

Details I am obliged to omit, and if that which I say seems to be meagre, believe me this subject has been before my mind throughout the preparation of this lecture more definitely than any other. Most of that which I have said tends towards this, the great object of our work, though it may be its tendency, is not always obvious. The treatment of these affections constitutes one of the most difficult and most obscure branches of therapeutics. This is not indeed surprising. We have seen to what a large extent they depend upon chronic processes. Of all slow processes of disease it may be said that the morbid process, at every period and at every stage, is an accomplished fact. Damage and cicatrisation go hand in hand, but the process of cicatrisation is not recovery. In acute disease the alterations caused in the tissue may pass away to a large extent, and the normal state to that extent may be reproduced. But a change indistinguishable from cicatrisation perpetuates all slow disease, and thus its effects necessarily endure. But we have some power even now, and we hear on every side lessons which should teach us to be very, very slow to give up hope or even anticipation, or to relax our efforts to obtain that which we now lack. How impressive is the lesson we have just had from science that the word "impossible" should find no place in our vocabulary. Even two years ago, had the wisest among us been asked the question, Is it possible that before the end of the century we should be able to see through a deal door—what would have been the answer? The humility which this should teach should equally prevent us from limiting the possibilities of power to any department of our science or any branch of the art that is founded thereupon. The reason why, through this effort to discern some of the relations of those subjective sounds, their treatment has been ever before me is because I believe that the first necessary step for the attainment of greater power is more minute and accurate recognition of their features and relations. It is through these that we must hope to be able to apply, with more prospect of good, the measures which we already know to have some influence, and by the discrimination they permit to employ any means which the future may give us. Empirical measures, on which we are obliged so largely to depend in many other maladies, have at present almost no place in the treatment of tinnitus. The treatment which is based upon a vague perception of the conditions which underlie the symptoms needs far more accurate discrimination of their character for its effective use. We know, for instance, that tinnitus is sometimes lessened by bromide, although the influence of this is far less than on the vertigo which is so often associated with it. But we know also that the influence of bromide is especially exerted on the centres in the brain. I doubt, indeed, whether we have at present any evidence of its action on the peripheral nerve structures. But the evidence I have mentioned that the auditory centres become secondarily involved in some cases and give rise to some special features of the subjective sensations is reason for a careful systematic attempt to discern the relation between such central coöperation and the power of bromide to relieve the symptoms. The possible power of relief by silence in suitable cases I have already sufficiently dwelt on. There are many cases of labyrinthine tinnitus in which counter-irritation has a considerable influence for good, but there are also many cases in which it is powerless and we are not yet able to distinguish the two. The distinction can only come, and probably will come, from careful discrimination both of the conditions under which the process develops and of the precise symptoms by which it is manifested. At present we cannot form any opinion, and certainly are not justified in forming any negative opinion, regarding the significance of differences in the minute characters of the subjective sounds. We are already supplied with so many means of lessening the morbid action of other peripheral nerves that it is strange if the early future does not yield us means of lessening, at least in some degree, that of the nerves of the labyrinth. We have almost no guidance in treatment from influences which are found by the sufferers to diminish the sound with the exception of the effect of silence already mentioned. In one labyrinthine case, it is true, the sound was always lessened by firm pressure on the posterior part of the temporal fossa, and the fact is suggestive, but it is no more. Another patient found that a diminution was always caused by inclination of the head, towards the side on which the tinnitus was heard, but this

I think, is only a special example of a more frequent influence. The sound may be often lessened by pressure on the arteries of the neck. Strange to say this result has seemed to me more common when the tinnitus is a continuous sound than when it is pulsating.⁴

I cannot now pursue the possible suggestiveness of this fact, but it leads up to another. I feel compelled to mention to you one case, which has been described in Germany,⁵ on account of the importance of the lesson it conveys. An elderly man had gradually become completely deaf in the right ear, with most troublesome tinnitus. The left ear was becoming deaf in the same way. The tinnitus was so loud, so persistent, and so distressing that the man had several times declared that he could endure it no longer and must end his life. This proceeding on his part was made unnecessary by the treatment which was adopted. It was found that pressure on the carotid artery diminished the sound. The right internal carotid was therefore tied. It is very difficult to understand the grounds for the operation. The blood supply to the labyrinth comes partly from the external carotid and partly from the basilar. The path by the auditory nerve to the opposite hemisphere is entirely within the blood-supply of the basilar. The opposite auditory centre is supplied by the opposite middle cerebral artery. It is thus easy to understand that the operation was found to have no effect whatever on the tinnitus. It produced left hemiplegia and left hemianopia and death resulted on the fifth day. I draw attention to this case because I think it should for ever prevent any repetition of this operation for the relief of labyrinthine tinnitus.

I wish that I could have made this lecture less unworthy, alike of this College and of the memory of him to whose request it is due. But when we find our work, our efforts to secure practical results, beset by conditions alike rugged and unyielding, one consolation is at least available—that that which is unsatisfactory now may be less so in the future. It does not do to pass such subjects by for those of greater promise. We have to till the fields to which our path may lead us, however barren they may seem, and hope that some of the seed sown, even upon stony ground, may germinate and bring forth fruit, at least in time to come.⁶

HYPOTHESES AS TO THE LIFE-HISTORY OF THE MALARIAL PARASITE OUTSIDE THE HUMAN BODY.

(*Apropos of an article by Dr. Patrick Manson.*)

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IN March of this year Dr. Patrick Manson delivered the Goulstonian Lectures¹ before the Royal College of Physicians of London on the Life History of the Malaria Germ outside the Human Body, in which, after having summarised the prevailing ideas as to the life of the malarial parasite within the human body, he discussed at length the various probabilities or possibilities in regard to its extra-corporeal existence. In so doing he has expounded a theory of his own and attempted to back it up with proofs which are, however, matters of speculation more than of observation. We will not occupy ourselves with what the lecturer says regarding the history of the life of the parasite in the human body since his remarks and observations are not new, but it will be worth while to examine briefly the lecturer's views as to the extra-corporeal life-history of the plasmodium, because even if they are not in our idea decisive of any question they may prove to be a starting point for fresh researches.

Where and how are the extra-corporeal forms of the malarial parasite to be found? asks the lecturer. There are

⁴ The fact that reduction of pulsating tinnitus by pressure on the vertebrals has been described by Dr. Dundas Grant and this leads me to express a hope that those whose observations are not mentioned will note the statement in an early part of the lecture. Its scope is limited to some of the facts which have come under my personal observations and the suggestions they seem to me to present.

⁵ Linsmayer: Wiener Medicinische Blätter, Nos. 8 and 9, 1893.

⁶ I hope to supplement this lecture by some notes on certain of the points which had of necessity to be omitted.

¹ THE LANCET, March 14th, 21st, and 28th, 1896.

two ways, he says, by which one can proceed in this investigation. First, to grope about in external nature in a more or less blind way, trying to find some form which, from morphological or other considerations, might be supposed to be the object sought for; next, when such a form is found, to endeavour to prove by experiment its identity with the malarial germ. Secondly, to commence with the parasite as it is found in the human body and to follow it through outer nature back to man again. That the first method must expose us to disillusion and failure is evident for obvious reasons; and this has been demonstrated by the history of malaria. The lecturer, therefore, prefers the second way, which he holds to be of itself almost certain. This hypothesis assumes, then, that the parasite can return from the human body into outer nature and there undergo development, whilst, as is well known, there are no facts which lead us to believe that the malarial patient diffuses or so to say, disseminates malaria about him. That the plasmodium must emerge from its human host and afterwards complete a new phase of life extra-corporeally the lecturer takes as proven by a series of theoretical considerations. He begins by excluding the idea that the parasitic life in man is a necessity to the plasmodium for the existence of the species (necessary parasitism). The reason is obvious. Malaria abounds where man is almost never seen, where evidently the parasite must complete, without his presence, its life cycle and go on multiplying itself. He excludes also the idea of its being an accidental parasite, also for obvious reasons. One of the characteristics of accidental parasitism is its relative rarity; now it is known that there are localities where all the inhabitants are malarial. Another characteristic of accidental parasitism is that the parasite, having found its way, so to speak, into its host, does not multiply; now, the plasmodium not only multiplies in man, but can even live in him for a certain time in the latent state. As the malarial parasite can thus live and multiply altogether independently of man and in man is a guest certainly not necessary and not even accidental we are forced to conclude, according to the lecturer, that the plasmodium finds in man an alternative host from whom it can escape and propagate as a species; that man, in other words, is only one of the media in which the parasite can exist, and that there is an alternation between its life in man and its life in other external media. The plasmodium must in this way be a true parasite of man, and not merely an accidental parasitic visitor. And if it is a true parasite, seeing that a means exists by which it enters the human body, so there must also be some provision made for its escape from the body; "otherwise the extinction of the species would be inevitable on the occurrence of the death of the host." If this were not so, the plasmodium would be different from other parasites in this respect and would not conform to the general law. This conviction, founded on arguments drawn from analogy, leads the lecturer to inquire how the plasmodium escapes from the human body; for granting his sequence of ideas, this becomes one of the fundamental questions. Considering *a priori* the escape as necessary, he does not ask if this happens as a rule, but only how it happens; and takes this inquiry as a starting-point for the study of the malarial parasite in outer nature. In what form does the parasite escape, and by what means? The attention of the lecturer has been attracted by the flagellate bodies. In speaking of these he confirms first of all the fact already noted that the flagellates do not form in the preparation of blood until some time after it has been taken from the patient; it is not, therefore, a form belonging to the intra-corporeal parasite; it is a form that appears only after the blood has been abstracted from the circulation. He returns to the free spherical bodies of quartan and tertian fever already so well described by Antolisei, and re-describes their transformation into flagellates. He returns in the same way to the well-known transformation of crescents into spherical and oval bodies and into flagellates. Now, in regard to this transformation of tertian and quartan bodies and of those of semilunar origin into flagellates, is it a degenerative process, or is it a developmental change naturally occurring in the life of the plasmodium? This, as is well known, is a much-discussed question. The lecturer naturally contends, in opposition to Grassi and Feletti and others, that the change is a developmental one. I limit myself to citing his arguments. He invokes first of all the test of movement. If movement is a sign of life, then in no stage of its existence does the plasmodium show such vitality as

when undergoing the transformation into the flagellate body. But Grassi has remarked that the same kind of movement can take place in dying forms or in degeneration. One cannot say in general terms that movement is a sign of life; one can only say so of special movements. Thus, when Marchiafava years ago contended, in opposition to many, that the young plasmodia without pigment are parasites and cited in proof their amœboid movements, he was justified, because amœboid movement, properly so-called, with the characteristics noted by Marchiafava, is not an attribute of degeneration of red blood-corpuscles. But it appears, on the other hand, that movements with the characteristics of those of the malarial flagellates, displaying the same vivacity, &c., are to be observed in degenerative forms. For example, the lively movements which are displayed by the bacilliform filaments which in special conditions—as, for instance, under the action of an elevated temperature—originate from the red corpuscles, are well-known.

The lecturer next insists on the fact that the flagellates are one phase of the regular and normal life of the parasite in its different variations. If we examine a case of malaria long enough, says Mannaberg, we shall always find flagellates. (We admit this in regard to summer fevers, and also tertian, but not for the quartan, where the presence of flagellates, according to our experience, is very rare.) To explain the rapidity of the formation of flagellates, the lecturer says it is more a phenomenon of birth than of development; perhaps the flagella pre-exist in the semilunar and in the spherical bodies. This is an opinion very similar to that already expressed long ago by Laveran, and is, according to our idea, in the present state of the question an arbitrary one, since it is not borne out by a study of the structure of the semilunar and of the spherical bodies. But the best argument, according to the lecturer, is to be drawn from direct observation. Whoever watches, he says, the flagellate bodies forming under the microscope will be convinced that he has to deal with living forms. It is therefore purely a matter of impression, which impression may evidently be fallacious. In the same way the flagelliform filaments of the red corpuscles give the same impression, so much so that they have actually been considered as living bodies and for the very same reason. But let us admit for a moment the views of the lecturer on the life history of the parasite. Granted that they are correct, what is the function of these bodies? Their life-cycle is completed only extra-corporeally, and this explains why the flagellates are formed only outside of the body. Considering the bodies from which they originate as mature the lecturer believes that an unknown cause prevents them from sporulating while inside their human host, and he regards the flagella as special spores which only form in outer nature. The flagellate body would thus be a parasite in the state of sporulation, "whose spores in the interests of the extra-corporeal life of the plasmodium" take this special form. It is easy to oppose to this the fact that the flagella do not contain chromatin and have the aspect and the structure of simple protoplasmic filaments. How then can they be looked upon as spores?

But let us continue with our account of the lecturer's ideas. If this be the significance of the flagellate bodies, how is their escape brought about in nature? Stated generally, it may be said that parasites leave their hosts, both definitive and intermediate, in one of the following ways: either by virtue of their own efforts, or through the efforts of the organism in which they are lodged, or by the action of extraneous agents, or in consequence of the decomposition of the host after death. That which is destined to become the extra-corporeal form of the malarial parasite is enclosed in a red blood corpuscle and cannot abandon its host either by its own efforts, or by being extruded in the excreta of its host. This cannot, then, be brought about except by the intervention of an external agent or by the decomposition of the host after death. This last hypothesis the lecturer leaves out of consideration, since malaria is rarely a fatal disease and he cannot believe that the escape of the plasmodium can occur only in an exceptional way. There remains, then, only one conceivable way—namely, by the intervention of suctorial insects, and more especially of mosquitoes, whose presence in the localities infested by malaria is known to all. The lecturer is also led to adopt this hypothesis from a consideration of the remarkable similarity between the habits and requirements of the plasmodium and those of the

flariæ—parasites which the lecturer himself had already shown to be dependent on the mosquito for their liberty and the opportunity for development in outer nature. Relying upon reasons of analogy, Manson puts forward the view that the plasmodia, having escaped from man, become parasites of mosquitoes and develop in them until they reach the stage of sporulation; that they then pass from the insect and are diffused in the soil, thence returning once more into man. It is easily understood, according to the lecturer, how the plasmodia, having passed from the mosquito into water, may be re-introduced into man with the same water. On the other hand, one can imagine how they would be inhaled when the ponds dry up and the plasmodia which are present in the larvæ and have been dispersed in the water pass into the resistant phase of their life-cycle on finding themselves in conditions unfavourable to their development, probably in these conditions becoming encysted, like other protozoa in similar circumstances. The dried-up sediment of the pools, transported by the wind and by currents of air, might be inhaled by man, and thus the plasmodium would eventually find its way again into its human host. On the other hand, the mosquitoes carried along by the wind might disseminate the germ in its resistant phase even in distant places and there deposit it, to be inhaled by man in the dust raised by the wind, or to infect him in some other way, during agricultural operations, &c.

Such is the theory of Manson, to support which Surgeon-Major Ross undertook in India a series of investigations the results of which were favourable, according to the lecturer, to the hypothesis in question. Ross studied especially the cases with semilunar bodies, because these can be more easily recognised than the other malarial forms which become transformed into flagellates. The chief thing, indeed, granted the correctness of Manson's views, is to study the development of the flagellates outside the human body. Ross, taking advantage of a case of malarial cachexia in which the blood was rich in crescents, and having collected many mosquitoes which had sucked the blood of the patient, undertook to make a systematic examination at brief intervals of the blood contained in the stomach of these malarious mosquitoes. At the same time he made a control examination of the blood of the same patient, collected on ordinary slides and prepared at corresponding times. He found that in place of being killed and digested in the stomach of the mosquito the crescents proceeded in development; almost all became spheres, and at least 40 or 50 per cent. of them became transformed into flagellate bodies, whence proceeded the free flagella. According to Ross this transformation of semilunar into spherical bodies occurs rapidly in the blood after its entrance into the stomach of the mosquito, the flagellate bodies being found from seven to thirty-five minutes afterwards, or after a still longer time. The same phenomena were observed in three cases. In one case in which quinine was administered Ross noted that crescents in ordinary preparations no longer became transformed into flagellate bodies (!), whilst the same blood in the stomach of the mosquito showed flagellates as before. This would lead one to believe, according to Ross, that the sojourn in the stomach of the mosquito is so favourable to the development of the crescents as to overcome the paralyzing influence of the quinine. Ross then attempted to communicate malaria to man by means of "mosquito water." To one person he administered some water in which malarialised mosquitoes had died after depositing their eggs. Eleven days afterwards the man was attacked by fever, the temperature rising to 103° F., &c., but no rigor. The fever lasted three days and subsided spontaneously; in the blood many annular forms of plasmodia were found; but although many examinations of the blood were made for two weeks no crescents appeared, nor was there any relapse. (I note that the course of this fever is so unusual for one of malarial infection that it seems to me permissible to doubt the exactness of Ross's observation: it is almost certainly an error of observation.) Ross afterwards repeated the experiment on other natives, but did not again succeed in producing fever of an unequivocal character.

According to Manson, Ross has, by these investigations, demonstrated "the first step" of his mosquito theory, proving by direct observation that the stomach of the mosquito is the natural medium in which is completed the development of the flagellated phase of the plasmodium. The aim of future researches should be, in Manson's opinion, to find out the fate of the flagella after they have become free in the stomach of the mosquito, for which purpose he believes it necessary

to study these insects by means of sections and appropriate staining re-agents. Manson foresees that many objections will be raised against the mosquito theory until (as he believes will happen) a complete demonstration is arrived at, but he does not consider them insuperable. The following are the objections he enumerates: mosquitoes abound in places where there is no malaria; there is said to be malaria in places where there are no mosquitoes—e.g., in some places on the West Coast of Africa where malaria rages. To the first the answer is easy; as regards the second the lecturer has collected information from which it appears that on the West Coast of Africa the matter is not quite as represented; while admitting that in some localities where fevers are common mosquitoes are not numerous, he says that close to the places where they are scarce there are others where they abound. He attaches no value to objections of a biological kind, as, for example, those which follow from the interpretation given to the flagellated bodies by various authors who regard them as degenerative forms similar to those seen in other protozoa shortly before death. Grassi and Feletti bring forward in support of their view the fact that the nucleus of the plasmodium takes no part in the development of the flagella. Labbé affirms the same thing; but Sacharoff, who also believes them to be degenerative forms, considers "the process of the formation of flagellated bodies as the result of a perverted karyokinetic division, the nucleus dividing into chromatin filaments, which leave the parasite; these filaments, which show lively movements, represent the flagella" (!). Against the objection that the mosquitoes do not attack birds Manson asserts that this is not true, experiences in China and India proving the contrary. According to the lecturer a more serious objection to this theory would be this—that up to the present the flagellum has not been followed into the tissues of the mosquito, and he ascribes this failure to the delicacy of the object, the complicated character of the structures, and the insufficiency of our technique. So much for the hypothesis of Manson.

If I am not mistaken it would be more rational to take another starting point in the search for the forms of the malarial parasite in its extra-corporeal life. Manson has been brought by his speculations to hold as a biological fact that the plasmodia, after having lived for a variable time as parasites in their human host, leave him in order to complete extra-corporeally a new life-cycle, and from this extra-corporeal existence return again to man. But if this were true the patient, on being transported into non-malarial districts where there are suctorial insects (and non-malarial districts infested by mosquitoes are known to all), ought to disseminate malaria; whereas no instance of such an occurrence has ever been adduced. Where a disease is concerned which has been so long studied from a clinical point of view, and about which popular experience has taught us much of the more reliable information we possess, it is not likely that such a thing could have happened without attracting someone's attention. Without desiring absolutely to exclude the possibility of the escape of the parasite and its subsequent development in the mosquito we cannot take such uncertain data as a starting point in our study of the question. In support of his theory Manson invokes a general law of parasitology; but when we look for a verification of that law from an examination of the facts we are constrained to have recourse to arbitrary interpretations of them. Thus, he asserts the flagellates to be a phase of life constant in all varieties of the malarial parasite, whilst in our experience in quartans they are only found exceptionally. He affirms that the flagella are spores, without bringing facts in support of the hypothesis. He interprets the observations of Ross on the way the parasites are modified in the stomach of the mosquito as proofs of a progressive development, without adducing sufficient reason for such a belief. It would be easy in the same way to hold a contrary opinion. Again, the general law which Manson cites might have exceptions, or, in other words, not be general in the strict sense. Without entering into a discussion which has already been done at length elsewhere, I desire here to call to mind the opinion already expressed by me and by Bastianelli² that the semilunar phase of malarial parasites, and naturally also the flagellates which are derived from them, represent abortive and sterile forms of that cycle of development which in nearly allied parasitic beings is only accomplished extra-corporeally. This view,

² Studi sulla Infezione Malarica, bollettino dell' Accademia Medica di Roma, 1893-94.

after all, would also agree with the general law to which allusion is made above. Now if this "necessary" escape of the plasmodium from the patient in order to complete extra-corporeally its life-cycle is more than improbable, the hope of seeing in this hypothetical escape the first extra-corporeal phase of the parasite naturally becomes very doubtful. But some way or other there is by which the parasite penetrates into the blood of man. We have nothing but theories and discussions as yet in regard to it, but if we could succeed in discovering what this way is we should perhaps have made the first step towards the knowledge of one at least of the phases of the extra-corporeal life of this creature. Indeed, it seems reasonable to think that investigation into the extra-corporeal life of the plasmodia should be preceded by the study of another problem—viz., in what manner and by what channel the malarial parasites penetrate into the blood of man.

It would take too long to explain all the theories which have been enunciated and the opinions which have been maintained upon this subject; they would nearly fill a volume. But a brief review will serve to place before us the actual state of the question. How is the fever contracted? Two theories are still discussed, that of water and that of air, according to which of these media is considered the vehicle of infection. The first, though it still counts not a few supporters, is open to an easy criticism deduced both from popular experience and from the experimental researches of Celli and Zeri. Laveran, who, as is known, is still the principal supporter of the water-conduction theory, enumerates the facts tending to show the importance of water as a means of carrying infection as follows. It has been proved several times, he asserts, that of individuals living in the same locality under identical conditions a large proportion of those drinking water from one source were attacked by fever, whilst those drinking water from another source escaped. That in certain unhealthy places it had proved sufficient, in order to bring about the disappearance of malaria, to supply pure water to the inhabitants instead of the stagnant water which they had been in the habit of using. That in very healthy places the fever can be contracted by drinking water coming from unhealthy parts, and that the individuals most susceptible to it are those who drink most water. That travellers who journey through unhealthy regions often succeed in keeping themselves free from fever by drinking only boiled water, whereas persons who do not take this precaution are struck down by it in great numbers. To the objection that infusoria and amoebæ are easily destroyed by the digestive juices Laveran answers that this may be the case where the juices are healthy, but in bad states of the digestive organs the protecting action of the juices may lose its effect. It is easy to reply that it is not on account of objections such as this that the hypothesis will cease to be tenable, but because the facts themselves upon which Laveran relies have not been authenticated. More particularly it has not been verified that the infection transmitted by water, in the way indicated above, was really malarial. It would be necessary to discuss such a question fact by fact; but already a lengthened criticism has been written upon it by Celli and Zeri, to whose works I refer the reader. Thus it has been demonstrated that the case of the ship *Argus* related by Boudin and referred to in all the treatises does not prove anything, since it is not at all likely that the soldiers of the ship were ill with malarial fever and not from some other infection. Thus, too, in the Roman Campagna popular experience in general attaches no value to water as a vehicle of infection; and in other localities where bad types of malaria prevail diligent observers, such as Ludwig Martin in Sumatra, Schellong in New Guinea, and Werner at Samara (Russia), arrive at the same conclusion.

The experimental researches which Celli has the merit of having initiated all point to the exclusion of water as a vehicle of infection. Celli made persons in the Hospital of Santo Spirito drink water collected in the Pontine marshes and in the marshes near Rome with negative results. Zeri, acting on Celli's advice, carried out three series of experiments. In the first place he made nine people drink water from malarious places in quantities of from one and a half to three litres a day, over a period of from five to twenty days, in such a way that each person consumed in all from ten to sixty litres while the experiment lasted. He next caused marsh water to be absorbed by the mucous membrane of the respiratory passages by means of a spray, experimenting

upon sixteen persons in this manner. Finally, in two adults and three children he experimented by injecting the water into the intestine. In none of these experiments did he succeed in producing the fever.

It may be observed that the majority of authors arrive at the air-conduction theory of malarial infection solely by the process of exclusion. Having excluded water from being, at least as a rule, the vehicle that carries the malaria germ into man, it seems as if no other channel were left by which infection can be conveyed excepting the passages of respiration. Now of all the diseases due to infection there is none which epidemiologists hold with more certainty to owe its origin to, or at least to have a more intimate relation with, the soil than malarial fever. Its infection is closely bound up with the soil, a view in accordance with the popular experience, to which, as pointed out by Tommasi-Crudeli, we owe the most valuable part of our knowledge of the epidemiology of malaria. But if we exclude, with good reason, the possibility of the infection being carried from the soil by means of water, in what manner does it come from the soil into the air? The supporters of the air-conduction theory in this very first question encounter difficulties for the resolution of which the investigations instituted and the proofs brought forward are defective and superficial. They find another difficulty in explaining why malaria is not transported by the winds, or at least if it is why its transport should have no practical importance in connexion with the diffusion of the disease, and yet another difficulty in accounting for the notable variations in the amount of malaria borne along in the atmosphere at different hours of the day, &c.

Let us see in what manner the difficulties are met. The transport of germs from the soil into the air might happen, as von Fodor³ remarks, by the dust rising from the surface of the malarious soil and the germs being inhaled with the dust particles. But I have observed that malaria does not behave itself after the manner of diseases due to inhalation of dust; that the more dangerous days are those that are sultry, without wind, in which least dust is raised, and especially the hot calm days which follow upon a rain, when no dust can rise from the humid ground. But many facts appear to be favourable to the hypothesis that a great factor in the transporting of malaria is the "ground-air" (Grundluft). Many observations prove that the fever is taken more easily in the evening or night than in the daytime, although during the day the air contains more dust from the surface of the soil than does the evening or night air, in which there is instead more "ground-air" present.⁴ Also for the same reason, according to von Fodor, it is dangerous to lie stretched out during the day, and especially at night, whence the ancient use of high habitations, &c., and the danger of confined air stagnating in the little valleys, whilst air which is open and in free movement, although it raises dust, is not dangerous.

We may conclude, with much probability of being correct, that malaria is not likely to be conveyed from the soil in those conditions in which dust is thus blown about. But even the hypothesis of the passage of the germs from humid earth into the "ground-air" (Grundluft) encounters various difficulties not easily to be overcome. As we are aware, von Fodor passed a large quantity of "ground-air" through a nutrient solution (Klebs'), and though he continued the experiment for many months he only succeeded in developing a colony of fungi; in other respects the nutrient solution remained sterile. Naegle, Pumpelly, Miguel, and Emmerich have demonstrated that air which has passed through an extremely thin layer of earth is perfectly filtered and free from bacteria, and Emmerich showed that bacteria only pass through dried soil. Afterwards other observers (Buchner, Renk, Petri, &c.) arrived at the same results by aspirating the soil itself or samples of earth and finding them free from germs.⁵ But we cannot content ourselves, remarks von Fodor, with these negative results because our epidemiological knowledge convinces us that some infections, for example that of malaria, are produced by micro-organisms which, according to the view generally accepted, rise from within the soil itself. Now there are some experiments which assist us in imagining how this passage of germs from the soil into the air may occur, although up to the present this has not been actually

³ Handbuch der Hygiene, herausgegeben von Dr. Th. Weil; Hygiene des Bodens, von F. von Fodor.

⁴ Von Fodor, p. 163.

⁵ Von Fodor, loc. cit.

demonstrated. Thus Buchner has called attention to the physical fact that if the water-level be lowered in a water-logged soil the capillary layers of liquid which are found around the particles of earth immediately on the top become gradually thinner and form as it were liquid lamellæ until they finally break up; then a part of the water contained in the layer is sprinkled about in a fine spray and dissipated in the "ground-air" (Grundluft), by means of which it may be carried away. If the bacteria are contained in this water they also would pass into the "ground-air." It is likely that the same thing happens after rains; but it is to be borne in mind that this passage of germs into the "ground-air" can only take place through those capillary lamellæ of water which break up near the surface, since otherwise the bacteria would be again filtered by a stratum (although a thin one) of earth. This brief examination of the facts shows then that if von Fodor is induced in part to refuse credence to the experimental data, it is chiefly because in nature the malarial germs pass from the soil into the "ground-air" precisely under those conditions in which experimentally this passage has not, in the case of other germs, been demonstrated. To maintain the common hypothesis one must therefore admit that what no one has been successful in proving in a convincing way in regard to ordinary bacteria can happen with great facility and as a regular rule in the case of malarial parasites.

That the winds do not transport malaria, except perhaps for a short distance, and that they have practically almost no share in the diffusion of the disease, has been maintained for a long time by Hirsch,⁶ and more recently, with much fulness of argument, by Tommasi-Crudeli. It is interesting that Hirsch avails himself of illustrations drawn from a study of the distribution of malaria in our own country of Latium. In considering the influence of the winds on the diffusion of malarial fever we ought to take into account, according to Hirsch, the modifications they produce in the state of the atmosphere and their capacity for transporting solid matters. In regard to the first point, we know that in various regions malaria is apt to prevail with a certain wind which affects the thermometric and the hygrometric conditions—e.g., with the sirocco in the malarial districts of Sicily and of the Italian mainland. The idea of the wind being the carrier of the malarial matter was started by Lancisi, the same observer who originated the theory of the paludal source of malaria; he believed that the conditions from which the unhealthiness of the Roman Campagna arose might be dependent upon the fact that after the cutting down of the woods in the reign of Gregory XIII. the emanations from the Pontine marshes found free ingress into the country round about; and other observers have concurred in this opinion and believed that it explains the baleful effects of the sirocco wind which blows towards Rome over the Pontine marshes. That the winds within certain limits may transport malaria the writer believes to have been proved by many instances, but that such a thing happens as often as has been made out appears more than doubtful. If the emanations from the Pontine marshes were the cause of the malarial fever of the Roman Campagna it is incomprehensible why Velletri, Genzano, Ariccia, Albano, &c., which are on the road between Rome and the marshes and ought to be the first and the most seriously affected by any harmful emanations carried by the wind, are, on the contrary, wholly free from malaria. If malaria could be transported by the winds for miles it would be difficult to understand its limitation to small portions of country, examples of which are to be found in Italy and other parts of Europe and in America. It would be easy to adduce many instances to prove that the diffusion of malaria by means of the winds is certainly very limited. The fact known to all that ships which anchor at a short distance from a dangerously malarial shore are not attacked by malaria is sufficiently convincing.⁷ To explain these facts the most arbitrary theories are had recourse to;

for example, it has been thought that the mountains might exercise an attraction on malaria and that great surfaces of water might absorb any malaria passing over them—in other words, the attempt is made to explain by unexplained natural laws things that are inexplicable.

(To be continued.)

THE EFFECT OF THE ROENTGEN RAYS ON URINARY AND BILIARY CALCULI.

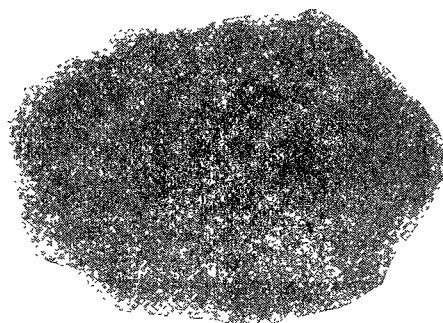
By HENRY MORRIS, M.A., M.B. LOND.,
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UP to the present time the New Photography has not rendered us any assistance in the diagnosis of renal and biliary calculi. Several attempts have been made for me with patients whose slight physical conformity rendered them favourable for the purpose, but hitherto without success. The interposition of the ribs, the position of the kidney close to the vertebral column, the depth of the cavity and the thickness of the overlying tissues are as yet obstacles which possibly in the near future will be overcome. In the meantime it has been a matter of some interest to ascertain the effect of the rays upon biliary and different kinds of urinary calculi after they have been removed from the body. The accompanying illustrations will give some idea of the results obtained.

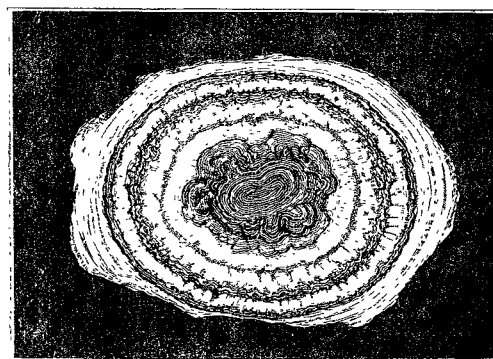
Figures 1, 2, 3, and 4 represent the shadows of four sections of three vesical calculi, and Figure 5 that of a

FIG. 1.



The section of a compound calculus an inch and a half in length and an inch in width, consisting of an irregular nucleus four-tenths of an inch in diameter, with concentric rings of uric acid and urates, in a setting of fusible calculus. Two rings of uric acid and urates interrupt the phosphatic deposit. These rings and the nucleus are darker in the skiagraph than the phosphatic parts of the calculus.

FIG. 1a.



The same calculus as Fig. 1.

be reconciled with the hypothesis that the malarial germ is in the air? From the moment that the parasite is suspended in the air it is certainly not easy to understand why the aerial currents should not be able to carry it for a few hundred yards. Such examples could easily be multiplied. It is known to Professor Marchiafava that of the inhabitants of Sezze those who live in the part of the village near the hills are exempt from fever, whereas those that live at a little distance away towards the marshes catch it easily. All the inhabitants of the village drink water from the same fountain.

⁶ Dr. August Hirsch: *Handbuch der Historisch-Geographischen Pathologie*. Erlangen, 1860.

⁷ Examples of places dangerously malarious near to districts completely or relatively healthy are not rare in Rome. Everyone knows that outside Porta del Popolo one can catch dangerous fevers, whilst a few hundred yards off, at the beginning of the Corso, the health of families living there even for many years is good. The Hospital of St. Michele a Ripa Grande is free, whilst at a short distance away towards St. Paul's there are places intensely malarious. The inhabitants of Palo state that in the same houses on the quay one can sleep with safety in the rooms on the sea side, whilst in those on the land side it is much easier to catch the fever. How can these and similar facts which have been communicated to me by Professor Marchiafava