It is very easy for physicians to mistake the slow improvement in symptoms that goes on for a considerable time after the syphilitic lesion has been removed for the slow removal of this lesion.

At the same time the question needs asking, and needs an answer—are there late syphilitic lesions over which iodide of potassium has no influence, and which yield to mercury? Some years ago this question was impressed upon me by the case of a woman who had a firm mass deeply seated in the posterior triangle of the neck, compressing the lower nerve-roots entering the brachial plexus, and causing paralysis of the muscles of the forearm and hand. She presented unmistakable signs of constitutional syphilis, including the loss of almost the whole of the soft palate, which had been destroyed by ulceration several years before. Iodide of potassium was given in large doses for about six weeks, but without the slightest effect on the mass. Perhaps, during the following two months, it did not get larger, but it certainly did not get less, nor did the pain which was caused by the compression of the nerve-roots. Mercury was then given, and as soon as its influence was established the mass began to lessen, and the pain ceased, and in a few weeks the neck had resumed a normal state. The lesion was no doubt the chronic syphilitic peradenitis not uncommon in this region. The significance of the case seems clear save for one source of fallacy I will mention in a moment.

I have not met with any case of intracranial disease in which there was reason to believe that mercury was successful while iodide failed. In the case of gummata, I have found as a rule the effect of iodide perfectly satisfactory, as great as seemed possible, with entire removal of the symptoms, except in cases in which such complete removal could not be expected. Syphilitic inflammation is, I think, better treated with mercury, since this has far more influence than iodide over the process of inflammation, irrespective of nature and cause; but I cannot say that this opinion is based on observations free from sources of fallacy.

With regard to the methods of administration of mercury, I have nothing to say. The old method of inunction seems to me to bring the point under the influence of the drug as speedily as it can be done with safety, and with a certainty incomparably greater than the administration by the mouth. I have been deterred from a trial by the hypodermic method because the published evidence seemed to me not to afford any satisfactory proof of superiority, being destitute of the element of comparison essential to such proof, and because this method seems to afford an opportunity for psychical influence not free from risk of that which is undesirable. But I would not for one moment suggest that such an influence has entered into the motives or action of those who have used this method.

There are two points in the treatment of syphilitic diseases of the nervous system on which I think a word of caution is urgently needed. The first is regarding the prolonged administration of antisiphilitic drugs, especially of iodide of potassium. By prolonged I mean exceeding from six to ten weeks. I believe that full doses in this time will effect all that it can achieve in the removal of the syphilitic process. Here, as I have just said,

we want facts that are visible—numerous and carefully observed—to guide us in our conclusion. I do not say that the symptoms will have disappeared. It cannot be too firmly remembered that symptoms are due to changes that are not syphilitic—changes in the nerve-elements secondary to the syphilitic disease, but so far independent in course that they may persist long after the specific lesion is at an end. Hence, the fact that the symptoms have not yet ceased is no indication that the specific lesion is not entirely removed. Here, then, I would range myself with the minority—a minority that is fast growing in size, especially in Germany—who hold that the long-continued treatment by small doses of mercury or iodide is a mistake great and dangerous; who hold that treatment of any true specific lesion should be energetic, but should continue only a little longer than is necessary to remove the lesion, being repeated, it may be, after an interval occupied by tonic treatment or by the other of the two chief drugs. If iodide is continued, as it often is, during many months (and much more, as it sometimes is, during years), there is a danger that the system and tissues of the patient may become so accustomed to its presence that the drug may no longer hold in check the syphilitic processes. Probably we may still influence the lesions by increasing the dose; but this process must have limits, in practice if not in theory. Such prolonged and augmented treatment may do definite injury to the patient's health, and even then fail to effect the desired object. If the poison is an organised virus, we might *a priori* expect this result. By long-continued gradual alteration in the conditions, low organisms—as Dallingier has shown in his remarkable experiments—can be made to endure influences that would at first be fatal to them. By slowly raising the temperature, they will not only live but flourish at a degree of heat which, had it not been for their acclimatisation, would have killed them in a few minutes. But the question is not one in which we can be guided by theory, far less by an analogy possibly remote. There is, moreover, a positive danger in this method. It is also great in the practice to which I referred at the beginning of the lecture—that of repeated courses of energetic treatment to remove residual symptoms that cannot be thus removed, because they do not depend on any residual specific process. I will mention this in a moment.

The test of experience alone can say whether this risk of loss of power is imaginary or real. But, at the same time, the fact does not need for its proof the evidence of accumulated experience or numerous observations. A single clear instance will suffice to establish it, and no number of negative instances would disprove it. We know how erratic the course of syphilis is, and how great must be the variations in the state and tendency of the virus in the system. In one case, if the energy of the disease is subdued, it is for a long time tranquil, while in another the tendency to fresh development soon reasserts itself. Hence, it may readily be that a series of cases may lead to a delusive confidence in the safety of a course of treatment which is, nevertheless, now and then fraught with danger. The following instance, I think, proves that the danger is not imaginary. Many years ago a man came under my treatment with symptoms of local chronic meningitis about the pons. Ten grains of iodide of potassium were given, and the symptoms rapidly lessened. Slight residual symptoms, however, remained, for which iodide was continued, and he took it continuously for about four months. At the end of that time some obscure cerebral symptoms developed, the syphilitic nature of which was doubted because they had developed in spite of the iodide. He was admitted to the hospital, and the iodide was continued and slightly increased, but the symptoms rapidly developed, spinal symptoms were superadded, and in a few weeks he died. *Post mortem* we found the remains of local syphilitic meningitis at the base, as had been suspected, and also a syphilitic gumma in the cerebral hemisphere, and another in the spinal cord. Here, then, we had clearly a new development of that syphilitic lesion over which iodide has most influence, although the patient was at the time, and had been for a long time before, continuously taking the drug.

I have seen other instances, both with iodide and with mercury, but this case impressed me so as to preclude the occurrence of fresh illustrations in my own practice, and I do not care to adduce instances not observed by myself throughout. Especially in early syphilis, however, I have been satisfied that the virus may resume activity during the continuation of mild mercurial treatment. Here is a source of a possible fallacy in the case I mentioned

just now, in which iodide had no influence on the syphilitic process in the neck. It is possible that the patient had been taking iodide for a long time before she came under my care. There is no evidence of it, but the point was not investigated and must remain doubtful.

I would add only one other remark. If it is true that we cannot cure syphilis, it is most important to consider how it can best be kept in check. This is why the fact of incurability, if true, is so important. A mistaken belief in curability may dangerously hinder attempts at prevention. If no present treatment can prevent future developments, then it is wise, whether these come or not, to anticipate them. I think a custom, sometimes recommended, is prudent, that every syphilitic subject, for at least five years after the date of his last symptoms, should have three weeks' course of treatment twice every year, taking, for that time, twenty or thirty grains of iodide a day. If this practice were adopted generally, is it not reasonable to anticipate grave lesions would be much more rare?

The idea that residual symptoms are necessarily proof of residual specific lesions which can be removed by a renewal of treatment that has already been energetic seems to me an opinion not only destitute of proof but fraught with grave danger. I have alluded already to the ease with which a mistaken conclusion can be reached. The danger depends on the tendency, which I have more than once discussed, and must again advert to in its therapeutical aspect, the tendency to degenerative changes in the nervous system presented so frequently by those who have had syphilis. The danger is, I think, as great in those who have had true specific lesions as in those who have not. These degenerations, as we have seen, are not checked by specific treatment, and there is specific reason to think that they are sometimes accelerated by such treatment, especially when it exerts a depressing influence on the general health. I have seen many instances of this, especially from energetic mercurial treatment. In most cases of the kind the evil has been done by the repetition of mercurial treatment in the hope of removing residual symptoms which were not, and could not, be removed by such treatment.

There is one other important element in the treatment of lesions in the nervous system that is often forgotten. We have to do with a syphilitic process, and with the disease of the nerve-tissues which the process causes. Thus there are three elements in the affection, the specific process, the damage to the nerve-tissues caused by that process, and the symptoms that are due to this. It is important to recognise each of these elements, and to consider in arranging our treatment how far the two pathological elements need special measures. In all the treatment of syphilitic lesions we must never forget, in treatment, any more than in diagnosis and prognosis, the simple changes in the nerve-tissues on which alone the symptoms depend. To confine our therapeutical attention to the syphilitic element is to neglect measures that may make an appreciable difference in the ultimate result. The cases, it is true, are rare in which we are able to appreciate the effect of such neglect on the one hand, or of recognition on the other. In the improvement that follows the removal of the syphilitic process, we are commonly liable to discern that which may result from collateral measures. A chief bane of modern therapeutics is, it seems to me, the demand for proof where proof cannot be forthcoming, and is the tendency to reject that of which the evidence is not clear, when the absence of such evidence is no ground for inferring the absence of benefit. In every case all measures should be employed that are suggested by what we can learn of the nature of the morbid process, and of the known tendency of therapeutic agents, confident that they cannot be altogether without influence in bringing about the ultimate result, and satisfied if now and then we can distinctly trace their beneficial action.

Here I must end. To me, and, if I have been in any measure successful in achieving the result that has been my aim, to you also, the subject cannot but grow in importance the more it is studied; its vast proportions seem to loom still vaster through the mists that obscure its features, and prevent clear vision of its outline—still to us unlimited. To those who have attempted to explore the range of its influence on the nervous system, or to those who look on and note the efforts of others—who see from time to time some fresh discovery extend our knowledge in directions altogether unsuspected, and see region after region of disease opened out before the mental eye as part of this affection—the malady must seem like some "dark continent" not yet half traversed, in which the known may still be less than the unknown. Moreover, the

nervous system is only one out of many parts of the human frame in which this terrible disease plays havoc, and it may be that the greater frequency and extent of its processes in this region is merely apparent—a consequence of the readiness with which the nerve-elements are deranged in their structure and function, and of the distinctness with which such derangement is revealed. Strange indeed it is to think that all this train of morbid processes, so long, so varied, often so disastrous, is the consequence, we cannot doubt, of the entrance into the system of a few organisms, scarcely to be seen even with the utmost increase in the power of vision, human ingenuity has yet contrived. And it is strange and sad to remember that this malady, rivalled in its total capacity for wrecking happiness and health and life by no other, and exceeded in apparent horror only by those whose effects are more sudden, is equally formidable by reason of our limited power over it. Make what deductions you can for the mild or latent forms of the disease, recognise all our power of repression, and the fact remains that we have yet to find the means of arresting it; and I may add we have yet to find effective means for its prevention. The philanthropy whose vision is so narrowed by misplaced feeling, that only one imperfect aspect of the means employed can be perceived, has decreed that the chief method that has been suggested shall not have so much as a trial under fair conditions, despite the manifold and varied benefit that has been obtained from its partial use—and has decreed that the disease shall continue unrestrained, to work its ruin alike on the innocent and the guilty. Against the influence of this mistaken zeal for a nominal purity and a real disease we are powerless. But there remains the fact, and it is certainly as old as the malady itself, that the disease can be prevented. One method, and one alone, is possible, and that one is open to all. It is the prevention and the safety that can be secured by unbroken chastity. Is this potentiality becoming greater? As we look back through the long centuries, we see the sensual more and more dominant in the past, growing less as the race slowly rises. But as we look at the present we can trace small ground for hope that this process will have any appreciable influence unless or until there is some change in men more potent and effective than the slow “live upward, working out the beast” of moral evolution. But that which will not perhaps be for the mass, may yet be for the individual. And in ending I must ask a question and give a warning that I would fain have left unasked, unsaid. But I cannot, I dare not, pass them by. Do we do all we can—and our profession gives us power that no other has—do we do all we can to promote that perfect chastity which alone can save from this, and from that which is worse? The opinions that on pseudo-physiological grounds suggest or permit incontinence are absolutely false. Trace them to their ultimate basis, and they are groundless. They rest only on sensory illusions—one of the many illustrations of a maxim I have often to enforce on various sufferers: “There are no liars like our own sensations.” Or rather, I should say, they rest on misinterpretations, always biased, and often deliberate. With all the force that any knowledge I possess can give, and with any authority I may have—as the result of long observation and consideration of the subject in all its relations—I assert that no man ever yet was in the slightest degree or way the worse for perfect continence or the better for incontinence; that for unchastity every man must be worse morally; and that most are worse physically, and that in no small number the result is, and ever will be, utter physical shipwreck on one of the many rocks, sharp, jagged-edged, or one of the many banks of festering slime that are about his course, and which no care can possibly avoid. My warning is: Let us beware lest we give even a silent sanction to that against which I am sure, on even the lowest grounds that we can take, we should resolutely set our face and raise our voice.

ELECTRICAL STIMULATION OF THE HEART IN MAN.

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It is, of course, only in a very limited number of the cases of cardiac failure that the question of artificial excitation of the heart beat becomes one of practical importance. In the majority of instances where a more or less sudden heart stoppage occurs there are underlying conditions which obviously render direct

stimulation of the organ inapplicable or hopeless. But, on the other hand, in certain forms of cardiac arrest there appears to be a possibility of restoring by artificial means the rhythmic beat, and tiding over a sudden and temporary danger. Such is especially the case in those instances where cardiac failure assumes the form of an inhibition of the heart beat by impulses reaching the organ along the vagus nerves. There is much reason to believe that among cases of sudden and fatal syncope such a mode of heart failure is not very rare. It probably obtains in many of those instances where sudden death has been caused by blows upon the præcordia or stomach (apart from the occurrence of any important visceral lesion); also in many cases of death from fright, and, as Lauder Brunton has urged, from irritation of sensory nerves (for example, the fifth) in certain phases of the administration of chloroform. It is in regard to sudden cardiac failure from the last-mentioned cause that the question of direct excitation of the heart becomes most important. There is much probability in the view that sudden stoppage of the heart under chloroform may often be due to a sudden excitation of the cardio-inhibitory centre in the medulla oblongata and a consequent arrest of the cardiac beat by inhibitory impulses carried down from the medulla by the vagus nerves; such appears to be especially probable when the heart suddenly stops, early in an operation, when a comparatively small amount of chloroform has been given and the anæsthesia may be more or less incomplete.

Now we know that when the mammalian heart has been inhibited through the vagus nerve it is quite possible to excite an immediate renewal of the rhythmic action by direct stimulation of the organ. No doubt it is very possible, as I have already suggested in a former paper, that the fate of the heart may be sealed in cases of fatal inhibitory arrest by the supervision of fibrillar contraction or heart-delirium in the ventricles. But, apart from the occurrence of such a disastrous complication, or before its inception, artificial excitation might be useful in rousing into action a heart that has been arrested by a temporary cause; for example, by inhibitory impulses profoundly depressing the rate and force of its action, or causing it to stand still in diastole.

As regards the means to be employed for the purpose of directly stimulating the cardiac action in cases of sudden failure, various expedients have been recommended—among others the application of galvanic and faradic currents to the region of the heart, electro-puncture, mechanical irritation by a fine needle passed into the organ, and the application of heat to the præcordia.

The application of strong galvanic and faradic currents to the ventricles is attended with disastrous results; an immediate abolition of the normal beat, and the occurrence of a wildly incoordinated, arrhythmic contraction of the ventricular muscle (fibrillar contraction or heart-delirium), attended by a great and rapid fall of blood-pressure, and, in the higher mammals (the dog at least) by speedy death. The ventricles are utterly unable to pump out the contained blood, and the circulation quickly comes to an end.

If such a phenomenon had been observed simply in one or two sorts of animals, it might be urged that the human heart is possibly exempt from such a danger. But the phenomenon in question is one that occurs in more or less extreme form in every mammal examined, and, indeed, in birds as well. I have myself observed it in the hearts of the dog, cat, rabbit, hedgehog, rat, mouse, guinea-pig, hen, pigeon, and blackbird. It is, in all probability, of universal occurrence in the hearts of warm-blooded animals; and it is significant that it is more easily excited and more extreme in its character in the higher mammals than in the lower.

It has been argued that the strength of current employed clinically would not be sufficient to bring about the grave result in question. In regard to this consideration there is a point of much importance, namely, that the susceptibility of the ventricular tissue to the occurrence of fibrillar contraction may become enormously augmented in certain conditions of impaired and disordered nutrition. Of this fact I have been most fully convinced in the course of my investigations on the mammalian heart, for on numerous occasions I have seen the ventricles going off into fibrillar contraction under the influence of extremely slight irritant causes, slight pressure or friction, faradic currents too weak to be felt on the tongue, etc.—causes which would have been entirely insufficient to bring about such a result in a normal, or approximately normal, heart. Now it is precisely in conditions