

Considerations
BEARING ON
OUR PRESENT KNOWLEDGE OF FEVER.
A First Lecture.
BY WALTER MOXON, M.D., F.R.C.P.,
PHYSICIAN TO, AND LECTURER ON THE PRINCIPLES AND PRACTICE
OF MEDICINE AT, GUY'S HOSPITAL.
(Concluded from p. 933.)

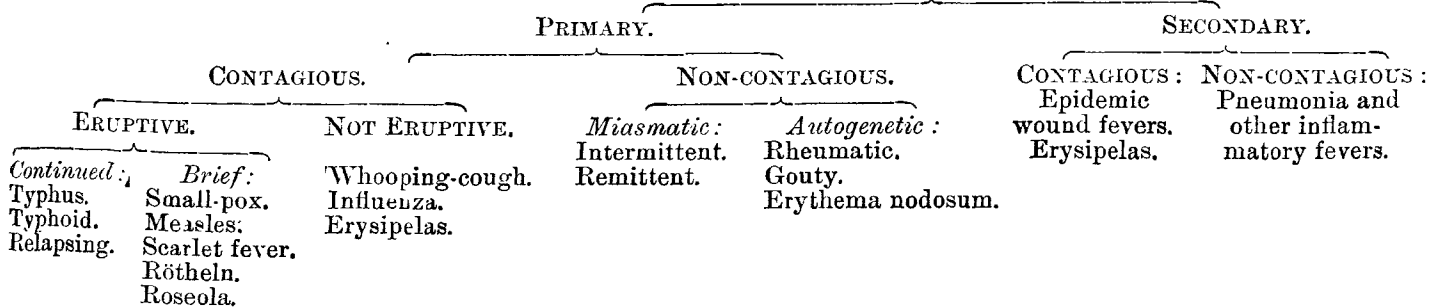
Now, with reference to fevers, I wish to draw your attention to the fact that we are able to be clear upon the idea of kind, both in the wider and the more restricted sense which I have just laid before you; and that in this relation of kind fevers differ from all other classes of diseases; at least this is true of the contagious fevers, in which the disorder is propagated in a manner only comparable to descent by generation. Indeed, one of the most practical of the fruits of wisdom which experience will develop in you when you have seen much of professional work amongst families will be the readier conclusion of the nature of an attack of illness by tracing the kind of fever to which a child has been exposed. And, again, it may be said that the chief evidence of the distinctness of kind among some of these contagious fevers has been obtained through most patient investigation and watching of local epidemic attacks of one fever or the other, by which it has been proved that each disease spreads in its own kind, and not in any other. This is the only conclusive argument to prove that typhus fever is distinct from typhoid fever; for all the differences which I shall have to detail to you as characterising typhus in its divergence from typhoid might be differences through accidents. I have seen a very crowded outburst of typhoid eruption amongst the rash in typhus, and *vice versâ*. Ulcerated intestine might be a character superadded to some cases of typhus, as pneumonia is superadded to some cases of measles, or as diphtheritic disease of the fauces is fatally superadded to some cases of scarlatina. But when you find typhoid always breed typhoid, and typhus always breed typhus, then all question as to distinctness of kind ceases, or at least lapses into the more general question, whether species of fever can be constant when, as modern biology has shown or brought up to a general belief amongst all biologists, no species of any living thing either is constant or ever has been constant. Not that this last reflection brings existing species nearer each other; for though they vary they do not vary towards each other, but always away from each other. They ever diverge; cats never varying to become dogs, nor dogs to become cats; so typhus never varies to become typhoid, nor typhoid to become typhus. This certainty of kind in fevers marks a vast difference in their nature and relations as compared with those diseases which I shall have to introduce before you when speaking of disorders of the lungs, heart, and brain. I shall then detail to you the characters of many things spoken of as kinds of diseases; but the word kind is much looser here, and indeed has only a quite vague and popular application. I remember being amused by a passage in some story. The author had got one of his characters into a hospital, and made a little poor fun in setting out the effect

said as how the thing what No. 3 has got in his head is the same as No. 6 has got in his stomach."

I suppose that the clinical teacher must have been asserting in the hearing of the author of this character that inflammation of the brain was of the same kind as inflammation of the peritoneum, and if the question was raised, Is meningitis the same thing as peritonitis? you would certainly see that meningitis differs from peritonitis, not like typhus and typhoid differ as generated kinds differ from each other, but meningitis and peritonitis would be always held to differ by differences untraceable in their nature, as the purple and white foxglove differ, so long as you considered them to be separated by the inalienable peculiarities of origin and relationship which separate the meninges from the peritoneal sac. But if you think that the physiologist and the pathologist could really set forth and explain all the differences between these membranes, then would peritonitis and meningitis differ in kind to you only when you used the word kind in a vague popular way, as when one might say that a knife is a different kind of a thing from scissors, or a pen from ink, in which case the phrase "different kind" means no more than that the differences are untraceable as regards the intents and purposes before you at the time. In short, popularly the word "kind" is applied to things in virtue of any likeness sufficient for the purposes of the occasion. This digression has been so long and our progress in the direct line so short, that you may scarcely remember that we digressed from the question, What are the general facts common to the several kinds of fever? into the question, What do you mean by a kind of fever? And now we must go back to the original question, and I must proceed to show you what those general facts are which are common to all fevers.

Stay though, we must have another digression, for I am afraid you might be content to regard every distinct kind of fever merely as a distinct sort of fever, and thus to ignore the relations of the several kinds of fever to one another. But if you take the facts of the several kinds of fever you will find that there are some of these facts special to each distinct kind, and others that are general to several of the kinds. And on perceiving and admitting this you must perforce regard the group of kinds of fever which have general facts amongst them, which facts are common to all of the group, but absent from all other kinds not included in the group. I say you must regard the group of kinds which have the common characters as in some sort genera of fever. And when you further find that some of the groups of kinds have facts common to such groups but absent from other groups, the groups thus connected must associate themselves into yet more general groups which become orders. So that, for example, Mr. Jones's fever has characters which bring it together with all cases of scarlatina, but amongst those scarlatinal characters is the fact of shorter duration, which makes scarlatina differ from continued fevers, but agree with a group including measles and small-pox, &c. But, again, amongst the characters common to all the group so included are some characters, such as the possession of a specific eruption, which bring Mr. Jones's scarlatina into association with the more continued fevers, typhus, typhoid, &c., but which separate it from all non-eruptive fevers. Mr. Jones thus becomes a case of the species scarlatina of the genus not continued of the order eruptive of the class contagious fevers, and thus you, as it were, perceive whereabouts Mr. Jones hangs upon the family

PYREXIA OR FEVER.



on the ignorant patients of the learned doctor's clinical remarks to his group of students. The impressions of the patient were rendered something in this way, "The doctor

tree of fevers, and you view his position with a greater total comprehension. Such a family tree view is a nosological view of fevers. You can construct such a tree as to

the family of fevers, but not as to other diseases. The nosology of other diseases is only an appendix of the anatomical and physiological arrangement of the several organs and parts of the body.

You will, I daresay, remember that all the while our question is, What are the general facts common to the several kinds of fevers? Well, now, you can see that we have reached a point from which we are at an advantage in solving that question. For when we have recognised what are the general facts that form the bonds of association of the several generations and families of fevers, then, as we have seen that these facts are themselves the principles of medicine with regard to fevers, we find that we thus reach at once our general principles, and can study them with due observance of their order of succession in importance. The facts common to the most inclusive group will be the most general facts and principles, and so on with less general facts to the less inclusive groups. Now, if you look at this scheme of fever, you observe that the most general division of fever is into primary and secondary fever. This is, indeed, not only the first in the order of nosology, but the first in practical application. In every case of fever that comes before you the first question is, Is it primary or secondary fever? One question only can precede this—namely, Is it a fever at all? What is fever? In its most general sense fever is still febris, fervid heat, and its only and sufficient criterion is heat. Not the feeling of heat. A patient may feel hot to you or to himself when he is really not so, and may be shivering with cold when he is thermometrically very hot, as in the cold stage of ague. But since the introduction of the clinical thermometer you can easily be quite sure of fever, or, as in this widest and most general sense it is more properly termed, pyrexia. You decide it by the thermometer. If his temperature is above 100° the patient has pyrexia. But in a vast proportion of cases pyrexia or feverishness is secondary to inflammation; and, in fact, before you pronounce a case to be fever you must be sure there is no local cause on which the pyrexia depends. Search for local inflammation. If there is no local inflammation, then it is fever, and it is called primary, or essential fever, or “fever” as distinguished from mere pyrexia. The next great characteristic, in which you find the first line of division of essential fevers is the character of contagiousness. This, too, is not merely first in order of generality, but in order of practical importance. When you are sure that the case is some kind of essential fever you will be asked by the friends, Is it contagious? A very serious consideration, on which depends the taking of immediate steps to ensure the safety of those around, steps which are annoying, troublesome, and costly, and which therefore must not heedlessly be enforced, but which are so imperative when necessary that there is no forgiveness if you fail to take them in due time.

And here I think it will be well to consider in a general way, and once for all, the great fact of contagion which is common to the more dreaded and deadly of the fevers. What do we know about contagion? Well, we seem on the very brink of knowing all about it, and yet we must perhaps on that very account be cautious. It is well to be circumspect on brinks. And the attitude of science is properly one of caution, especially of caution against the very probable. Science used to be always being misled by the probable. Therefore now let us challenge strictly all evidence when a general conclusion is to be drawn. The general conclusion is none other than that all contagious fevers arise through the entrance into the system of what are called “germs,” and that these germs are of the nature of living organisms, such as have from the first microscopical ages been known to microscopists as bacteria spirillum, micrococcus, &c. Many an hour have I spent in watching these, but never thought of associating them or any of their kind with fever; yet what I witnessed many years ago might have awoken a quicker perception to the anticipation of recent discoveries. I was watching floscularia, a stationary species of rotifer, resembling a relatively large bell with a Japanese quaintness of design about its figure. These creatures had come in large numbers in the aquarium at my dining-room window. Their textures are entirely transparent, and I was discovering their sense organs and the ganglion that represents their brain, and in particular I was watching the curious disappearance of a pair of bright-red eyes, which these creatures possess whilst they are young and active,—eyes which, like some other creatures, they lose when they grow old and acquire established positions. But, to my great sorrow, my floscularias, which had been thriving and multiplying freely, began

to die away before I could finish all the observations I had hoped to include in a paper which is in the Linnæan Society Transactions. As the creatures sickened they became turbid instead of clear; and whilst I was endeavouring to follow the nerves from the ganglia I saw that the obscurity which clouded them was due to the presence of countless bacteria, exactly such as I am now familiar with in Koch's figures. I now know that I was witnessing an epidemic of bacterium fever in a population of floscularias, but I was not seer enough to see what was before me then, as I looked for and thought only of nerves.

In 1873, Oberheimer of Berlin discovered spirillum in the blood of persons suffering from relapsing fever; and the fact that such organisms are in the blood in relapsing fever is beyond all question. This discovery might at first seem only a further extension of the knowledge we have of scabies through the finding of the itch insect. But there is this vital difference, that whilst the itch insect is living his quiet little life as an unwelcome guest, he creates no general disturbance except for some importunate sensations; whereas when spirillum gets into the system there is fever, and indeed intense fever. Evidently, then, the spirillum has a much more intimate and general relation with the system than the itch insect has, and it becomes a most interesting question what those relations really are. The facts are simple enough; indeed, are they not precisely such as science has long been aware of as occurring in the popularly well-known processes of vinous and acetous fermentation? In the fermentation and in the fever have we not definite systems of turbulent changes determined by the presence of living germs? Even before the discovery of the yeast plant fever was seen to resemble fermentation. Well, there we have a familiar and apparently close analogy to incline us to the view that relapsing fever, at least, is a colonisation by foreign living creatures which in some way breed a poison, or which, as some think, fill up and choke the life out of vital parts of the frame. This is a simple and plain view, and is easily accepted and understood. But the truth may not be so simple and plain, and I wish to draw your attention to an alternative view. This alternative view will appear to you if we ask the question, Are these spirilla—which, by the way, much resemble some spermatozooids—spermatic in any sense useful to recognise? Are they the offspring of the human body endowed with powers to disturb the vital processes of other human bodies? In a general way, there is a range wherein life and fever show enough in common to have been more than merely poetically or metaphorically parallel. For fever is like life in having its times of duration limited though uncertain, and in having its stages so that you can anticipate them, and you can recognise whether the fever is a young or an old fever; and, as our very question shows, fevers reproduce their kind. Here, then, are all the characters of a life: its uncertain yet sure limits; its stages of development; its power of reproducing itself. And so a fever might appear to be another life lived in and by its sufferer, and the human fevers would thus become, as it were, episodic lives supplementing human life; so that you have not quite lived until you have had your fevers; though it may be found for you that you have quite lived when in the middle of one of them.

There may be too much of some sorts of life. In strychnia poisoning or in laryngismus stridulus, for instance, the convulsions themselves cause death. But convulsion is itself an act of life, and so you may die by too much life. Life is a paradox, look at it as you will. You know that from the chemist's point of view life ought to settle itself at once by the several elements obtaining their favourite affinities, and being forthwith satisfied, which they never do until life is over. Life is such a paradox. But from my present point of view life itself is, as it were, a protracted infection of the germ by the sperm, and the living body remains during its life germinal to other infections when suitable spermatic elements come in contact. If this view seems to you more fanciful and supported by less analogy than the fermentation theory, I will at once put before you what is a serious difficulty in accepting the fermentation theory of fever. On this theory is it not very difficult to explain how it comes to pass that one attack of a contagious fever protects from the danger of that fever ever after? This, as we shall subsequently see, is not strictly true, but it is generally true. If fever is merely colonisation with germs who feed upon and live in the body, why should they not come and feed and live again? The only explanation I remember to have seen offered is that the unsophisticated human frame is born

with a small amount of some very special food of which the fever germs are very fond, and it is supposed that these little organisms are so fastidious, that they will not take any other nourishment, so that when they have consumed the whole supply of this kind of food, they die or depart to other feeding-grounds. Now, seeing that according to biological science these germs are only protoplasm, it would be very surprising if it turned out to be true that minute unspecialised organisms such as swarm in every puddle where organic decay is progressing should refuse to accept any nutriment other than some inborn kind of delicacy; and when you reflect that there must be a number of these delicacies, for no fever germs will pasture on the food of others, and in fact the ambrosia of one is caviare to the rest, it puts no little strain on the highest genius for imaginative scientific belief to suppose that we are all sent into the world each to bear a little special supply for the several kinds of fever germs to thrive upon.

On what seemed to you perhaps the more fanciful view we may suppose that the human frame can cast off spermatic elements which when they enter other human frames engage in an activity remotely like that which originates germination, so that for awhile, if I may so speak, the protracted germination which constitutes life is itself reinfected and diverted. We may suppose that this activity constitutes the fever; and that when once the fever is over and its life lived, it does not return. Were this really the case, would you not expect that these episodal lives would be most probably lived during the actively growing germinal years of earlier existence, or, in other words, would not the fevers attack children? Which indeed they do. Again, if you suppose the immunity from subsequent attacks which fever affords to be due to exhaustion of materials suitable to the parasitic germs, how can you explain those rather frequent cases in which typhoid fever or measles repeats its attack upon the convalescent patient, and even recurs a third time after defervescence? For if you suppose the immunity to be due to exhaustion of a material suitable to the parasitic germs, how can you explain the reappearance of the fever, and therefore of the germs with their supplies, just after the cessation of the first typhoid attack has shown that all supplies were exhausted? But if you concede that this proves the immunity in question not to be due to such exhaustion of supply, then how do you at all explain that subsequent immunity which is so marked and so happy a feature in the history of contagious diseases?

Well, then, the recurrence of typhoid fever cannot be a question of food of the germs. And if not, we must suppose that the repetition which makes three typhoids is due to some measure of time in the life of the typhoid germs themselves, so that each brood lives just the time of one attack, and the next attack is due to other broods whose life has a time limit equal to that of the fever. But these germs are minute unspecialised particles, which reach their little perfection with extreme rapidity, and all that is known of their rate of growth is the reverse of conformable to periods of fever measured by weeks.

On the other hand, if we suppose the fever process to be an episodic vital action between the elements of texture of different individuals, there is nothing contradictory in supposing such vital actions to be sometimes lived through a second or even a third time. Not long ago at the Pathological Society, there was under debate a question how we should explain the late manifestations of so-called tertiary syphilis and the syphilisation of the foetus whose mothers, having passed through the disorder, are not then actively diseased. And I endeavoured to show that these and similar facts are explicable by assuming that tissue of recent origin, or tissue by chance left unsyphilised during the syphilitic fever, undergoes impregnation with the syphilis germs which chance to come from habituated older parts of the frame into contact with the new tissue, either through such an intercommunication of the several parts of the frame as is supposed in Darwin's theory of paragenesis, or, else, in the more easily understood instance of the syphilitic foetus, through the blood of the mother. And I pointed out that the observations of Mr. Tomes on the Haversian spaces of bones, proving that fresh texture arises and replaces old texture in the life of adult bones; that these observations make it probable that similar new formation occurs in other textures, giving rise to new-formed tissue, which would be germinal to or capable of vitally receiving any poison which might be transmitted to it from the already syphilised general textures. If this is true, though I now cannot give you all the evi-

dence in favour of such a view, then, in tertiary syphilis we have occurring within the body of one and the same individual an infection of young and virgin tissue by the poisonous offsets of older tissues charged with a cause of infection to which those older tissues are themselves no longer susceptible. Such a fact would form a step towards the admission of a like infection by the tissues of an habituated individual of the tissues of another not yet habituated.

But I have hitherto overlooked a branch of evidence of the existence of which you must be duly apprised. Though I may be allowed to deal judicially with the facts as offered, and facts are offered by the most accredited observers which would go to prove that the germs of a fever to which mice are subject can be grown into a fungus which produces spores, and that the spores of this fungus will reproduce the fever in other mice, whilst the fungus threads fail to do so unless spores be present. Such researches are, of course, very elaborate, and when admiring the thoroughness and continuity of the work, and the skill and foresight displayed in the views of the experimenters, we must recognise also the extreme difficulties in avoiding fallacy, and the corresponding possibility that fallacy was not avoided; for to obtain the germs of fever about the 10,000th or 20,000th of an inch large, and to plant them and grow them to a fungus, you must be able to follow their individuality; nay, you should never, if possible, lose sight of the individuals. It would be well to identify these germs; and, indeed, it would be much more secure if they could be marked—say, for instance, by tying little bits of blue ribbon upon them to distinguish them from the infinitude of vulgar multitudes of other germs everywhere around them. And it might be said that these germs did deserve such a token of distinction on account of the sober way in which they behave exactly according to what the commanders in the experiment expected of them. Indeed, if you will trail a little poetry amongst bacteria, they are so numerous and light that they will gather around and give body enough to the form of your conception so as to make good your theory and set it going in living shape; and a biological theory will have a very happy life when enclosed in a body of active germinal infective protoplasm. It is, indeed, the most fashionable creation of the period. But one must not speak irreverently of protoplasm, and our subject is all too serious. The question may appear a very narrow one when we ask whether fever germs are independent parasitic organisms on the one hand, or specialised parts belonging to the human species originally taking their life from the human body. But to be stuffed with foreign germs is really so very different a thing from having your texture life set into new vital action by spermatic particles from the texture of another person, that it would be well to know which is the truth, if we can only find it out. As to the coarser view which has been advanced, to the effect that the merely mechanical action of fermentation germs may cause the phenomena and fatality of fever, we are not without an instance to show us what really are the consequences of the presence in immense numbers of microscopic organisms in the human blood. The recent discoveries concerning *filaria sanguinis hominis* show that these minute worms may exist in myriads and circulate with the blood without producing any fever, and, indeed, without causing any grave symptoms, and this when these worms are so numerous as to be immediately found in every drop of blood examined. Indeed, the history of *filaria sanguinis hominis* seems almost to give a crucial negative to any theory which would suppose fever to arise from a merely mechanical action of germs—that is, through mechanical obstruction produced by the germs. For when we thus speak of fever germs as perhaps always present in the blood, you must not suppose that one only has to get a microscope and a slide and put a little fever blood under it to find it full of germs. No, try in any of our cases of typhoid in the wards and you will find these germs by no means very easily discovered or obvious things. At the outset of such an inquiry you must take notice that the blood serum is often crowded with minute particles which must not be confounded with bacteria, and which exist often to a large extent in the blood of healthy persons. During last winter clinical session some of my most acute and intelligent friends, perhaps now present, searched carefully for germs in the blood of several severe typhoid cases. The result was that one bacterium was seen, only one, but I was told it was a very active one. When I say that Mr. Booth saw it, you will know it was

well seen, for we all regard Mr. Booth as one of the very ablest and very best students at Guy's, but perhaps the main fact was that all were quite sure that there was only one bacterium. Next lecture we will take up in detail the consideration of the history of contagion.

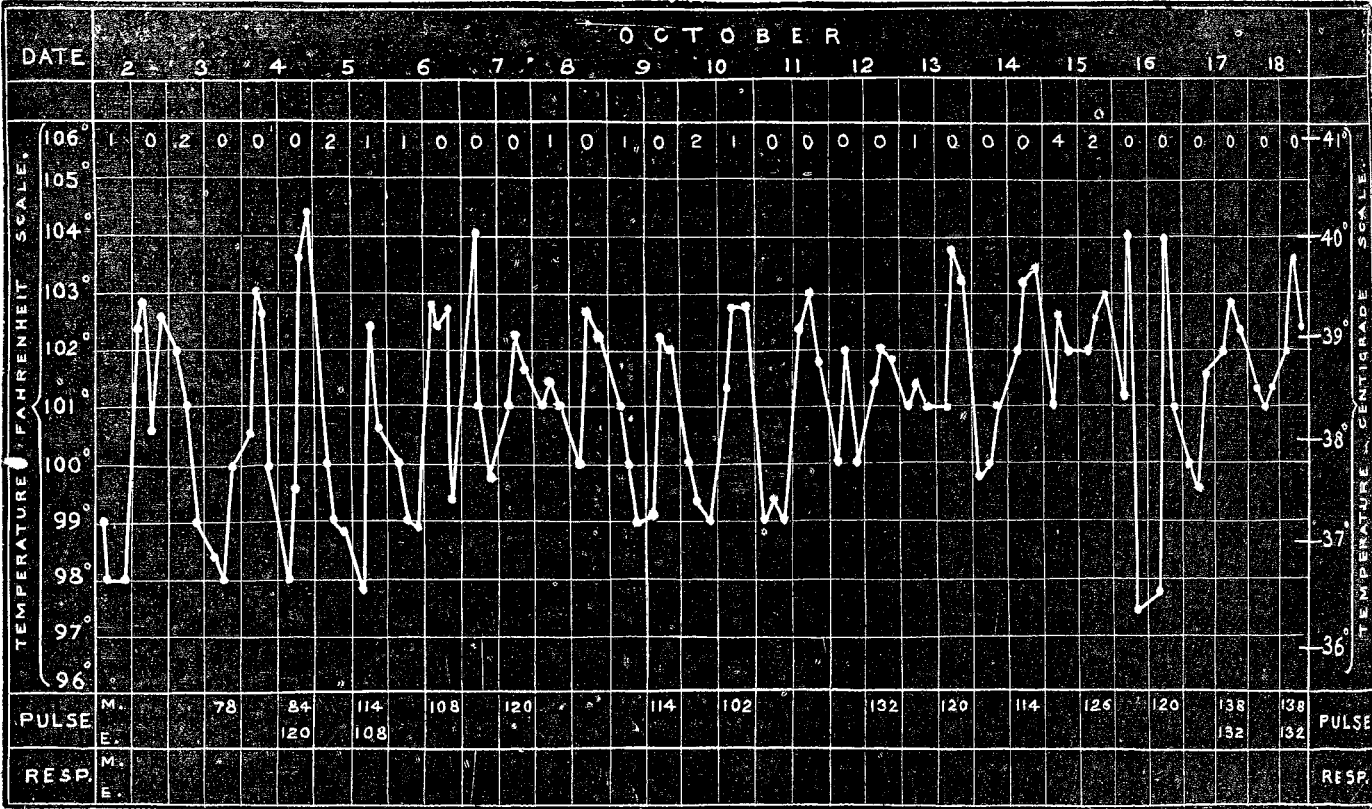
A CASE OF ULCERATIVE ENDOCARDITIS
WITH PYÆMIC SYMPTOMS.
BY JULIUS POLLOCK, M.D.,
PHYSICIAN TO CHARING-CROSS HOSPITAL.

ATTENTION has lately been drawn by Dr. Sidney Coupland, Dr. Wilks, and others, to a rare and interesting form of ulcerative endocarditis with pyæmic symptoms. In most, if not all, of the cases there has been old valvular mischief of the heart, upon which the fresh ulceration has been grafted, and some importance has been attached to the presence of former cardiac mischief. In the following case the symptoms came on during the course of a first attack of rheumatic fever, the heart being quite sound; and it has this further interest, that the case is absolutely complete, having been under observation from the first appearance of the rheumatic fever until the closing scene. I am indebted to Mr. C. A. Wigan, resident medical officer, for the carefully prepared notes of the case.

M. J—, aged twenty-two, a single woman, was admitted into Charing-cross Hospital, under the care of Dr. Pollock, on Sept. 7th, 1882, suffering from a first attack of acute rheumatism. On admission she complained of pain in both knees and ankles and in the shoulder of the left arm. She also had some pain and stiffness in the joints of the right hand. Temperature 101·2°; pulse 84; heart normal, with no irregu-

loss of power in the left shoulder. On the 5th the joints in both legs became painful and swollen, so a doctor was sent for, who recommended her to go into a hospital, and accordingly she was admitted into Charing-cross Hospital on Sept. 7th.

Her temperature on admission was 101·2°, and the pulse 84. There was the usual sour-smelling sweat, and the urine was high-coloured and acid, but otherwise normal. The heart was carefully examined, and found to be normal. As the bowels were confined, she was ordered ten grains of colocynth and calomel pill for that night, and the following mixture: Salicylate of soda, fifteen grains; carbonate of ammonia, four grains; decoction of cinchona, one ounce, every six hours. A chart of the temperature, taken every four hours, was started.—Sept. 3rd: No change in temperature. The urine contained no albumen, but was found to be very acid, and thirty grains of bicarbonate of potash and three grains of iodide of potash were added to the mixture. Pills repeated. The patient shortly lost all pain in the right arm, both knees, and ankles, but suffered much in her left shoulder and arm, the slightest movement causing great pain. Pulse about 90; heart normal.—28th: Patient still complains of great pain in the left arm, especially in the muscles of the upper part. The temperature at 6 P.M. was 102·8°, consequently the old mixture was discontinued, and the following ordered:—Sulphate of quinine, two grains; carbonate of ammonia, two grains; bicarbonate of potash, twenty grains; chloroform water, two drachms; to one ounce of water: to be taken three times a day. This seemed to have a very beneficial effect, as the temperature fell to normal, and remained so until the morning of October 2nd, when at 9 A.M., without any apparent cause, she was seized with a severe rigor, lasting fifteen minutes, and followed by a profuse sweat. The temperature ran up from 98° to 102·4°, but fell at 2 P.M. to 100·6°; rising again to 102·6° at 6 P.M.—3rd: The temperature at 2 P.M. was 98°, but another rigor, as severe as before, occurred at 12 P.M., the temperature



larity nor bruit. From her previous history she appears to have had measles when an infant, but no other serious complaint. Her father died of pleurisy (double) aged forty-four; had always been a healthy man. Her mother is still alive and healthy. Three brothers and five sisters alive and healthy; four died quite young. The patient states that on Sept. 3rd, the day before she felt ill, she had a hot bath and went out for a walk directly afterwards in the rain, and got wet through. The same night she felt chilly and sick, and vomited shortly after getting into bed a greenish fluid with a bitter taste. Patient got no rest during the night, owing to the feeling of sickness, which continued. The next day (Sept. 4th) she got up. She did not complain of any chilliness or fever, but felt some stiffness and experienced rising to 103°.—4th: Another rigor at 6 P.M.; temperature 104·4°.—6th: Another rigor at 9 A.M.; temperature 102·8°. The rigors were followed by a profuse sweat in a quarter of an hour. Up to this date the heart-sounds had been normal, and excepting some increase at the time of the rigors, the pulse-rate had varied from 76 to 96 per minute: but on the morning of the 6th a very soft and indistinct whiff was heard with the first sound at the apex of the heart. This gradually developed, until, on the morning of the 12th, a characteristic murmur was plainly heard at the apex with the first sound, and was gradually lost when traced into the axilla.—14th: Three grains of the sulphate of quinine were given in milk three times a day, and four ounces of brandy per diem. —16th: The murmur is now very dis-