THE LUMLEIAN LECTURES

ON

THE ÆTIOLOGY OF PHTHISIS.

Delivered at the Royal College of Physicians.

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LECTURE I.

THE ætiology of phthisis covers so wide a field, requires for its complete discussion so many different lines of inquiry, so many different methods of research, that it would be absurd to attempt to teach it in detail within the time at my disposal here. Nor was such my intention in adopting these words for the title of these lectures. It was convenient to have a short heading, and yet one which would allow me the fullest liberty to choose, from any quarter, whatever facts or arguments there may be available for my more limited purpose. Far from attempting an exhaustive treatment of so vast a subject, I shall regard it from one comparatively narrow point of view—viz., the communicability of the disease immediately or mediately from the sick to the healthy. Is or is not phthisis contagious?

And here it is my duty at once to confess myself to be an anti-contagionist, and to admit that, in spite of the sincerest wish and endeavour to state the case fairly and without prejudice, my argument must have been, in form if not in matter, more or less biassed by its being framed rather to attack a given proposition than to establish any other in its place.

This question of contagion, wherever it arises, whether in the case of phthisis or of other diseases, has rarely been discussed with judicial calmness. The practical issues are so important, and appeal so strongly to the imagination, whilst facts lend themselves so readily to contradictory theories and *ex parte* treatment, that the opposite sides too often deal out to each other but scant courtesy, or even justice. And thus the argument is carried on by arguments unsound and tainted from the very first—by ingeniously manipulated statistics, by facts incompletely given, and by utterly irrelevant appeals to feeling. It may be in part from this emotional element in the controversy that the very idea of contagion has often been rejected in cases which seem to us now scarcely to admit of even speculative doubt. Thus, Dr. John Armstrong hotly denied the contagiousness of typhus fever in his *Lectures on the Morbid Anatomy, Nature, and Treatment of Acute and Chronic Diseases* (posthumous edition by Joseph Rose, 1834), and this, too, in cases where glandular swellings and a petechial rash were present which it is impossible to believe to have been cases of typhoid fever. I am well aware that very different opinions were held of Dr. Armstrong's character and of the value of his opinions; but, by the testimony of friends and foes alike, he was the most popular teacher of his day, a careful observer, who had enjoyed special opportunities for studying fevers; and yet in so plain a case as the spotted fever of London and Edinburgh he renounced his belief in its contagiousness.

At the very outset of the inquiry, a difficulty confronts us which has never hitherto been satisfactorily met, which I, at any rate, cannot hope to remove, but over which I must somehow contrive to stumble. It is the definition of phthisis. All that I can do is to state the sense which I myself attach to the term phthisis, and to beg that my argument may be judged of by reference to that and not to any other description. Of course, it must be so framed as not to beg the question under discussion, and must, therefore, be imperfect. By phthisis, then, I mean a specific disease, characterised, anatomically, by a new growth, which may wear very different aspects at different periods of its existence, and in different organs or tissues, especially, according as it is found in the interior of organs, or as it presents upon a free serous, mucous, or cutaneous surface. The new growth may occur in almost any organ or tissue, but does so most frequently in the lymphatic tissues and in the lungs. Generally it proceeds from numerous different points, the growths from which may or may not coalesce, thus leading, e.g., in the lungs to two different types of lesion, viz., miliary tuberculosis, and to more or less extensive areas of infiltration and consolidation.

Further, in the case of the lungs, this new growth does not follow the general law of distribution or localisation of pulmonary diseases.

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It has some special affinity for the apices, and descends in levels, attacking the upper portion of the lower before it has reached the base of the upper lobes. It kills by the disturbance and disorganisation which its presence works in some vital organ, and thus the disease answers roughly to the old definition of Joannes Marifeldus, in the fourteenth century, in his *Breviarum Bartholomæi*, the earliest medical work by any member of the staff of St. Bartholomew's Hospital, with which I am acquainted, "ptisis est ulcus pulmonum cum consumptione totius corporis," (*Anecdota Ozoniensia*, vol. i, part i, 1882). But its clinical history is that of a general rather than of a local disease; the constitutional symptoms, especially in the earlier stages of the disease, bearing no definite relation to any ascertainable structural lesions.

In the words of Dr. P. M. Latham, "pulmonary consumption is no more than a fragment of a great constitutional malady, which it would be in vain to think of measuring by the stethoscope, and which it belongs to a higher discipline than any mere skill in auscultation rightly to comprehend" (*Latham's Works*, New Sydenham Society, vol. ii, p. 171).

I know, only too well, that this definition, if I may call it so, bristles with controversies of fact and of theory. Let it be taken then as the expression of my belief, that consumption, phthisis, tuberculosis, is a specific disease occurring in both acute and in chronic forms; that it is not a mere catarrhal inflammation, a peribronchitis, a chronic apex-pneumonia, but a disease as distinct in its nature as scarlatina, or as gout. And further, let me here add, that a very large, if varying, proportion of deaths caused by chronic lung-diseases are rightly attributed to it.

No doubt it is impossible to get any large statistics of tubercular phthisis which are not more or less vitiated by the intrusion of non-tubercular cases, but the broad general conclusions which may be chosen from those statistics are trustworthy; and perhaps in the presence of other than tubercular cases is to be found the explanation of some of the apparent anomalies and inconsistencies which make it so difficult to formulate the general laws of its causation and natural history.

The question of the contagiousness of phthisis must be beset by peculiar difficulties, for in no other cases has the discussion raged so long without even yet leading to any generally accepted conclusion. To this conclusion it is now necessary for me to mention the well-known names of those who, at different periods, have taken, some one, some the other, side in the dispute. The recent discovery of the tubercle-bacillus, and the consequent addition to the old stock of arguments of a fresh fact of the utmost importance, must be my excuse for venturing to bring so trite a subject before you. It seemed to me all the more allowable to do so, because the bacillus has been looked upon by many as a conclusive proof of the doctrine of contagion in its most extreme form; and, as often happens when some striking discovery, some fruitful generalisation, has just been made, a strong tendency has been shown to treat the older views and observations with contempt. In order to estimate the influence of our knowledge of the bacillus upon the doctrine of phthisis, I shall endeavour, first, to state the position of that doctrine with the value of the facts on which it was based, prior to Dr. Koch's discovery; and, secondly, to discuss the modifications which that discovery may render necessary in the older views. In this way only can we expect to arrive at a safe conclusion. In carrying out this plan, however, I must ask your forbearance, when, as must often be the case, you find me traversing well-known paths. An advocate would surely do his duty in a strange fashion who omitted to state an argument because it was an old one and hitherto unanswered.

It will be convenient to consider, first, one argument in favour of contagion, apparently of great weight, which was very strongly stated by Dr. Wm. Budd of Clifton, in his famous Memorandum on Phthisis which appeared in the *Lancet* for October 12th, 1867. He says: "Among the data relating to geographical distribution the following striking facts may be here mentioned. 1. When the South Sea Islands were first discovered, phthisis did not exist there. Since the aborigines have come into intimate contact with Europeans, the disease has not only made its appearance among them, but has become so widespread as to threaten their extermination. The contrast between original entire immunity and present extreme fatality is very striking, and can only be rationally explained by the importation of a new and specific morbid germ. Try every other supposition, and the facts are inexplicable; make this our supposition, and they are at once explained. 2. The late Dr. Rush of Philadelphia, who made very accurate inquiries to determine the point, satisfied himself that, when America was first discovered, phthisis was unknown among the native American Indians; now it is very fatal to them. The very significant contrast here exhibited between the past and present history of these

two races, in respect of phthisis, is exhibited at once, and at the present time among the negro races in Africa, in different parts of the area of that great continent. It is well known that negroes are peculiarly liable to phthisis. Now, everywhere along the African seaboard, when the blacks have come into constant and intimate relations with the whites, phthisis causes a large mortality among them. In the interior, where intercourse has been limited to casual contact with a few great travellers or other adventurous visitors, there is reason to believe that phthisis does not exist. Dr. Livingstone and other African travellers have given me the most positive assurance on this point."

It would be impossible to exaggerate the importance of this statement if it could be established. Fortunately or unfortunately, this seems to be impossible. To take, the case of America.

The oft quoted authority of Dr. Rush, when analysed, is very far indeed from being decisive of the question of the importation of phthisis into North America. It is known that in his "Inquiry into the Natural History of Medicine among the Indians of North America, and a Comparative View of their Diseases and Remedies with those of Civilised Nature," he does not mention consumption. "Fever," he says (p. 20 of London edition of 1789), "constitutes the only disease among the Indians;" and under the head of these fevers he mentions specially "pleurisies and peripneumonia." On p. 21 he asserts that small-pox and the venereal disease were communicated to the Indians in North America by the Europeans, but he did not, so far as I can find, take the same view of consumption. In point of fact, what Dr. Rush states is not merely that consumption was unknown in the north-west of the New World before its discovery by Columbus, but that it was still unknown among the Indians at the time when he was writing in the latter half of the eighteenth century; i.e., nearly three centuries after Columbus, consumption had not been communicated by Europeans to the natives. He gives not the slightest hint of believing in its foreign origin, but regards it, even in its incidence on the Whites, as one of the results of advancing civilisation. This he says in "Thoughts on Consumption" (*Op. cit.*, p. 159):

"I shall begin my observations upon the consumption by remarking: (1.) That it is unknown among the Indians in North America. (2.) It is scarcely known by those citizens of the United States, who live in the first stage of civilised life, and who have lately obtained the title of the first settlers. The principal occupations of the Indian consist in war, fishing, and hunting. Those of the first settlers are fishing, hunting, and the laborious employments of subduing the earth, cutting down forests, building a house and barn, and distant excursions, in all kinds of weather, to mills and courts, all of which tend to excite and preserve in the system something like the Indian vigour of constitution. (3.) It is less common in country places than in cities, and increases in both with intemperance and sedentary modes of life. (4.) Ship and house carpenters, smiths, and all those artificers whose business requires great exertions of strength in the open air in all seasons of the year, are less subject to this disorder than men who work under cover, and at occupations which do not require the constant action of their limbs. (5.) Women, who sit more than men, and whose work is connected with less exertion, are most subject to the consumption."

It is clearly unnecessary to draw from this passage any more stringent conclusion than this: that the climate of North America had not sufficed to prevent the development of an hereditary tendency to phthisis among certain of the settlers living under conditions which would have produced the same effect in Europe. It must further be remembered that Dr. Rush writes of the savage tribes of the West and North; but there is reason to believe that phthisis existed in Mexico before the arrival of Europeans. N. M. Bancroft (*Pacific States*, vol. ii, p. 592), speaking of the Nahara natives, the leading tribe among the Aztecs, says: "The diseases most prevalent were acute fevers, colds, pleurisy, catarrh, diarrhoea, and, in the coast-districts, intermittent fevers, spasms, and consumption, aggravated by exposure." Even if this statement is not literally true, if some other disease has been mistaken for phthisis, it is still an apt illustration of the uncertain character of the evidence relating to the diseases of the American aborigines. Yet phthisis might easily exist among barbarous races, and escape the notice of even the most competent observer. A savage stricken with phthisis would rarely live to develop the later and more striking features of the disease. The value of his life in the earliest stage would be a vanishing quantity, like the "prairie-value" of land. With the first serious failure in strength of lung or of muscle, the struggle for existence would be promptly decided against him. In the words of Dr. Rush, speaking of deformed children, the severity of the Indian manners destroys them. And thus civilisation, with its care for the sick and weakly, allowing time and opportunity for the full evolution of the disease, would sometimes appear to have intro-

duced that which it had only preserved and, so to speak, cultivated. An almost necessary consequence of this artificial survival of the unfit would be the spread of the phthisical taint, in virtue of its hereditary character (to say nothing of the unwholesome influences of a nascent civilisation), and a great increase, both absolute and relative, in the number of cases of phthisis. From this point of view, it is interesting to note even indistinct traces of the existence of phthisis among the people who had attained the highest degree of civilisation in the New World, who kept trustworthy records, and even established hospitals for the sick. Nothing can be more unsatisfactory than the reports of the first visitors of any newly discovered land on the diseases of its inhabitants. The writers themselves seem to have been very often singularly unfitted for such a task, and the opportunities at the disposal even of the best qualified observer most insufficient. It could not be possible to learn much during a stay of a few weeks, or it may be only days, among a newly found people, speaking an unknown and unwritten language. Taking the three vast regions specially mentioned by Dr. Budd—regions containing among them nearly every variety of climate and of natural productions—I trust I have sufficiently illustrated the difficulty, if not impossibility, of proving that phthisis was introduced into America from Europe.

In the Pacific, the first European visitors of many of the islands were South Sea whalers, escaped convicts, or, at best, intelligent sailors, like mariners, without any special medical knowledge; certainly not persons well qualified to conduct a very difficult scientific inquiry. It is impossible, in the absence of trustworthy records, to come to any certain conclusion; it is easy enough to meet with statements on either side. A friend, a member of the Melanesian Mission, who has known many of the islands well for some years past, assures me that he believes consumption to have existed among them before the arrival of Europeans. At any rate, twenty years ago, he saw natives dying of what seemed to him to be consumption, in islands which had been visited only by the mission-ship, in which no infection had been conveyed. The case is very different with regard to a highly contagious disease like syphilis. There seems little room for doubt that it was in part brought to the islands by European vessels, and that it is still being constantly reintroduced. Unhappily, it is impossible to deny the great mortality now caused by phthisis among the Melanesians. In the mission-establishment in Norfolk Island, there are about 150 young natives, who stay there for one or two years, to be trained as teachers. Even there, the mortality from phthisis is about two per cent. annually, and in the islands themselves it is probably much higher.

In the case of Africa, it has to be remarked that, although very few white men penetrated into its interior, regular caravan-routes have existed for ages, by which, e.g., starting from the Mediterranean coast, with its thickly set European colonies, any disease so contagious as some believe phthisis to be, must have been spread over the whole interior, even before the time of the Romans. Such a disease, once started, would have travelled surely, if slowly, from one trade-centre to another. By the medium of neutrals, it would pass freely between hostile tribes, and penetrate into the remotest districts. So that the distribution of phthisis in Africa, its prevalence on the coast, and its rarity inland; its being confined to certain parts only of wide regions traversed for centuries by largely used trade-routes, is a strong argument not for, but against, its contagiousness. If its spread depends upon the conveyance of any material germ along the lines of human intercourse, it is difficult to believe that the interior of Africa could have escaped. In a general way, a better explanation of its unequal incidence is to be found in the fact that, in other countries to which phthisis has been long known, and where no question of restricted intercourse arises, it still prevails to a much greater extent near the sea than in the interior. Further, it is not true that phthisis rages among the natives along the entire coast-line. There are singular exceptions to this general rule, even in the case of districts, such as the Gold Coast, being the seat of large foreign trades, exceptions which it is very difficult to explain on the contagium-theory.

Granted, however, for the sake of argument, that it could be established as a certain fact that phthisis has made its appearance among a certain number of savage tribes after the arrival of Europeans, and that it was unknown before that event, surely the importation of a new and specific morbid germ is not the only rational explanation which can be offered.

If, for the time, we adopt the view that phthisis is a diathetic disease, the outcome of unhealthy conditions of life, especially those connected with the two prime necessities of mankind, shelter from the weather, and food, then it is well known that the first contact of civilisation with barbarism subjects the savage to many most unwholesome influences. He is tempted to indulge to excess in alcoholic liquors,

often of the vilest quality. He forsakes his old free life of hunting or fishing for one of comparative inactivity and indolence, hanging about the white settlements, and seeking shelter in the most unsanitary of huts.

What room for surprise is there if, under such changed circumstances, he becomes liable to new forms of diathetic disease? If, however, we accept the doctrine, the true doctrine, that phthisis requires for its production not only predisposition, but also the action of an external agent, probably organic, then a reference to its world-wide distribution on the one hand, and, on the other, to the complete immunity from it which some races, so long as they follow certain modes of life, enjoy, even though like the North American Indian, in Dr. Rush's time, they are in contact with phthisical communities—such a reference will meet the supposition that the impaired vigour of the savage has subjected him to external agents, always present although hitherto impotent, at least as probable as that of an "imported germ."

In the case of the South Sea Islands, the coming of the white man was also followed before long by a great change of diet, by the introduction not only of stronger alcoholic drinks, but also by that of pork in place of their former uncertain supply of flesh-food.

The introduction of European clothing also is attended by grave dangers to health, especially those arising from the intermittent way in which the natives use it, not only at first, but even when civilisation has made considerable progress among them. A ludicrous instance of this fell under the notice of a near relation of my own, who settled in New Zealand thirty years ago. Soon after landing, he went up the country in search of a suitable district for a sheep-run, taking with him a native as guide and porter. This man, very lightly clad, carried with him a large parcel, of which he took the utmost care, but for some days was never seen to open it. At last Sunday came round, and then he appeared dressed in the contents of his parcel, viz., a full dress suit of black cloth, with white shirt and necktie. On the Monday he resumed his scanty rags.

It is admitted on all sides that civilisation at first brings to bear upon the savages some of the most powerful predisposing causes of phthisis, causes which many of us would have said, a few years ago, are sufficient to produce it in certain constitutions. Surely, then, it is a begging of the question to say that the facts cannot be explained, except on the "imported germ" theory. The same statement might be made with equal truth of gout, or of ague.

I must apologise for having dwelt at perhaps too great length on this historico-geographical argument, but it forms so important a part of the contagionist statement, it has been regarded as so decisive of the whole question, it made so deep an impression upon myself until in self-defence I was driven to examine the grounds on which it rested, that it seemed necessary to begin by dealing with it. It is also a most insidious argument; one which lies outside ordinary medical studies, and is often stated confidently, and accepted thoughtlessly. If the history of the earliest appearance of phthisis among savages can only be explained on the theory of contagion, there is an end of the matter. All that remains is to endeavour to discover the nature of the contagion, and the mode in which it gains access to its victims, whether in New Zealand or in London. But I trust I have shown good reason for believing that this is not the case; that the facts have come to us in so imperfect and distorted a form as to be worse than useless; for, while they can be made to harmonise readily with either the diathetic or the contagion theory, they are absolutely worthless for the proof of either.

I shall now review the statements which have been made, with more or less truth, as to the influence of certain conditions on the prevalence of phthisis. It is impossible to give in detail the enormous number of facts by which these statements have been supported; but it will be my endeavour, by fairly chosen illustrations, to indicate the kind and value of the evidence on which they rest, and their bearing on the doctrine of contagion.

The geographical distribution of phthisis, the degree in which it depends upon climate, as determined by latitude or by isothermal lines, admits only of the most general treatment. It exists in all climates—tropical, temperate, and arctic—and apparent exceptions to this serve only to bring into strong relief the much greater importance of local than of general influences. According to the common statement, it is more prevalent in the tropics, and, as far as the absolute number of cases is concerned, this is no doubt true, for the population is many times more dense there than in the north. Whether it is relatively more frequent is a very different question, the final answer to which will require vast statistics yet to be compiled, the labour and the record of many generations. But there are facts which prove that arctic rigours do not prevent consumption.

Moose, or Moosonee, a trading station on the north shore of Hudson's Bay, is in the latitude of London, but enjoys a typical arctic climate, viz., a short hot summer, and a seven or eight months' winter, with the thermometer frequently as low as -40° Fahr. Mr. Walter Haydon, just returned from five and a half years' service there, has favoured me with the following table of causes of death from 1811 to June 1882. It illustrates both the scanty population of the north, and the extreme difficulty of getting facts even under apparently favourable circumstances; for the register, though an efficient one, is, he says, imperfect and badly kept. Of 162 entries of the cause of death, an average of a little over 2.4 a year,

68 were due to consumption.

22	"	"	old age.
16	"	"	cold and starvation.
14	"	"	whooping-cough.
7	"	"	bronchitis.
6	"	"	teething.
15	"	"	paralysis, epileptic fits, peritonitis—5 each.
3	"	"	gastric fever.
2	"	"	influenza.
6	"	"	apoplexy, hydrocephalus, hemiplegia, strangulated hernia, croup, heart-disease—1 each.

3 were infants under two days old.

It is interesting to observe the presence of some members of the class of "specific febrile diseases" dependent upon morbid poisons, and the absence of others, e.g., small-pox, typhus, and scarlet fever. Mr. Haydon further told me that, during his stay there, he used nine different consignments of vaccine-lymph, all without success. This he was disposed to attribute to their having all reached him during the winter, and thus been exposed to intense cold for some weeks.

Of the cases of phthisis which he himself observed, all occurred among the native Indians or half-breeds, and their symptoms were the same as among ourselves—cough, hæmoptysis, night-sweats, diarrhoea, in chronic cases "clubbed nails." The duration varied also from a few months to years. Syphilis and gonorrhoea are unknown among the natives. The greatest possible care is taken to prevent their introduction; and as the ship which brings supplies visits the station only once a year, or, not unfrequently, in consequence of the ice, only once in two years, this care is effectual. The lung-disease, therefore, which causes such large mortality among the natives, is not syphilitic phthisis.

Here, then, is an Arctic climate where the percentage of phthisis in the general death-rate is enormous, and where it seems almost exclusively among the natives. On the other hand, there are tropical countries where phthisis is all but unknown among the natives. This is the case in Senegambia and on the Gold Coast within 8 degrees of the Equator. A fellow-student of my own, Dr. Thomas Jones of Mansfield, spent five years on the Gold Coast. He tells me that among 4,000 cases of sickness annually, he never saw a single case of phthisis in a native. It is, however, of frequent occurrence among the West Indian soldiers stationed there, and their families; although in their own country they are said to possess an almost complete immunity from it. I wish to draw your special attention to this and similar exceptions to the general statements which have been made, as to the conditions which favour or arrest the spread of phthisis; for, I believe that these exceptions furnish a most useful clue to the nature of the disease.

In 1879, the Director of the Medical Department of the Norwegian Government, Dr. L. Daht, published a pamphlet on the distribution of phthisis in Norway, in which he gives statistical proof that it is most destructive in the extreme south and in the extreme north, regions separated by more than 10 degrees of latitude, but which, with many differences, agree in one respect, viz., that their climate is much more severe than that of the parts lying between them. There is, however, a very striking exception in the district of Røros, one of the highest and coldest in the country, and also one of the most phthisical, whilst the town of Røros in its centre is entirely free. But this most curious anomaly will come more conveniently under discussion in connection with the effect of altitude above the sea-level.

On the whole, the facts, so far as they are known, which bear upon the relation between climate and phthisis, do not justify any more distinct conclusion than this, viz.: that, as a matter of fact, phthisis may and does exist in any climate, and that the influence of climate, whatever it may be, so far as it depends upon latitude, is but slight, and constantly modified or overridden by other more potent conditions. Further, it is probable that phthisis is, relatively as well as absolutely, more common in the tropics than in the extreme north; but this cannot be looked upon as having been as yet certainly established.

About the influence of elevation above the sea-level in diminishing

the phthisis death-rate, no reasonable doubt exists; the higher we ascend, the greater is the immunity from phthisis, until at last it becomes almost if not quite complete. But the conditions on which this result depends are by no means equally clear. It cannot be the mere effect of the climate proper to the elevation upon non-microscopical plants or animal life; for, in the north, phthisis prevails in regions clothed with a flora, among which, on the Alps or Andes, it it would be practically unknown.

It is difficult to believe that it can be due to the difference in the level of subsoil-water at varying heights on a steep mountain-side, when we see that not only slopes, but high mountain valleys, which often contain lakes, are equally exempt.

Is it due to diminution of atmospheric pressure? or to freedom of the air from impurities common at lower levels? My own belief is, that this last is the real explanation; and that, as Professor Tyndall's flasks remained clear and unaltered, when freely exposed to the air at the Bel Alp in the absence of the bacillus of putrefaction, so, too, at certain heights, the micro-organism, whatever it may be, which excites phthisis, is no longer met with.

In Norway, phthisis, as already stated, is most prevalent when the winter is most severe, and to this rule the district of Røros as a whole forms no exception. But the town of Røros escapes, although it is probably the coldest inhabited place in the country, from its situation on a dreary and inclement plateau, where winter prevails for fully eight months in the year. From personal inquiry which I had the opportunity of making last summer, I have no doubt of the reality of this immunity as far as the town itself is concerned, and it is generally attributed to its elevation, for it is the highest as well as the coldest town in the country. But, on a closer scrutiny of the facts of the case, this explanation seems to me untenable. Røros lies in an elevated district, on the watershed between the river Glommen and the streams flowing to the north and east, but not by any means on its highest point. The whole district is an elevated one, there are in it no deep valleys but little above the sea-level; and it is difficult—on the direct contagion-theory impossible—to believe that a slight difference in elevation—sometimes in one, sometimes in the other direction—should preserve the town from phthisis in the midst of a deeply infected neighbourhood, especially when it is remembered that the deep, narrow, level valleys at the head of the west coast fjords—*e. g.*, the Hardanger and Sogne fjords escape, by comparison, almost entirely. It may, however, be that in the industry of the mining town of Røros *i. e.*, copper-smelting, is to be found the cause of this absence of phthisis. At any rate, in this connection, it is interesting to note that in the West Indies, in the sickly island of St. Lucia, the sulphureous localities are the only salubrious localities; that, immediately round sulphur-works and factories for making gunpowder and sulphuric acid, the vegetation and the ague disappear together, and that in Cornwall ague is said to have been almost unknown in the neighbourhood of copper-smelting works. However this may be, the district of Røros supplies a twofold exception to general statements about phthisis: 1, the district, as a whole, although elevated, is extremely phthisical; and 2, notwithstanding this infection, a portion of it is almost entirely free.

Another exception is to be found in the prevalence of phthisis in Spain on the high plateaux of Castile and Estremadura (*Journal de la Soc. de Statistique de Paris*, October 1867; *Repartition Geographique de la Phthisie Pulmonaire*, par M. le Docteur Gross of Berne.)

But, whilst in the case of climate it is difficult to say what is the rule and what the exception, there is no doubt that, as regards altitude, the prevalence of phthisis at considerable heights, although instances of it do exist, is exceptional. We shall have to consider again the effects of these exceptions on the doctrine of phthisis; but it would be a waste of time to dwell longer now on the general law of altitude, or to parade a long list of writers whose labours have established it.

It must, however, be borne in mind that a residence at a high altitude not only prevents phthisis, but also cures many persons suffering from it.

Dampness of Soil.—In the ninth report of the Medical Officer of the Privy Council for the year 1866, appeared Dr. Buchanan's classical report "On the Results which have hitherto been general in various parts of England and Wales, by Works and Regulations Designed to Promote the Public Health." In this he establishes this fact, at that time an unexpected one, that the drying of the soil effected by sewerage works is generally followed by a great reduction in the mortality from phthisis. In the following year Dr. Buchanan submitted another report, "On the Distribution of Phthisis as affected by Dampness of Soil." The conclusions arrived at here are so important to my argument, that I may be permitted to remind you of them. They are as follows.

Reduction in Phthisis-Mortality.

Salisbury	— 49 per cent.	.....	Much drying.
Ely	— 47 "	.....	"
Rugby	— 43 "	.....	Some drying.
Banbury	— 41 "	.....	Much drying.
Worthing	— 36 "	.....	Some drying.
Macclesfield	— 31 "	.....	Much drying.
Leicester	— 32 "	.....	Drying (see text).
Newport	— 32 "	.....	Local drying.
Cheltenham	— 26 "	.....	Some drying.
Bristol	— 22 "	.....	"
Dover	— 20 "	.....	Local drying.
Warwick	— 19 "	.....	Some drying.
Croydon	— 17 ¹ "	.....	Much drying.
Cardiff	— 17 "	.....	"
Merthyr	— 11 "	.....	Some recent drying.
Stratford	— 1 "	.....	Some local drying.
Penzance	— 6 "	.....	No change.
Brynmaur	+ 5 "	.....	No notable change.
Morpeth	+ 8 "	.....	No change.
Chelmsford	+ 0 "	.....	Slight drying.
Penrith	+ 5 "	.....	No change.
Ashby	+ 19 "	.....	Some drying.
Carlisle	+ 10 "	.....	Drying, with local defects.
Alnwick	+ 20 "	.....	No drying.

¹ This includes phthisis and lung-disease.

(Tenth report of the medical officer of the Privy Council, 1867, p. 109.)

The following general conclusions, then, result from the present inquiry.

1. Within the counties of Surrey, Kent, and Sussex, there is, broadly speaking, less phthisis among populations living on pervious soils than among populations living on impervious soils.

2. Within the same counties, there is less phthisis among populations living on high-lying, pervious soils, than among populations living on low-lying, pervious, soils.

3. Within the same counties, there is less phthisis among populations living on sloping impervious soils, than among populations living on flat impervious soils.

4. The connection between soils and phthisis has been established in this inquiry; *a*, by the existence of general agreement in phthisis mortality between districts that have common geological and topographical features, of a nature to affect the water-holding quality of the soil; *b*, by the existence of general disagreement between districts that are differently circumstanced in regard of such features; and *c*, by the discovery of pretty regular concomitancy in the fluctuation of the two conditions, from much phthisis with much wetness of soil to little phthisis with little wetness of soil. But the connection between wet soil and phthisis came out last year in another way, which must here be recalled; *d*, by the observation that phthisis had been greatly reduced in towns where the water of the soil had been artificially removed, and that it had not been reduced in other towns where the soil had not been dried.

5. The whole of the foregoing conclusions combine into one—which may now be affirmed generally, and not only of particular districts—that wetness of soil is a cause of phthisis to the population living upon it.

6. No other circumstance can be detected, after careful consideration of the materials accumulated during this year, that coincides on any large scale with the greater or less prevalence of phthisis, except the one condition of soil.

7. In this year's inquiry, and in last year's also, single apparent exceptions to the general law have been detected. They are probably not altogether errors of fact in observation, but are indications of some other law in the background that we are not yet able to announce.

Some years earlier, in 1862, Dr. Henry J. Bowditch delivered, at the annual meeting of the Massachusetts Medical Society, an address "On Consumption in New England, and elsewhere, or Soil-moisture one of its chief Causes." In this, he anticipates Dr. Buchanan's conclusions, and also makes certain statements as to the very narrow area within which a most intense phthisis-infection may be confined, which have a most important bearing on the question of direct contagion. In the preface to the published essay, he says: "The force of facts presented to me by correspondents compels me to resign the commonly received opinion that consumption is equally diffused throughout New England, and to adopt the new one of the address, *viz.*, that there are some spots which have very little of that scourge of the human race, while in other places, and even in particular houses, it prevails to a frightful degree; and, moreover, that these spots may be, perhaps, within a very short distance from each other. Again, facts, as I believe, indicate that dryness of the soil in the surroundings of any places is the prominent characteristic of the former, or of places comparatively free from consumption; while dampness of the soil charac-

terises the latter; or, as they may be aptly called, consumption-breeding districts."

In reviewing the evidence which has been adduced in proof of direct contagion, Dr. Bowditch's essay will be again referred to. At present, I wish to point out that the influence of dampness of the soil in increasing phthisis-mortality, although it rests on unusually trustworthy facts, is yet not an universal law. Dr. Buchanan's list of towns in which more or less drying of the soil had taken place, supplies, as he himself acknowledges, exceptions to his general conclusions. The most striking is that of Ashby, where, with "some drying," an increase of 19 per cent. in the deaths from phthisis took place. It is true that the numbers in this and in other exceptions which I shall mention are small, and, if included in large tables, could make only a fractional difference in the percentages. But statisticians are apt to overrate the power of figures. A single real exception is sufficient to invalidate the claims of any supposed universal law; and the study of these exceptions, numerically so insignificant, is absolutely necessary if we would reach a final generalisation. The present registration-districts are so large, and many of them include localities differing so widely from each other in important particulars, that it is impossible to investigate satisfactorily the true causes of diseases by the aid of figures which do not deal separately with the subdistricts. In 1866, Mr. G. A. Rowell, of Oxford, published a pamphlet on the *Effects of Elevation and Floods on Health*. He was a man of deservedly high local reputation, spent immense labour on his task; and his facts, remarkable as many of them are, are probably trustworthy. Unfortunately, their verification is extremely difficult; for he compares single parishes and small districts which it is impossible to pick out of the Registrar-General's tables. Here is his account of two villages a few miles from Oxford, up the Thames valley, given with some verbal alterations and omissions of unimportant details (*op. cit.*, p. 24). "Cassington and Wolvercot are both situate on gravel; both have excellent water for drinking purposes; and both in the same poor-law union, and within two or three miles of each other. Cassington is not situate on a hill, but upon a bed of gravel which projects into the valley towards the river, but is high and dry above it. The village is fully exposed to the currents of air which sweep through the valley from west to east, having but little protection from trees or other shelter. The roads through the village are wide and well kept; the cottages appear to be much better than in most villages; are well distributed; and, although I saw it in wet weather, during a flood-season, I thought it one of the cleanest villages in the neighbourhood. Wolvercot presents appearances as reverse as possible to those of Cassington. It is protected from the east wind, and Lower Wolvercot is well sheltered by trees on the north and north-east. Along the bottom of the rising ground in Upper Wolvercot there is a ditch of stagnant water; and the ground between the lower houses and the canal is liable to flood in winter, presenting at all times the nearest approach to a swamp that I know of in the Oxford valley. In Lower Wolvercot, some of the houses are at times flooded, and all of them are but little above high flood-level. There are stagnant ditches around the village, and stagnant pools within it, except in dry summer-time. Even the road through the village, till very recently, was generally impassable for carts in flood-seasons, and partially flooded at all times. The cottages are certainly not better than they are at Cassington, while they are much more crowded together. At Wolvercot, a great many ducks and geese are bred; and the early broods, in cold spring seasons, are often kept within doors in the cottages. Thus, in accordance with general opinion, the condition of Wolvercot is much less favourable to health than that of Cassington; but statistics for the twenty years from 1841 to 1860 show that in Cassington the death-rate was 27 per cent. higher than in Wolvercot, and the average of life 25 per cent. in favour of the latter village also. The actual figures given by Mr. Rowell are as follows.

Cassington	Annual deaths.....	1 in 40.2 persons.
	Average age.....	25 years 10 months.
Wolvercot	Annual deaths.....	1 in 51.3 persons.
	Average age.....	31 years 7 months.

Allowing for the difference in the population of the two villages, more than double the deaths from consumption occurred in Cassington than there were in Wolvercot. Classing that disease with all others of the respiratory organs, the deaths were 70 per cent. in excess in Cassington."

Whatever may be the relation between dampness of the soil and phthisis, it is clearly one which may be overpowered or masked by other forces.

Of the three conditions so far considered, two—viz., altitude and soil-dampness—have, undoubtedly, great influence; the first in preventing, the second in developing phthisis; and, in both, the facts in-

dicate that there is a something, in large measure independent alike of the constitution and social habits of the population (subject to it, but without which phthisis cannot exist). It is certainly improbable that altitude acts by producing, sooner or later, a race of men which is proof, or all but proof, against certain morbid conditions. For, on the one hand, natives of the hills readily contract phthisis in the lowlands; and, on the other hand, the lowlanders recover when removed to the hills. The antiphthisical constitution, if it be in any way due to altitude, must be a very temporary affair, easily acquired and easily lost; and yet there is no *a priori* improbability against the formation of such an antiphthisical constitution, or against its acquiring a permanent character. Indeed, we know that there are races which remain comparatively unharmed by phthisis in the midst of deeply infected localities, e.g., the Chinese in Melbourne.

Since, then, at a certain height above the sea, phthisis ceases to occur, and affected cases recover without affecting injuriously their friends and neighbours, whilst there is no reason to suppose that these latter possess any special protection against phthisis, other than what may be temporarily due to residence at a certain altitude, it is surely reasonable to suggest that the cause of this immunity is to be found in the supposition that some external agent, essential for the development of phthisis, is here inert or absent. In the case of soil-dampness, the probability of the existence of some such agent is even greater. It is difficult to understand in what way the drying of the soil, by the execution of sewerage-works, could have so profoundly and so rapidly altered the constitution, occupation, habits, and vitality of the people of Salisbury for example, and of other towns, as at once to reduce, by a very large percentage, the number of cases of a developmental disease, if phthisis be recognised as developmental, or of a directly contagious disease, if it be recognised as directly contagious. Surely here, too, as in the case of altitude, a reasonable, perhaps the best, explanation of the facts is that phthisis is produced by some external agent, but yet, not spread in the ordinary course of things by direct contagion. The evidence from the effect of climate pointing in the same direction is less strong, for the affirmative facts are less certain and less striking. The exception, however, under all three heads, climate, altitude, and soil-dampness, permits even a stronger proof than can be drawn from the general laws to which they are related. It is, I believe, all but impossible to explain these exceptions on either the developmental theory or on that of direct contagion; but this will be treated of in greater detail in the final summing up of the conclusion to be drawn from the nature of the so-called causes of phthisis. At present, I would only add that these three conditions are, in themselves, entirely independent of any human agency; that, so far as man's action modifies the unwholesome influence of any one of them, it will be to minimise it by the use of suitable clothing, food, fire, and shelter, and that therefore, their real power, as gauged by statistics, is probably under rather than over stated.

EXTREME LOWERING OF TEMPERATURE IN HÆMORRHAGE INTO THE MEDULLA OBLONGATA.—M. Lemcke has reported, in the *Deutsch. Arch. für Klin. Med.* for 1883, p. 84, the case of a man, aged 38, who had been for many years given to drink, and who was suddenly seized with shivers and delirium. When admitted into the Rostock Hospital, his pulse was 38, respiration 18, and rectal temperature 23° C. (73.4° F.) after a warm bath. On the following days, it reached 26° and 28° C.; but the patient died without recovering consciousness. At the *post mortem* examination, the signs of a recent hæmorrhage were found in the left half of the medulla oblongata, above and on the outer side of the origin of the vagus. M. Lemcke thinks that the extreme lowering of the temperature cannot be wholly explained by the action of alcohol, but that the thermic centres had been injured by the hæmorrhage.

TO PREPARE CORROSIVE SUBLIMATE GAUZE.—In the New York Hospital, corrosive sublimate is used almost exclusively as an antiseptic upon gauze or jute. The sublimated gauze is prepared by immersing the bleached material in a solution as follows: corrosive sublimate, 20 parts; water, 4,480 parts; glycerine, 500 parts, for twelve hours; then wringing out, and allowing to dry, as far as the glycerine will permit. At the time of operation, a sublimate solution (1 in 1000) is allowed to trickle slowly but nearly continuously over the incision. Bleeding vessels are tied with sublimated catgut. No impervious protective is used over the dressings, as, by retaining the moisture of the dressings and the sweat, it is thought to act too much as a poultice. Metallic instruments must be immersed in a 5 per cent. carbolic solution, as the bichloride will form an amalgam with them. In the New York Hospital, not only is the part to be operated upon washed with soap and water, but also with turpentine and alcohol, two ounces to the pint.