health so much impaired, that at one time I was on the very point of proposing excision, you will see that the child is now stout and well, that the limb is a very useful one, and the knee is only partially stiff. The relations have brought these children here to-day for inspection; for it is now many months since some of them were discharged from the Hospital, and, consequently, the lapse of time enables you to see that the cure is a permanent one. It is, in fact, I may mention by way of parenthesis, one advantage that we enjoy here, that we are usually able to keep sight of the case long after it leaves our wards, that we are able to watch the changes which subsequently occur, and, in cases of operations especially, to judge of their ultimate results; a great advantage, and one but too frequently overlooked or found impossible in hospital patients generally.

Whilst writing this lecture, I was glad to find my views on this subject corroborated and confirmed by a most eminent surgeon in Dublin, and one whose opinion might have been expected to incline differently, for he has been the great and successful advocate of excision; I mean Mr. Butcher. In his recently published *Third Report on Operative Surgery*, Mr. Butcher states, that it is "impressed strongly on his mind, nay, that it is his sincere conviction, that these severe operative measures will very seldom indeed be either warranted or called for" in children. That "repair in the young is to be looked forward to, to be depended upon as a certainty", and consequently that there should be "no hasty removal of diseased joints" in childhood.

Among the poorer classes, it is true, we have many disadvantages to contend with. Time, good air, and good living, are three great requisites in aid of our surgical treatment, but the demand for our beds is so urgent, that the first can seldom be afforded within the Hospital; and, as an out-patient, the child will seldom enjoy the two last, whilst it is deprived also, too often, of systematic care and of judicious nursing. Such impediments to success, as we cannot pretend to ignore them, must be fairly encountered and grappled with; difficulties exist in all cases, and in every sphere of action; it is the part of him who hopes to succeed in life, or who strives to conscientiously perform his duties, to be prepared for difficulties, to exert himself to his utmost to overcome them.

In bringing to a conclusion the first course of lectures ever delivered at this Institution, I may briefly state that they were undertaken because my colleagues, as well as myself, were most anxious to render generally available any information which this Hospital may afford; because we felt that our position here involved a trust, and that our offices had been bestowed upon us, not for our own individual advantage, but with the hope that we might contribute something to the knowledge of the diseases of children, and of their treatment.

INFLUENCE OF SEX IN THE DISEASES OF CHILDREN. According to Dr. R. Küttner of Dresden, long before the complete development of the sexual organs, even in the earliest infancy, diseases depend much on sexual difference. From a comparative examination of ten thousand cases of infantile diseases, he says that:—1. Boys, particularly in the first year, suffer more from inappropriate food and diseases of the digestive organs than girls of the same age, and a greater number of boys will perish in consequence of such diseases; the relative mortality being about the same in either sex. 2. Diseases of the brain and the nervous system in general, are twice as frequent in males as in females, particularly in and after the fifth year of life. 3. Umbilical and inguinal herniæ are more frequent in males than in females. 4. Diseases of the respiratory organs are more frequent in females after the third, and even more so after the fifth year. A larger number of such diseases prove fatal in females. 5. The same observation is made in organic diseases of the heart. 6. Acute diseases of the blood exhibit no perceptible difference as to sex, but chronic anæmia and scurvy-like decomposition of the blood is more frequent in females than in males, especially after the eighth year. At this age, the proportion of thus diseased females to males is ten to one. Scrofula and tubercles show no particular difference in the first years; but, after the fifth year of life, tubercles in the lungs are found more frequently in females. The number of cases of rachitis is pretty equal in the two sexes, but it is often developed at a later period, and is apt to last longer in females than in males. 7. Chronic diseases of the skin, particularly eruptions of the scalp, are more frequent in females than in males, after the ninth year. 8. The same is found to be the case in tumours of the thymus gland. (*Jour. für Kinderkrankheiten, and New York Journ. of Med.*)

Vacation Lectures

ON

FIBRINOUS DEPOSITION IN THE HEART:

ITS HISTORY AND PRACTICAL MEANING.

DELIVERED AT THE GROSVENOR PLACE SCHOOL OF MEDICINE.

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LECTURE I.

INTRODUCTORY REMARKS; *Plan of the Course*; *Different conditions of Blood in the Heart after death.* FIBRINOUS CONCRETIONS IN THE HEART: *their Forms; Structure; Weight; Chemistry; Process of Deposit; Post Mortem and Ante Mortem Deposits.*

[Concluded from p. 47.]

IV.—DEPOSITION BEFORE AND AFTER DEATH.

WHETHER the separation of fibrine from the rest of the blood in the heart, as it is found in many instances after death, is an act *post mortem* or *ante mortem*, is a question, which for more than a century and a half has caused more disputation than almost any other in pathological science.

Harvey seems to have been acquainted with "polypi" (for so the old men characterised fibrinous concretions) in the heart; and Malpighi knew that the white clots found in the heart after death in inflammatory diseases, were the same as that crust which forms on the surface of inflammatory blood. Kerckringius, a writer of no mean repute, opposed the view, that the "polypi" were formed before death. His hypothesis was ably met in 1684, by a countryman of our own, Dr. William Gould, who, in the *Philosophical Transactions* for that year, described fibrinous deposit with remarkable accuracy. He not only denoted the symptoms which attend the progress of deposition, but pointed out the diseases in which deposition is most imminent; and even the further fact, that there may be deposited a form of concretion from which portions of fibrine may be borne away, and, being carried into the extreme parts of the circulation, may plug up the finer vessels, and stop the nutrition of the part beyond. Morgagni was well conversant with concretions of the heart. He also knew their source. The principal and peculiar substance of which they were made, he taught to be the same as that which forms the crust of blood in inflammation. These concretions, he adds, are sometimes begun and even completed in the living body. He thought they might commence in cases of protracted syncope. We are now pretty well assured, all the world over, that there is such a thing as deposition of fibrine from the blood in the living body. It is also admitted by those even who see most clearly the full importance of this admission, that there is such a thing as separation of fibrine from the blood after death in certain cases. Let us consider both these positions with candid care.

Firstly, then, we may learn whether a concretion has been formed before or after death by examining the concretion, and by observing its relations to the surrounding blood.

There is one true and unmistakable form of *post mortem* concretion; that, namely, which I have marked first in my list of illustrations. I mean that separation in which the fibrine lies in a thin layer on an under stratum of red coagulated blood. Everything here points to the fact that the fibrine left the blood while the source from which it came was in a state of rest. No other explanation of this form of concretion is possible.

But, in reference to every other description of concretion which I have given, the evidence is equally clear in support of the view that they, whenever found, are of *ante mortem* date. I do not at present discuss how long before death any one of them may have been produced—that will constitute a subject of inquiry further on; but I say for the moment, that they are produced before death, and that they can only be explained as resulting from blood in motion. Where, for instance, there is a separated clot filling the whole of a cavity, such as the right auricle, it is certain that the clot must have been formed antecedently to death; for the simple reason, that there is no blood left in the cavity from which it could have been pro-

duced after the final result. When the clot is grooved, we have again demonstrative proof that the concretion was of *ante mortem* date; because the groove, whether curved or spiral, is in the line of a blood-current, and is, in fact, made by the transit of blood, as the bed of a river is made by its current. If the concretion is a hollow cylinder, containing red blood in the tube, the evidence in favour of the origin of the concretion from blood in motion is equally conclusive; for, as the tubular concretion is never met with except when the deposit takes place in a cylinder, as the pulmonary artery or the aorta; as the tubular concretion is never grooved spirally; as the centre of the tube is filled with blood; as these tubular concretions (in every case, at all events, in which I have met with them) show evidence of having been broken away from a basic attachment; and lastly, as they entirely fill the vessel in which they are found,—there can be but one reading of their origin, which is, that they were laid down from their circumference layer by layer; that they formed, in fact, a false and increasing tubular lining to the inner part of the vessel; and that the red blood found in the centre is the last remaining portion left there at the moment of dissolution.

It is but fair, nevertheless, for me to state that a hypothesis has been projected, to the effect that the tubular concretion may be produced after death. Dr. Humphry of Cambridge, in a most able essay on the *Coagulation of the Blood in the Venous System during Life*, assumes that a tendency to separate coagulation of fibrine will produce its effects after death in much the same manner, and in the same position as during life; and that it will thus lead to the formation of fibrinous clots upon and near the valves, and in the interior of the vessels. In this manner, Dr. Humphry further assumes, a tubular lining of fibrine may be laid down in the pulmonary artery after death, and be moulded upon the sigmoid valves, while the remainder of the blood in the vessels is fluid. This remaining blood, subsequently coagulating, would form a soft dark cylindrical clot, inclosed in and continuous with the fibrinous tube.

I am sorry that I am obliged to dissent entirely from this conclusion of Dr. Humphry. I dissent because, from my observations of the coagulation of fibrine in the static condition, it never separates from blood in the manner suggested; it always separates in such way as to crown the blood from which it was produced. True, blood left in the pulmonary artery, blood very rich in fibrine, may yield after death a thin coat of fibrine; but such coat will express but half a cylinder, and will lie on the upper surface, like the buffy coat of inflammatory blood.

Further, the true *ante mortem* fibrinous tube, as I have described it, is not a mere film of fibrine inclosing a large cylinder of red coagulum. Such a separation I have myself never seen. The hollow cylinders I describe are made up almost entirely of fibrine in layers; the red blood, not more than the size of a quill in diameter, and quite incapable of yielding such a surrounding mass, running along the centre. The same observations apply to those forms of separation in which the fibrine lines the whole of a cavity, as the auricle, and incloses a loose red coagulum.

The firm adherence of a concretion to the walls of the heart is another proof of an origin before death. To produce such adhesions as we have seen already, even though the adhesion be only mechanical, motion is required. The pulsating heart must *churn* out the fibrine; and it is in this act that the firm interlacement takes place. Owing to this fact, we find that, in the separation of fibrine which takes place after death, there is no adhesion of the fibrinous layer to the cardiac surface; yet it is clear that, if separation were the result of mere contact, there would be in these, as in other cases, a firm adherence. If the connexion betwixt a concretion and the heart be organic, however slight the connexion may be, the evidence is of necessity all the stronger, not only of the existence of the concretion prior to, but of an existence *long* prior to, death. This argument is so entirely obvious, that it is unnecessary to push it further.

I have one further word to say on the distinction of fibrinous concretions formed before and after death; this relates to the structure of the deposit itself. If a concretion be laid down in one layer, it may have been formed before death, though it need not necessarily have been so formed. But if a concretion be laid down in distinct layers, such concretion bears its own evidence of having been constructed in layers, step by step, from the blood. Or, if a concretion found in a newly dead body (in a body twenty-four hours dead) be changed in its centre into a soft purulent looking mass, then the proof is carried with the concretion, that the concretion was formed in

the living body, and at a period of at least twelve or sixteen hours before death; for it takes forty hours to produce the softened change in fibrine at a temperature of 98°. I am not sure, indeed, that the change would progress at all at the temperature of the dead body; but, this point waived, certain it is that it would not occur within forty hours after death.

Lastly, some indication of a concretion being formed before death is to be gleaned from the mode in which it is moulded to the parts surrounding it. I agree with Dr. Humphry, that a *post mortem* concretion may bear the impress of that part of the cardiac wall against which it is left in contact; for I have found, by some recent inquiries directed to this point, that if gelatine dissolved in warm water be run into the vena cava, so as to fill the right side of the heart and the pulmonary artery, and if the solution be left to gelatinise, and the heart be then opened—the concretion, for so I may call it, of gelatine will be found bearing a slight impression of all the parts of the heart against which it has lain. At the same time, when the impression made on the concretion is very distinct, indicating that contraction has been made upon the deposit, and when with this modeling there is adhesion, as was the case in the concretion delineated in Fig. 2, then the presence of the impression is corroborative evidence of a separation during life.

To sum up. The proof of a *post mortem* concretion is its position—on the upper surface of a red coagulum. The proofs of an *ante mortem* clot are—(a) The fact of its filling a cavity; (b) the facts of its being grooved externally by a blood-current, or bored by a current through its centre; (c) its being firmly adherent to the heart either by mechanical or organic tie; (d) its structure being laminated, or containing in its centre broken up fibrine; (e) the fact of its being deeply indented by surrounding structures.

Are there any other proofs wanting in support of the view that deposition of fibrine takes place during life? To some minds, such proofs may be required; and so I proceed to them.

In continuance, then, I remark that the *weight* of a concretion is in many cases proof direct of the *ante mortem* nature of the deposit. The position stands as follows. The amount of fibrine in healthy blood is—I take an extreme point, in order to make the argument fairest—3 parts per 1000. The right side of the heart of an adult man, including auricle and ventricle, is capable, I find, in the relaxed state after death, of holding from 1400 to 1500 grains of blood. To make it contain this quantity, however, it is necessary to tie the inferior vena cava, to fix a funnel tube into the superior vena cava, and a straight tube into the pulmonary artery. The organ must then be held with the base upwards, and must be filled through the funnel tube until fluid appears in the tube in the pulmonary artery. The right side of the organ will thus be charged with fluid, and that to an extent rarely if ever met with as a *post mortem* result. The amount of fluid required for this state of fullness will be, as I say, 1400 to 1500 grains; let us accept 1500 grains as the amount. Granting that the heart were left at death thus charged with blood containing the natural amount of fibrine, the fibrine, however it might be coagulated—as a red coagulum, as a white concretion, or as a mixture of the two—could not exceed in weight 4.50 grains.

The greatest increase in the amount of fibrine in disease is up to 12 parts per 1000. I do not myself understand how blood so charged with fibrine could circulate; but, as the statement has once gone forth that such a blood was found, let us take that as the extreme example.

But I have found in the right auricle of the heart of a child, dying of pneumonia, thirteen grains of fibrine, which would represent 4333 grains of blood in health. Admitting, therefore, that the right auricle of this child was capable of holding not less than the auricle of an adult heart, and half as much as the whole of the right side—auricle and ventricle together, I mean—and that after death the auricle was left distended with 750 grains of blood, then it would follow, on the hypothesis that the concretion was formed after death, that the blood left in the heart contained 13 grains of fibrine in 750, or at the rate of 17.3 parts per 1000, which is absurd; for a blood so loaded with fibrine could not circulate at all.

In the above sentences, I have made over to the *post mortem* hypothesis of concretion more by double than any one of its own exponents could claim. For, in the case I have quoted for illustration, the auricle in reality had a dimension only capable of holding, at full distention, 240 grains of blood, and the auricle and ventricle together but 700 grains. Further, there was no red blood either in the auricle or in the ventricle, and, in fact, nothing save fibrine combined with water; but I have thought it best to extend to an absurd limit the *post mortem* hypothesis,

in order the more effectually to show its absurdity. I could labour out the question of weight from at least a dozen more cases from similar data, but the result would be simply a repetition. Any one desiring to follow out the same mode of proof, has only to remove a concretion from the heart of a patient dying at the acme of an inflammatory disease with the symptoms of concretion, as I shall give them in the end, and to proceed as follows:—to weigh the concretion in its moist state; to dry it gently on the water bath till it loses weight no longer; to weigh again, and take the last figure as the amount of fibrine. The dimensions of the heart being then calculated, and the amount of blood in the cavity of the heart in which the concretion was found also calculated, the excess of fibrine over the proportion of 3 per 1000 of blood may be readily ascertained.

It remains for me to adduce one other proof of the fact that fibrinous concretions may be formed during life. This proof, reserved to the last, may be considered the most direct, because it appeals to the visual sense. It is this: that an animal can be so placed that fibrinous deposit in its heart shall be induced—induced by experiment, by what I have elsewhere called the production of disease by synthesis; that the presence of such concretion being diagnosed by symptom, the animal may be made insensible to pain by a volatile narcotic; and that then, the respiration being sustained artificially, and the chest laid open, and the pulsating heart laid bare, the heart may be opened, and the concretion found there present, and removed. I have succeeded in various ways in thus producing concretion: I have succeeded by arresting a large tract of the circulation, as by passing a ligature beneath the trachea, carrying its ends back behind the vertebral column, and by the tourniquet screw stopping all the cerebral circulation. I once narcotised a dog with sulphuretted hydrogen; and, when the animal was entirely insensible, I allowed him to remain in the open air for several hours. He was still insensible at the end of nine hours, but was evidently sinking: I then laid open the heart, and found in the pulmonary artery a distinct fibrinous concretion.* But the best way of performing this synthetical experiment is to place a carnivorous animal in a closed chamber, and to fill that chamber steadily and constantly with pure and freshly made oxygen. A carnivorous animal so placed will live, I find, for a period of from twelve to thirty-six hours. When the animal begins to fail, or, in other words, to sink, there is manifested in all his movements the signs of obstruction of the heart—signs which will hereafter be more carefully indicated. The sinking fully set (for, before the sinking, there is no concretion, the prostration being the mere result of the obstruction), narcotic vapour is introduced into the chamber, and, insensibility complete, the vivisection is made. I have extracted large concretions from six animals under this condition. The colour of the clot has varied in these cases from red to perfect white. In one instance, a cat being the subject of experiment, the concretion was of pure white, and filled and distended the right auricle, and passed into the ventricle, and passed into the pulmonary artery. In this instance, the sinking symptoms were allowed to go on until death was imminent. Nevertheless, the heart was actively contracting when it was exposed, and continued to contract for nearly twenty minutes after the concretion was removed from its cavities.

I have thus offered, I trust, not a theory, but a complete demonstration, of the fact that fibrinous deposition is an event possible previous to death. I shall have to trace out, in the next lecture, the symptoms indicative of concretion, whereupon the opportunity will recur of adding further proof to the proofs already supplied.

POSTSCRIPT. I append in full the account of the "serpent in the heart" case, quoted at the beginning of this lecture. Dr. May, the author of the case, was a man in good practice in this town in his day. We must not laugh at him; for there may be physician-physiologists, two hundred years hence, whom we would have forgiven our ignorance as we forgive.

"The seventh of October this year current, 1637, the Lady *Herris* wife unto Sir *Francis Herris* Knight, came unto me and desired that I would bring a Surgeon with me, to dissect the body of her Nephew *Iohn Pennant*, the night before deceased, to satisfie his friends concerning the causes of his long sickness and of his death: And that his mother, to whom my selfe had given helpe some yeares before concerning the Stone, might be ascertained whether her Sonne died of the Stone or

no? Upon which intreaty I sent for Master *Jacob Heydon* Surgeon, dwelling against the *Castle Taverne* behind *St. Clements Church* in the *Strand*, who with his Man-Servant came unto me: And in a word we went to the house and Chamber where the dead man lay: We dissected the naturall Region and found the bladder of the young man full of purulent and ulcerous matter: The upper parts of it broken, and all of it rotten: The right kidney quite consumed, the left tumified as big as any two kidneys, and full of sanious matter: All the inward and carnose parts eaten away and nothing remaining but exterior skins.

"No where did we find in his body either Stone or gravell. The Spleen and Liver not affected in any discernable degree, only part of the Liver was growne unto the Costall membranes, by reason of his writing profession.

"Wee ascending to the Vitall Region, found the Lungs reasonable good, the heart more globose and dilated, then long; the right Ventricle of an ashe colour shrivelled, and wrinkled like a leather purse without money, and not any thing at all in it: the *Pericardium*, and Nervous Membrane, which conteyneth that illustrious liquour of the Lungs, in which the heart doth bath its selfe, was quite dried also: The left Ventricle of the heart, being felt by the Surgions hand, appeared to him to be as hard as a stone, and much greater then the right: Wherefore I wished M. *Heydon* to make incision, upon which issued out a very great quantity of blood; and to speake the whole verity, all the blood that was in his body left, was gathered to the left Ventricle, and contayned in it.

"No sooner was that ventricle emptied, but M. *Heydon* still complaining of the greatnesse and hardnesse of the same, my selfe seeming to neglect his words, because the left Ventricle is thrice as thicke of flesh as the right is in sound men for conservation of Vitall Spirits; I directed him to another disquisition: but he keeping his hand still upon the heart, would not leave it, but said againe that it was of a strange greatnesse and

Alongside the body of the "serpent", in the original drawing, is the following certificate:—

"That the 7. of October, this 1637 an Embrion of this forme and dimentions, as is here described was found in the Left Ventricle of the heart of *Iohn Pennant* Gentleman, of the age of 21. yeares, or thereabouts, Wee who saw it testifie under our hands:

Edward May Doctor of Physick.

Jacob Heydon Surgeon.

Elizabeth Herris Aunt unto the said *Iohn Pennant*.

Dorothy Pennant Mother to the said *Iohn Pennant*.

Ricard Berry

+*Mrs Gentlemans* marke.

This is my wives marke I testify *George Gentleman*."

The figure has been here reduced to the scale of one linear inch in three.

* Since the above remark was delivered, I have again performed this experiment three times; but the results have been imperfect. I do not, therefore, offer the experiment for repetition.

hardnesse; whereupon I desired him to cut the Orifice wider: by which meanes we presently perceived a carnouse substance, as it seemed to us wreathed together in foldes like a worme or Serpent: at which we both much wondred, and I intreated him to seperate it from the heart, which he did, and we carryed it from the body to the window, and there layed it out, in those just dimensions which are here expressed in the figure.

"The body was white of the very colour of the whitest skin of mans body: but the skin was bright and shining, as if it had beene varnished over; the head all bloody, and so like the head of a Serpent, that the Lady *Herris* then shivered to see it, and since hath often spoken it, that she was inwardly troubled at it, because the head of it was so truly like the head of a Snake.

"The thighs and branches were of flesh colour, as were also all these fibraes, strings, nerves, or whatsoever else they were.

"After much contemplation and conjectures what strange thing that part of the heart had brought forth unto us, I resolved to try the certainty, and to make full exploration, both for mine owne experience and satisfaction, as also to give true testimony to others that should heare of it: And thereupon I searched all parts of it, to find whether it were a pituitose and bloody Collection, or the like: Or a true organical body, and Conception: I first searched the head and found it of a thicke substance, bloody and glandulous about the necke, somewhat broken (as I conceived) by a sudden or violent separation of it from the heart, which yet seemed to me to come from it easily enough.

"The body I searched likewise with a bodkin betweene the Legs or Thighs, and I found it perforate, or hollow, and a solid body, to the very length of a silver bodkin, as is here described: At which the Spectators wondered. And as not crediting me, some of them tooke the bodkin after me, made triall themselves, and remained satisfied, that there was a gut, Veine or Artery, or some such Analogicall thing that was to serve that Monster for uses naturall: Amongst whom the Lady *Herris* and the Surgian made tryall after me with their own hands, and have given their hands that this Relation is true. This Lady dwelleth at the signe of the Sugar loafe in S. Iames street in the *Convent Garden*."

Original Communications.

TREATMENT OF TETANUS BY ACONITE.

By LEONARD W. SEDGWICK, M.D., Boroughbridge.

ACONITE having been recently recommended by Mr. Page and Mr. Campbell de Morgan as worthy of fair trial in tetanus, and all old established methods of treatment having failed in affording any hope of cure in this formidable disease, I thought myself justified in subjecting a case which came under my care last May to the full influence of the drug. Before offering any remarks, I will give a condensed history of the case from detailed notes taken at the time.

CASE. A farmer's labourer, aged 30, strong and muscular, healthy and temperate, in jumping off a cart with a dung-fork in his hand, stuck it into his left thigh about three inches above the knee-joint on the inner and anterior surface. For some days the knee was stiff, and he was unable to work. Ten days afterwards, being much better, he began chopping sticks. About noon he got wet; and, whilst at his dinner, he was seized with a sharp pain between his shoulders; at the same time he thought he could not open his mouth as well as usual. On the eleventh day, he was unable to work from acute pain in the back and jaws.

On the twelfth day from the accident and the third of the tetanus, I saw him. He was then lying on his back, perspiring intensely, with an anxious painful expression of countenance. His jaws were nearly closed; the muscles of the back were very rigid; the loins were almost always some distance off the bed; the arms were not much affected; the legs were stiff; the abdominal muscles hard. Pulse 95, not very full. His tongue was moderately clean. The bowels were regular. He had no sleep. The urine was natural. I ordered him beef-tea and six ounces of brandy in the day; and five minims of Fleming's tincture of aconite in water every four hours. I laid open the wound, and removed a considerable

piece of woollen cloth, which had been driven in from his trousers by the fork. To save repetition, I may here state that the wound healed steadily.

Fourth day. He was much the same.

Fifth day. The spasms were not so constant, but more violent. No aconitism had appeared. Seven minims of the tincture of aconite were given every four hours.

Sixth day. He was worse. When the spasms were relaxed, which was only for a very few minutes, the pulse was 68; during the spasms, it rapidly rose to 120, and became smaller. Opisthotonos was extreme; the jaws were clenched. Ten minims of the tincture were given every four hours; and he was ordered to have ten ounces of brandy daily.

Seventh day. Tingling in the hands and feet and slight giddiness having come on, the spasms had been much less severe. The pulse was weaker, and he had great sleeplessness and restlessness. The aconite was omitted; and twenty minims of chlorodyne were given in an ounce of water every four hours.

Eighth day. He continued easier, and slept well. The pulse was stronger. The spasms were not so frequent. He complained of much pain from flatulence. Twenty minims of tincture of sumbul were given with the chlorodyne. I may remark, that I have seen more benefit from sumbul in flatulence than from any other drug.

Ninth day. He took more beef-tea, etc., and was improving. The flatulence was diminished.

Tenth day. Immediately after being startled by a loud noise, he had a violent spasm, lasting some time. It recurred at intervals with great violence. He was ordered to have three minims of tincture of aconite and ten of chlorodyne in an ounce of water every four hours.

Eleventh day. He was much the same. The dose of tincture of aconite was increased to four minims.

Nothing of importance occurred until the seventeenth day. He continued the mixture, and the cramp decreased. The bowels having been confined several days, he had a turpentine enema, which greatly relieved him, and was repeated, every other day. On the seventeenth day, some tingling came on, and continued until the nineteenth, though the aconite was reduced to a minim and a half every four hours. On that day, the aconite was suspended. The next day there was more cramp. The aconite was resumed for a week longer, and he gradually recovered. In less than three months he was at work again. The muscles were some time in regaining their extensibility after the tetanic spasms had ceased.

REMARKS. The subjects for consideration are, the severity of the attack; the action of the aconite; the effect of the chlorodyne; the chances of spontaneous recovery; and the value of the case as a guide to future treatment.

It was unquestionably a most acute attack. A punctured wound, containing an irritating foreign body, prevents a man from working for ten days. He gets wet; and immediately tetanus sets in, and rapidly worsens. Every muscle of the body is racked with cramp; more than half his time he rests on his head and his heels; every step across the floor, every noise in the house, agitates him with this torturing spasm; and from this agony he is free for only two or three minutes at a time. To those who saw him, it was truly a most severe case.

If I have related honestly and faithfully what I most certainly saw, there will be little reason for any detailed argument as to the action of the aconite. When the symptoms of aconitism came on, then, and not till then, did the symptoms begin to abate. Twice, when the aconite was suspended, once to try the effect, once because the tingling and giddiness were becoming extreme, did the spasms return, to abate again on the resumption of the drug. This contratetanic action of the aconite is rendered more probable by the large doses which were originally borne, a part of the remedy seeming to be neutralised, as it were, by the disease, and only the superfluity able to produce its poisonous effect. This view will explain why smaller doses produced greater effects towards the end of the case, there being then less disease to combat.

Perhaps some justification is necessary of the administration of the chlorodyne. Perhaps it may be objected, that I marred the decision of the result by its use. I do not think so. I have often seen the good effects of chlorodyne in producing quiet without depression. Here was a man under the influence of aconite—sleepless, intensely and emphatically restless. My object was to cure him, not to conduct an experiment only. I saw reason to believe that chlorodyne would relieve the disagreeable symptoms probably produced by the aconite,