THE LUMLEIAN LECTURES ON THE ÆTIOLOGY OF PHTHISIS.

Delivered at the Royal College of Physicians.

By J. ANDREW, M.D., F.R.C.P.,
Physician to St. Bartholomew's Hospital.

LECTURE III.

It was my intention to have dealt separately with the evidence as to the contagiousness of phthisis, and with its analogy in many respects to malarial fever; but the two diseases are so closely related, each throws so much light upon the other, in respect to the question of contagion, that to do so would involve much needless repetition without adding any force to my argument. From our review of the ætiology of phthisis, it is easy to recognise those points in which the two diseases strikingly resemble each other. I shall begin by stating their chief resemblances dogmatically, and without unnecessarily quoting well known facts in their support.

Both have been known from the earliest times, and in both the evidence of their having been introduced into any new country by man is very inconclusive. The facts, such as they are, are quite consistent with the view that the first discoverers and colonisers of any new country did not, in either case, import some special morbid germ, but found it already there.

Both, within historical times, have prevailed in varying degrees in the same locality. Thus, the increase of phthisis in America and in Australia is fairly comparable to that of malarial fever in Italy. In each case a change, more or less complete, in the race of the inhabitants of the country, with in the first case an advancing, and in the second with a retrograding civilisation, seems to have favoured the spread of the specific disease. Both are met with almost everywhere, but phthisis extends somewhat further towards the north than malaria. There is no need to involve human agency to account for this wide dispersal of either one or the other. The same fact obtains in the case of many of the lower forms of vegetable life, *e.g.*, the mosses and lichens. Of these latter several species, common in Britain, have been found in the Falkland Islands, Graham's Land, Kerguelen's Land, and in other antarctic islands or continents; on the Himalayas, in Chili, in Equatorial Brazil and South America, in Spitzbergen, Lapland, Iceland, Nova Zembla, and more northern Polar islands or continents. (*Popular History of British Lichens*, by Lauder Lindsay, page 97.) In the *Transactions of the Linnæan Society of London*, Botany, Vol. I, page 391, will also be found a long list of self-fertilising British plants of world-wide distribution.

Both, judging by their effects, vary in their characters to a certain degree in different parts of the world; and thus variation, in the case of malaria certainly, in that of phthisis probably, cannot be altogether due to differences in the personal factors. Compare, *e.g.*, the greater rapidity of the course of phthisis in the tropics, with the greater violence of malarial fevers in the same regions.

Both prevail under certain well known climatic and social conditions; and yet both are, here and there, unaccountably absent from likely and present in unlikely spots.

Both decrease in severity, and finally disappear, with increasing elevation above the sea-level.

Both are closely connected with wetness of soil, and both are greatly diminished, if not extirpated, by efficient drainage.

Both are sometimes confined locally within very narrow limits; *e.g.*, in malarious countries the second floor of the houses is said to be less dangerous than the first.

The action of the micro-organism in each is greatly modified by the personal constitution of those exposed to it. Thus, white races suffer specially from malaria, and negroes from phthisis.

In both, a bacillus has been demonstrated, cultivated, and proved by inoculation-experiments to be the exciting cause of the disease.

They differ in their habitat, phthisis being most prevalent in cities which afford all but complete protection against malaria.

Clinically, they agree in that recrudescence takes place after long periods of apparent health. In the case of ague, this frequently happens without any fresh exposure to infection; and there is often, as in so-called Roman fever, a well marked approach to periodicity in its returns. The history of many cases of phthisis makes it probable that the same thing takes place in it too.

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Again, in both the pyrexia is periodic. Some members of the group of specific febrile diseases—and those, too, of the most common occurrence—are so undoubtedly communicable, either by direct infection, as scarlatina, or by indirect, as typhoid fever, that the idea of contagion dominates the view we take of the entire class; and any disease which has once been placed in it, is forthwith regarded as contagious. But it is clear that, in different cases, the micro-organism, which is the exciting cause of the disease, stands in very different relations to the living creature which it attacks. Sometimes, as may be the case in typhus and in small-pox, the bacillus may be truly parasitical, *i.e.*, incapable of existing, or at any rate of multiplying, unless it meet with a certain "host." Of course, here the disease is directly contagious; and, apart from any question of the origin of species, every case of it is a proof that another case has already existed.

Sometimes, the bacillus may not be a parasite in so complete a fashion. It may be capable of enormous growth and multiplication in the soil, or in fluids of various kinds, and yet may require, if not for its very existence, yet for vigorous growth, to pass from time to time through the body of some host. This is probably the case with the micro-organism of typhoid fever. In both these cases, the resulting disease is almost certain to be highly epidemic in character, sometimes causing a vast number of deaths, sometimes almost unknown, or presenting only the mildest type for many years in succession. An instance of this in the case of a directly infectious disease, and of the cases in practice which may arise from forgetfulness of it, is given by Dr. Graves in his twenty-second lecture on the history of scarlatina in Dublin. In the last four months of 1801, and in the spring of 1802, scarlatina committed great ravages in Dublin. It ceased in the summer, but returned at intervals during 1803-4, when the disease changed its character; and, although scarlatina-epidemics recurred very frequently during the next twenty-seven years, yet they were always in the simple or mild form, so that, out of eighty boys attacked in a public institution, not one died. The epidemic of 1801-2-3-4, on the contrary, was extremely fatal, and left many parents childless. The long continuance of the mild type of scarlatina led to the belief that the fatality of the former epidemic was chiefly owing to an erroneous method of treatment, and that bleeding and antiphlogistics would have averted it. "This," he says, "was taught in the schools, and scarlet fever was every day quoted as exhibiting one of the most triumphant examples of the new doctrines. This I myself learned; this I taught; but the experience derived from the present epidemic has completely refuted this reasoning, and has proved that, in spite of our boasted improvements, we have not been more successful in 1834-35, than were our predecessors in 1801-2."

The history of typhoid fever, the disease which I have mentioned as the instance of indirect communicability, is essentially one of epidemics—epidemics varying much in type and in extent, but always explicable by some specific fouling of food or water. Sometimes the micro-organism may be entirely independent, if not of any host whatever, at any rate of man; its sojourn in his body may be unfavourable to its full development, and it may there cease to bear fruit after its kind. Of course, even here transfusion of blood, or inoculation with solid tissues or with the products of careful cultivation, might propagate the disease; but it would be absurd and mischievous to assert that this proved it to be communicable from the sick to the healthy in any practical sense of these words. This last relation between the germ and the disease which it induces probably holds good in the case of malaria; and with it, all the broad facts ascertained relating to its ætiology prior to the discovery of the bacillus, prove that phthisis is far more closely allied than with the other two classes of communicable diseases. Is there any reason why the discovery of the bacillus should make us reject the conclusion to which the other known facts, with but few exceptions, point? I can see none. That discovery has placed phthisis in the group of specific febrile diseases, a position which it ought to have occupied before the first bacillus was seen, a position which it ought to continue to hold, even though the true bacillus should never be seen at all. Of these diseases, some are communicable from the sick to the healthy directly, some indirectly, and this as a part of their natural life-history; but some are not communicable at all, except by artificial means, and then with difficulty. There is no *a priori* reason in the nature of things why phthisis should belong to one of these classes rather than to another, or why it should not occupy any conceivable intermediate position among them. It is true that its natural history as a whole indicates its affinity with the last named class; but the question, after all, is one which can be determined only by direct evidence.

What, then, are the proofs that phthisis is contagious? No doubt the belief that it is so is an old one, and might be supported by a most respectable array of authorities, easily neutralised by an equally

respectable array on the other side. By the regulations of the old hospital of the Knights of St. John at Valetta, embodying the details of the system in use from the foundation of the building in 1575, if not a still older tradition, the beds used by persons suffering from consumption are directed to be burnt, with all the sheets and other things belonging, without any reservation. But the force of this is not a little diminished by the circumstance that the beds of persons suffering from other complaints were also destroyed. In fact, the knights were, no doubt, very wisely resolved to be on the safe side in dealing with possible infection.

It will be convenient to consider, first, the effect of the success of inoculating animals with the blood or sputum of tuberculous patients, or with the bacillus alone, purified as far as possible by successive cultivations. It would be waste of time to adduce proof of the trustworthiness of these experiments, still more to raise captious objections against them. There can be no doubt about the matter. They are also a necessary part of the proof that the bacillus, or some similar organism, is the *vera causa* of tubercle, and that, consequently, phthisis is a specific disease. For the sake of this last truth, I could almost, were it necessary, bring myself to admit the truth of the doctrine of contagion. I also fully believe that the inoculation would be successful in man as well as in animals; for, indeed, the thing has been done some years ago. I have not seen the original authority, but quote from the account given by Mr. George Fleming in his well known article, "On the Transmissibility of Tuberculosis," in the *British and Foreign Medico-Chirurgical Review* for October 1874, p. 473.

Experiments conducted by Demet, Paraskeva, and Zallonis in Syra, Greece, were not only successful in transmitting the disease to rabbits by inoculation with the sputa and blood from a man affected with phthisis; but they ventured upon the unprecedented experiment of inoculating a human patient whose history gave no indication of tuberculous taint, and whose lungs were perfectly healthy, so far as a physical examination could discover, but who was suffering from gangrene of the big toe of the left foot, due to obliteration of the femoral artery. Amputation of the limb was proposed by the surgeon, but would not be submitted to; and as a fatal termination was inevitable, a quantity of sputa, from a man who had abscesses in his lungs, was inoculated in the upper part of the left thigh. About three weeks afterwards, auscultation revealed very slightly increased respiratory murmur at the summit of the right lung, and somewhat prolonged expiration in the subclavicular region. In thirty-eight days after inoculation the man died from gangrene; and, on examination of the body, it was found that the upper lobe of the right lung had seventeen tubercles in the first stage of development, two of which were of the size of a lentil, the others being as large as a grain of mustard; they were of a grey colour and very hard. Two similar tubercles existed at the apex of the left lung. At the centre of the convex surface of the liver were two tubercles, one as large as a shelled pea, the other smaller, both being very hard and of a yellow colour. From this unusual experiment, it is evident that tubercle is inoculable in man himself; for it is scarcely possible that this patient, who was 55 years of age, could have had his organs infiltrated with as many as twenty tubercles in the first stage of development only; had they been naturally developed, they must have multiplied and passed through their regular evolution, especially in an individual predisposed to phthisis. Their limited number, stage, and size, offered a direct relation to the brief interval separating inoculation from the decease of the patient.

It is to be observed that this unique case lends no support to the idea that the period of incubation in phthisis is a long one, and that, consequently, the disease may be contracted by exposure to infection two or three years before its first symptoms declare themselves. In this respect, the result coincides closely with that of inoculations of animals; in which the creatures killed within five weeks after the operation presented tubercle-growths, which must have begun not less than two or three weeks before death.

There is also another point relating to inoculation of animals to which it is necessary to call attention. In the course of the numerous experiments carried out by various observers about sixteen to eighteen years ago, it was supposed to have been established, among others, by no less an authority than Dr. Burdon Sanderson, that tuberculosis could be produced in rodents by inserting substances which could not themselves have possessed any specific properties, e.g., setons of unbleached cotton. This is Dr. Sanderson's account of his experiments in this direction; and of the inference to be drawn from them given in the tenth report of the Medical Officer of Privy Council for 1867, and which he again affirmed before the Pathological Society in April 1868.

"It has been found that, if animals are inoculated with pus de-

rived from the secondary abscesses of patients affected with pyæmia, death is apt to occur during the third or fourth week after inoculation, in which case the results of dissection are as follows. Abscesses are found at the seat of inoculation, and purulent deposits in the neighbouring subcutaneous tissue; these, although obviously of the same character as those observed after inoculation of tubercle, differ from them in being larger, more numerous, and more generally distributed. The subcutaneous lymphatic glands are healthy, or at most a little enlarged. The internal organs are usually free from disease. When this is not the case, lesions of an obviously inflammatory nature are found; as, for example, pleuropneumonia, pericarditis, or sub-pleural or subperitoneal abscess. If, on the other hand, the animals survive the primary, i.e., pyæmic, effects of which these lesions are the expression, and are examined several months after inoculation, morbid appearances are found in the lungs, liver, and spleen, in which all the anatomical characters of a tuberculous process are to be recognised.

"From these facts it is evident that these lesions can no longer be regarded as dependent on any property or action possessed by the inoculated material in virtue of its having been taken from the diseased organs of a phthisical patient. This conclusion is rendered almost certain by the result of the following experiment.

"On October 29th last, setons of unbleached cotton were inserted, by means of a needle which had not been used before, in the shoulders of two guinea-pigs, which, from their being entirely white, could be easily distinguished from any of the other animals under observation, all of which happened to have spots of colour. One of these animals was killed on December 28th, and was dissected by Mr. Simon and myself. One of the setons was surrounded by a hard mass of semi-transparent substance, obviously dried-up pus, which was surrounded by connective tissue induration. The other abscess was of considerable size; the material contained in it was of the consistence of cream-cheese, and, on microscopical examination, the corpuscles of which it consisted were found to have the characters of pus. The internal organs were perfectly healthy. The other animal died on March 15th. There were two open wounds on the left shoulder, each of which led to a sinuous cavity, containing a small quantity of pus, the greater part of the contents of the abscesses having escaped externally. Both cavities were connected with each other, and with the neighbouring scapular gland, which was enlarged and softened, by subcutaneous bands of induration, and there were numerous purulent deposits in the neighbouring muscles and areolar tissue. The lungs contained very numerous iron-grey nodules. The liver was much enlarged, and presented the characteristic appearances of interstitial adenoid deposit. In the spleen, there were numerous miliary nodules. There was enlargement of the agminate glands, both of the large and small intestines, as well as of the lymphatic glands in the gastrohepatic omentum and mesentery. In this case, therefore, a pathological process which had originated in traumatic subcutaneous suppuration, resulted in lesions which were characteristically tuberculous.

It is impossible to neglect a deliberate statement like this, or to put it on one side with the answer that there must have been some fault in the mode of performance of the operation. We have no right to assume that in this, as in all other similar experiments with the same result, tuberculous material must have been introduced into the wound; and yet it is almost certain that these same experiments repeated now, with the fuller precautions suggested by a better knowledge of the extreme difficulty of excluding bacilli or their spores, do not give rise to the same specific lesion. The comparison of the two sets of observations, former and recent, with these seemingly contradictory results, calls to mind the difficulty found by Professor Tyndall in preventing the access of certain bacteria to his solutions. These earlier experiments were in one sense failures, although, from another point of view, they are most valuable, as proving the necessity of strict antiseptic precautions, which could scarcely be required if the tubercle-bacillus were not generally diffused. It must further be remarked that, notwithstanding the readiness with which tubercle is produced in guinea-pigs by inoculation, and even the difficulty of preventing an unpoisoned wound affording an inlet to the disease, the creatures, in their natural state, are not specially liable to it. Even when kept for physiological purposes, and in the near neighbourhood of the spot where experiments are being carried on, they rarely contract it through the natural channels. Thus, in March 1869, Dr. Wilson Fox exhibited at the Pathological Society some specimens as the first instance which he had ever observed, out of nearly two hundred guinea-pigs which had passed under his hands, of spontaneous tuberculosis occurring in these animals independently of local injury; adding that, in this particular case, the disease may well have been inherited, for the parents had been inoculated, and had both died tuberculous within six months of the inoculation.

The results, then, of direct inoculation justify fully the assertion that phthisis may be communicated artificially; they fall far short of proving—that, indeed, other evidence disproves—that it is likely to spread through a community by personal intercourse, immediate or mediate, after the fashion of scarlatina or typhus; whilst the results of the older experiments with indifferent substances point to the probability that the bacillus of tubercle is widely diffused, like that of ague, which can also be successfully inoculated, and that, therefore, the risk of infection is not, perhaps, greatly increased by personal contact with the sick. The bacillus malariae, so far from gaining increased virulence by its sojourn in the human body, must become less dangerous; and, looking to the capricious distribution of phthisis, and the absence of any sufficient proof that it has been imported into any country, the same may also be the case with the bacillus of tubercle. Anyhow, we require some further more cogent proof of its being propagated in communities by contagion.

Such evidence has been sought for in the results of feeding experiments. Granted that direct inoculation must be a very rare event in common life, the contamination of air, water, and food, by sputum or other excreta, containing, as they certainly do, bacilli, might be at least as frequent an occurrence as it is in the case of scarlatina and typhoid; might, indeed, be very difficult to prevent. Again, as in inoculation-experiments, there is no room to doubt the result of artificial feeding. An animal with whose food bacilli-containing substances are mixed becomes phthisical, if not always, yet so frequently that it would be hypercritical to deny the connection between the food and the development of the disease. Whilst, however, in an epidemic of scarlatina or typhoid fever, the distribution and the relation of the cases to each other almost always lead the investigator to the primary source of infection, at first unknown; in phthisis, with full knowledge of the foci from which the disease might spread in this way, we fail to discover anything of the kind, except in very rare instances and on the smallest scale. In this connection certain veterinary experiences, rather than experiments, are specially interesting. In the article by Mr. Fleming already referred to, there is an account by M. Grad, a veterinary surgeon in Alsace, of a most remarkable series of events. A large farmer at Leinheim, five years in succession, lost one cow by phthisis, and always in the same stall. A sixth animal, selected for its vigorous health and the absence of any taint of phthisis in its pedigree, shared the same fate. Then all the woodwork of the stall was removed, the manger and rack thoroughly disinfected, and the spot left unoccupied for some time. It was then rebuilt and re-occupied by several animals in succession, but tuberculosis made no more victims among them or in any other part of the stable. It is difficult to imagine a stronger case of a virulent contagion, or of one confined for years within such narrow limits; for the stalls were "very short stalls," preventing the animals from injuring each other with their horns, but apparently not separating them by the whole length of their bodies.

In this case, the disease must have been transmitted by some non-volatile substance in the stall itself; otherwise, and if the breath of the tainted animals had been infectious, it is difficult to believe that its neighbours on each side, for six years, could all have escaped unhurt. Indeed, their escape, so far as it proves anything, proves that people of cleanly habits run little or no risk in occupying the same room with a sufferer from consumption; for the cows probably fell victims to their habit of licking the rack and the sides of the stall. But the whole story is so exceptional that it may have been an instance of local, not of personal, infection; and on this theory the extremely narrow area within which so destructive an agent was confined for many years becomes a little less perplexing.

The largest collection of observations upon the communicability of phthisis among men, and by far the most important, inasmuch as it includes communications from men of large experience and of every shade of opinion, is that for which we are indebted to the Collective Investigation Committee of the British Medical Association. The report, based by the Committee upon the facts submitted to them, evidently leans to the contagionist side; but the premises are so faithfully given, that it is easy for anyone to judge for himself how far the conclusion drawn from them is correct. There is, however, one fault in the mode in which the investigation was conducted, which must have had considerable influence on the result. In the questions circulated the Committee ask for affirmative cases only; notwithstanding this, they received several answers containing cases which appear to disprove the contagiousness of phthisis. The number of these would have doubtless been much larger had they been specially asked for; and in any future inquiry it is to be hoped that this will be done. In consequence, a great number of the returns, 673 out of 1,078, were simple negatives, of which the Committee could make no further use.

The statement that the word "No" means that no case had been observed by the member making the return, without any expression of opinion, is true to the letter, but not to the intention, of some of those who made those returns. Thus a friend told me, with some slight indignation, that his "No" was a very active little word indeed, by which he meant to convey, not merely that he had seen no cases of contagion, but that he had noted its absence in cases where, if present, it must have manifested itself. I may add that he is a man who has enjoyed exceptionally large opportunities for studying phthisis. Thus, while the affirmative observers are pretty certainly present in their full proportion of 261 in 1,078, the proportion of negative observers is certainly not fully represented by 105; the majority of affirmative observers is unduly magnified; perhaps, even, if negative facts or opinions had been asked for, the majority might have passed to the other side.

The first of the groups into which the affirmative returns are classified is that of cases between husband and wife only. Here, as also in Dr. Hermann Weber's remarkable series of similar cases, the disease generally appears first in the husband. In 188 out of 192 returns under this head, in which it is stated which partner suffered first, in 119 it was the husband, in 69 only the wife. As Dr. Longstaff has pointed out, a certain unascertained number of these cases between married people would occur accidentally—i. e., if there were no such thing as contagion possible; and these would affect both sexes pretty equally; so that in the remainder the difference between the death-rate of the two sexes would be even greater than it appears to be from the full figures. As it is, the disproportion is so great, that it requires some explanation. A common remark, containing some truth, is, that the woman's natural office of nurse confines her more closely to the sick-room, and exposes her to more and greater chances of infection, than would come upon her husband were she herself the sufferer; in that case, the duty of providing extra comforts for her would oblige him to work even harder than usual at his occupation, and to spend less, not more, time in her company. Even on the diathetic theory, this would be quite sufficient to explain the greater mortality among the wives. Grief, time spent in anxious tendance, and under most painful conditions, hardships and privations endured that the sick dear one may lack nothing which can be of any service or comfort to him, a life spent almost entirely within doors, and very likely in a close ill ventilated room, all this would, on the diathetic theory (which, by the way, is perfectly true so long as it is confined to the predisposing causes of phthisis)—all this would account, in the most natural simple manner, for the excessive mortality on the female side. In point of fact, the personal contagion-theory fails signally to explain the facts under this head. For it must be remembered that there is a most important difference between the contagion of phthisis, if it exist, and that of most, if not all, other diseases; and that is, its duration. It is not an affair of a few weeks at most, but of months, or even years; and, during a considerable portion of this time, there may be but little change in the domestic life. The husband is still able, if with difficulty, to follow his calling; the wife has no need to undertake the special duties of a nurse. The danger of personal contagion is fairly equal between the two for a very long period, whichever may show the first symptoms of the disease. But there is yet another powerful factor, quite distinct from any phthisical contagion properly so-called, which turns the scale heavily against the wife. This is syphilis, which undoubtedly, in most cases where it exists, is transmitted from the husband to the wife. The relation of syphilis to phthisis is by no means simple or uniform; and the number of cases which may be called more or less truly syphilitic phthisis is, I believe, very large. Sometimes, an affection of the lungs closely simulating phthisis, which may be seated at the apices, and is frequently attended by repeated and profuse hæmoptysis, is really nothing more than a visceral syphilis, and, though powerfully predisposing to, need not be followed by, tubercular phthisis. I have known cases of this kind begin within a few weeks of the appearance of the primary sore, and end in complete recovery when treated as syphilis.

In the seventeenth volume of the pathological *Transactions* is a valuable drawing from a case by Dr. Hermann Weber, showing the earliest appearances of the lung-affection and its beginning in the lymphatics. If the nature of the illness be not recognised, considerable destruction of the lung-tissue may take place, and, if the bacillus is met with, tubercular phthisis will follow; but even then the syphilis may continue to form an important factor in the complex morbid process.

A few weeks ago, a patient was admitted under my care in St. Bartholomew's Hospital for hæmoptysis. There was a history of chest-affection, with more than one attack of hæmoptysis, lasting over two years, and with marked physical signs of consolidation at both

apices, especially, I think, the right. Luckily, there was also present on one leg a circular sore of very characteristic appearance. Under the use of perchloride of mercury, he rapidly regained flesh and strength, the sore healed, and the physical signs almost entirely vanished.

This is not the place to discuss individual cases; but the existence, by no means uncommon, of this form of disease, seems to me to give a more reasonable explanation of the undoubted fact that sometimes two or three wives, in rapid succession, fall victims to phthisis, than to be obliged to believe that a contagion, ordinarily so mild as even its advocates represent that of phthisis to be, should yet exhibit such portentous and fitful activity. The same explanation may perhaps hold good in some other cases of apparent direct infection, than those between husband and wife. On the whole, the excess of female mortality in the married cases seems to me to be almost inexplicable on the direct personal contagion theory, simple and natural if we attribute it in part to the presence of a certain number of syphilitic cases, in part to increased predisposition to be affected by a widely diffused exciting cause, produced by the special duties of the wife, and the indoor nature of her occupations; and to these must further be added the fact that the constitution of the wife soon becomes more or less assimilated to that of her husband, whilst but little, if any, change takes place in his. It is a little remarkable that this exceedingly doubtful class of cases should furnish no fewer than 192 out of the 261 affirmative observations. In reviewing the remaining groups, notice must be taken of the very small number of instances, especially of those observed between unrelated persons, which amounts at most to 21; and this, be it remarked, in the case of a supposed infectious disease, the contagion of which, if it exist at all, is active, in the majority of its victims, for months, or even years. If cases of typhus or of scarlet fever retained their contagious property for an equal period, no human being could escape them. And yet, upwards of one thousand observers, speaking from a collective experience of many thousand patients, can only record about a score of cases in which the disease was transmitted between persons unrelated to each other.

Contagia which act directly from person to person, and which are probably parasitical in the true sense of the word, are, for the most part, extremely powerful. They do not recognise slight differences of race or family, or climate or subsoil-water, or even of altitude above the sea-level. The only protection against them for the unseasoned is to be found in the isolation of the sick, and in the most perfect ventilation of the sick-room; but phthisis behaves after quite a different fashion. Among persons living together in the same house, or even in the same room, it picks out special strains of blood, or attacks in one family the males, in another the females only. A widow with eight children, having lost one, came up from the country and occupied a small house at New Cross. One who spent but little time there remained in good health. Of the other seven, two died of phthisis; one in 1872, the other in 1873; and two more became seriously ill with the same disease. These left the house; both gradually recovered, and are still alive. The house was a small one; the family was crowded together; conditions highly favourable for contagion certainly existed. But there were two families. The father had been twice married, leaving five living children by the first wife, and three by the second who survived him. Not one of the latter has suffered from phthisis. The only blot on the family history of the first wife was that a niece had died of consumption. The circumstances made a great impression upon me, and at first I was disposed to look upon it as an instance of contagion; but now my belief is that there was some local influence in the house, something distinct from any personal infection.

In the last group of cases observed between persons unrelated only, consisting of the very small number of eight, the following is perhaps the most remarkable.

"Miss R., aged 48, a dressmaker, living in rather a lonely cottage in Bedfordshire, had three apprentices, young girls from 17 to 19 years of age, not related, from three adjoining villages, who took it in turn to remain in the house and sleep with her, each one for a week at a time. During their apprenticeship Miss R. was taken with phthisis, of which she died. In less than two years afterwards all three apprentices died of phthisis, although in the family history of each no kind of phthisis existed, and the parents, brothers, and sisters, of two of them are alive and well at the present time."

Here again there are various little difficulties in the way of the explanation by contagium. This must have been most virulent in the case of Miss R., and yet no harm came to any of the friends of her unfortunate apprentices. Again, I find it difficult to understand how the same contagion can be now and then so destructive as this, and yet for the most part so inert, that it is impossible to trace its operation. After all, the number of these astonishing facts is not greater

than might be fairly expected to occur as accidents or coincidences in the case of a disease so common as phthisis. But this is an unsatisfactory mode of regarding them when, in the light of the recognised general laws of the distribution of phthisis, and of the exceptions to those laws which point to a wide and yet capricious dispersal of its exciting cause, they can be so naturally explained as instances of special local infection. The following quotation from Dr. Bowditch's paper on consumption in Massachusetts will illustrate my meaning.

"1. Four grandparents lived to a healthy old age, and died over eighty years old. One of the grandmothers always bore on her neck scars, said to be from scrofulous ulcers in early life.

"2. Two brothers, A. and B., married two sisters, both couples being children of these grandparents. All parties were in perfect health at the time of marriage, and with no thought of consumption ever entering their families.

"3. After his marriage, A. stayed in the old homestead, situated on a southern slope, a dry and warm well drained spot, where he was born, and there all his children were born and reared to adult life. The statement of the two families may be made briefly as follows.

"Both families, as we have seen, had by one grandmother slight tendencies to scrofula, although consumption proper had never entered into the known tendencies of any ancestors. This was, in fact, a strong belief in the minds of both families.

"Of thirteen persons (parents and children) in family B., nine have died; seven (more than two thirds of them) by consumption, one of croup, and one of diabetes. Of the surviving four, father and three children, one has been for years slowly suffering from apparently pulmonary disease. The three others, viz., father and two sons, are healthy, but those two sons have not lived in the homestead for many years. On the contrary, of the eleven persons (parents and children) in family A., three have died (less than one-third), and not one of them died of consumption, though one died early with scrofula."

It remains to be added that B.'s home was situated at the foot of a hill in the midst of wet meadows, and not far from a bog which is wet all the year through. Dr. Bowditch gives several other similar histories of houses in low damp situations, whose inhabitants met with a like fate. The sporadic distribution of the bacillus furnishes the best explanation of cases such as these; and that this distribution is effected by means of direct personal contagion is extremely unlikely. Were it carried to, it would almost certainly be carried from, those foci of infection; and this by no means always happens. It did not do so in the case of Miss R.'s three apprentices, in the instance just quoted from Dr. Bowditch, or in my own case at New Cross. Indeed, in this last, the fact came out very strongly, not only that the infected persons did not serve as carriers of the disease to others, but also that they themselves showed marked and rapid signs of improvement when withdrawn for a time from the unwholesome local influence. To say nothing of Professor Tyndall's bacteria-clouds, there is no want of facts in connection with plant-life attesting the possibility of organisms flourishing most vigorously in very narrow, isolated, and widely separated spots; e.g., in *Island Life*, by Mr. Alfred Wallace, at page 481, note, I find the following statement. "A correspondent in Tasmania informs me that whenever the virgin forest is cleared in that island, there invariably comes up a thick crop of a plant locally known as fireweed—a species of *Senecio*, probably *senecio australis*. It never grows except when the fire has gone over the ground, and is unknown except in such places. My correspondent adds: 'This autumn I went back about thirty-five miles through a dense forest, along a tract marked by some prospectors the year before; and in one spot where they had camped, and the fire had burned the fallen logs, etc., there was a fine crop of fireweed. All around for many miles was a forest of the largest trees and dense scrub.'

"Here we have a case in which burnt soil and ashes favour the germination of a particular plant, whose seeds are easily carried by the wind, and it is not difficult to see how this peculiarity might favour the disposal of the species for enormous distances, by enabling it temporarily to grow, and produce seeds in burnt spots."

Again I must repeat here that those cases only can be admitted as evidence of contagion in which the disease appears within a moderate interval after the last exposure to the supposed source of infection. Granting that the period of incubation is unknown, and may even be very variable, still all our present knowledge, gained from inoculation and feeding experiments, and the results of exposing animals to an atmosphere loaded with finely divided tuberculous matter, point to the probability that the period is a short one. Certainly cases in which it must have lasted for over a year ought not to be used as a double-barrelled argument to prove at once the contagiousness of phthisis and the great length of its incubation. These cases are best explained by the theory of local, as distinct from personal, infection.

It is noteworthy that scattered instances, apparently establishing the existence of contagion, occur in the history of agues as well as in that of phthisis. Thus in the article on "Intermittent Fever," in the *Cyclopædia of Practical Medicine*, edited by Drs. Forbes, Tweedie, and Conolly, I find the following passage. "Some authors have supposed ague contagious. This was the opinion of Baunarez and Cibat in Spain, Bailly and Andouard in France, and Cleghorn and Fordyce in our own country. Cases have fallen under the writer's observation which have led him to entertain at least a suspicion that such was the fact; but the most forcible case for the affirmation of the question which he has met with is one related by Bailly. 'A lady arrived in Paris with an intermittent fever, which she had contracted in the country in a marshy situation. This fever was accompanied by violent vomitings, and other serious symptoms, which displayed themselves at every paroxysm, and forced me to give bark. Scarcely was she cured, when her husband, who had never quitted Paris, but who had the imprudence not to keep himself apart from her during her illness, was struck with the same symptoms, and in a manner altogether similar.'" As I have endeavoured to show in the case of phthisis, apparent instances, such as this, of an infectious ague may have been either accidental coincidences, or cases of the recrudescence of an old contagion, or due to some unsuspected local, not personal, infection; perhaps, now and then, there may have been an error in the diagnosis of the first case, in the same way that it is by no means difficult to mistake visceral syphilis for tuberculosis.

Without denying the great difficulty of proving a negative, there is still no doubt that the non-occurrence of phthisis under certain conditions does establish a strong presumption that it is not spread by direct contagion. The facts relating to the Brompton Hospital, recorded by the late Dr. Cotton and Dr. Theodore Williams, are too well known to need repetition here. But the same immunity from phthisis has also existed among the various officers of the City of London Hospital for Diseases of the Chest, to which I was myself at one time attached. On this point, I have been favoured with the following statistics by the Secretary, Captain Storrar Smith.

"Of thirty-one physicians who have had care of patients there since 1855, with one single exception, the details of which I cannot learn, none have suffered from phthisis. Of twelve resident medical officers, whose term of office has varied from one or two to five years, one only has died of phthisis; he was attacked by hæmorrhagic phthisis, which ran a very rapid course, before he had been more than about a twelvemonth in the place. Of five chaplains, none have suffered from phthisis. Of three secretaries, one was delicate when he took office, and eventually died of some chest-disease. Of four matrons resident in the hospital, the first and the present one having been each there for more than ten years, none have had phthisis.

"Since 1867, there have been 255 nurses, sisters, and female servants employed in the hospital; of these, one, a nurse, has died in the hospital of phthisis, and thirty-three have left on account of illness. Of these latter, some, but certainly not all, were phthisical.

"Since 1865, there have been thirty-four male servants at the hospital. Of these, one, the house and out-patient porter, died of phthisis, at the age of 48 or 50, after seven or eight years' service. He had been in the army, had a fistula, and drank freely. Another out-patient, after service with the institution from its commencement, was discharged in 1882 on account of ill-health; a week or two ago, he was in the hospital with bronchitis.

"Out of seven dispensers since 1860, one died while away on sick-leave, probably from phthisis. His wife died a few years before from phthisis. He was aged about 50 when he came to the hospital, and 63 when he died.

"Of fifty-one clinical assistants, the appointment involving residence, I know certainly that two or three have become phthisical; but I have not been able to learn the history of a sufficient number to draw any conclusion. I think, however, that, in the case of very many of them, the fact of their being attacked by phthisis would have come to my knowledge."

On the whole, then, facts negative the idea of any direct contagion under the ordinary conditions of hospital residence and service.

In conclusion, let me recapitulate the conclusions which I have endeavoured to establish as to the ætiology of phthisis, with special reference to its contagiousness. None can be more sensible than myself of the many imperfections in the matter and form of my argument.

1. The historico-geographical argument is insufficient to prove that the present distribution of phthisis has been brought about by the carriage along lines of human intercourse of a special morbid germ. Indeed, many of the facts under this head are distinctly antagonistic to any such theory.

2. Before the discovery of the bacillus, one and all of the reported

causes of phthisis were inadequate to account for its distribution, or for the anatomical and clinical characters of the disease.

3. Those causes, even those which appeared to act as exciting causes, were all predisposing causes only.

4. From the nature of those predisposing causes, their relation to each other, and the conditions under which their influence seemed to make itself felt, it was a probable inference that phthisis belonged to the group of specific febrile diseases; and this view was held by some writers in the face of many difficulties and perplexities.

5. The facts on which this inference was based were insufficient to prove that phthisis was personally contagious, and were, indeed, rather opposed to any such notion.

6. The discovery of the bacillus proved that phthisis was a specific febrile disease; and thus the question of contagion cannot now be usefully discussed without acknowledging this fact.

7. As some specific febrile diseases are contagious, and others not so, this property existing in very different degrees and modes in different members of the group, the question as to the contagiousness of phthisis can only be satisfactorily answered by determining its affinities with other members of the group and by distinct evidence of its contagiousness.

8. Although phthisis may be undoubtedly produced in many ways experimentally in animals and also in man, there is not sufficient evidence to prove that its prevalence is materially affected by direct contagion.

9. In many most important respects it very closely resembles ague.

10. It is at least highly probable that the exciting cause of phthisis, like that of ague, the bacillus, or some other micro-organism, is in no way dependent upon man for its existence, and is widely diffused, irrespective of human agency.

From these, I may be allowed to make one short practical deduction; viz., that the prevention of phthisis, like that of ague, is to be attained by sanitary works, especially by improved ventilation and drainage, and not by isolation; and that, for its cure, as we should not send a case of ague to the Pontine marshes, so, too, it would be wise not to send a case of tubercular disease to any place where the death-rate from phthisis is high among the native population.

ON THE MARINE HOSPITAL SERVICE FOR SICK AND HURT MERCHANT SEAMEN OF THE UNITED STATES.

By SIR WILLIAM SMART, K.C.B., K.L.H.,
Honorary Physician to the Queen; Ex-President of the Epidemiological Society.

THE Marine Hospital Service of the United States is a branch of the Treasury Department, allied to that of Revenue or Customs, having charge of quarantine and the health of the Mercantile Marine and Revenue Service, quite apart from that of the Navy; with its own hospitals at the great ports, even where naval hospitals are established. It has existed now eighty-six years; and, during the last eleven years, its annual reports have been published. The volume for the year ending June, 1883, is nearly four times as large as our own *Statistical Report of the Health of the Navy*; and yet it is concise in all its chapters, with complete statistics for each of the eight coast-districts, admirably presented in full nosological detail, followed up by reports of cases, some of which are illustrated with photographs. The last one hundred and twenty pages contain very valuable reports on two epidemics of yellow fever on the east and west shores of the Gulf of Mexico, with topographical maps; and the whole is made the more practical for reference by a copious and clear index. In every way this volume does credit to the contributors and to the Marine Hospital Service in general for its matter, style, and completeness.

This administration for the benefit of merchant seamen is analogous to our own for the treatment of the sick of the Royal Navy on our own coasts, in possessing its special hospitals at great centres, supplemented elsewhere by sick-quarters, under the management of selected medical practitioners, who may or may not be charged, in addition to treatment, with the entire provision for the wants of the sick in contract hospitals or lodgings, with allowances that vary much at different localities. Thus, in contract hospitals, the authorised charges, generally a dollar (4s. 2d.) a day, vary from 3s. to 6s. for lodging, board, nursing, with, sometimes, medicines; increased for contagious diseases from 6s. to 20s., but without any regard to rank; and the medical treatment is by officers of the service. Where that is not the case, and private practitioners are employed, they are remunerated with monthly wages, from £2 10s. to £9 3s. 4d., according to the liabilities; or, it may be, by fees of 4s. 2d. a visit, as at the contract quarters at Edenton, in North Carolina, where the subsistence is 4s. 2d. a day,