turbid by fine granules. The impurity was removed by solution and recrystallisation. A simple test of the purity of picric acid for chemical purposes is to boil a mixture of one volume of the saturated solution with half its volume of liquor potasse. The resulting liquid should be quite pellucid, and of a pale lemon yellow colour, with no red tinge.

BRITISH MEDICAL ASSOCIATION.

FIFTY-SECOND ANNUAL MEETING.

PROCEEDINGS OF SECTIONS.

ON THE CAUSATIVE RELATIONS OF PHTHISIS.

Introduction to a Discussion in the Section of Medicine.

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I ACCEDED to the flattering invitation to introduce the subject of the etiology of phthisis for discussion to-day, because I thought the present time and occasion peculiarly fitting; for in the light of recent discoveries and active speculation with regard to the real nature of phthisis, all previous observations as to its etiological relations will have to be reconsidered. I venture to think, too, sir, that this is the right place for such a discussion, for the Father of Medicine in this city, Dr. Macnamara, has done much in his time to quicken thought on this matter; and the outcome of his teaching, in the shape of prophylactic measures, would go far to satisfy the requirements suggested by the most modern discoveries for the prevention of this scourge of our race.

The great objects I shall have in view in shaping my own remarks will be to elicit the points calling for further elucidation, and to see how far clinical observation and experimental research are in accord, and whether in any points they clash in such a manner as cannot be met by any mere readjustment in terminology.

Phthisis may be described as synonymous with consumption, namely, a progressive consolidation and decay of the lungs with progressive wasting of the body. And it will be convenient and generally correct to consider the other lesions of phthisis as for the most part of secondary tubercular origin.

The progress of consumption is remittent, or even intermittent, there being with each period of activity fresh destruction of lung, extension of local signs, renewal or exacerbation of local symptoms, and the phenomena of hectic.

What is the lesion of phthisis? Hippocrates seems to have regarded it as a suppuration and ulceration of the lung; and in a sense it has ever since been so regarded, whilst "inflammation," "deposit," "growth," have for centuries been the standards round which have most densely gathered the disputants in the controversial field of tubercular pathology.

Finally, we seem to have come to this pass, that all the lesions of phthisis are stated to be tubercular, but that we have no anatomical criterion of tubercle. The inoculation-experiments of Villemin indicated a new test for tubercle—its power of reproducing its likeness when inoculated into healthy animals; and the further pursuance of experimental inquiry has brought to light a still readier test of tubercle in the wide sense of that term, as including the whole morbid anatomy of phthisis, namely, the bacillus of Koch, a vegetable parasite, by the presence of which these lesions are determined, and to the necessities of which they minister. Hence one's definition of phthisis might now run, that it is a progressive consolidation and decay of the lungs, and progressive wasting of the body due to the presence, growth, and diffusion of a particular micro-organism. And if we accept this definition, the causes of phthisis must be spoken of as those varied conditions favourable to the preservation of this organism, and to its entrance into the human body.

Now, the causes of phthisis may be grouped under the headings climatic, social, industrial, and infective. It is manifestly impossible to enter with any minute detail upon the several reputed causes of phthisis; nor is it necessary, for they have quite recently been very fully considered by Dr. Andrew, in his Lumleian Lectures. I shall attempt little more than to refer to those most interesting to us at the present day.

Much careful search up and down the world has shown that no climate is exempt from phthisis, but that towards the same levels in

altitude and latitude it becomes of rare occurrence, and ceases to be endemic. It most abounds in subtropical and temperate climates, and within the lower ranges of elevation. The only positive climatic influence made out in relation to phthisis (and this only distinctly so for temperate latitudes) is that dependent upon insufficient surface-drainage. "Towns, villages, hamlets, and houses situated at or near undrained localities, on heavy impermeable soils, or on low-lying ground, and whose sites are consequently kept damp, have a much larger number and proportion of cases of consumption than those which are situated on dry or rocky ground, or on light porous soil, where redundant moisture can easily escape." Such was the observation of Gavin Milroy (*Seventh Annual Report of the Registrar-General for Scotland*) in Scotland.

Buchanan, in 1867, established the relationship with regard to our home counties between phthisis mortality and wetness of soil, and his observations showed that the mortality in certain localities—for example, Worthing and Salisbury—had considerably diminished since drainage had been effected.¹ The researches of Bowditch, in America, in 1862, pointed to the same conclusion. "Phthisis is invariably present in low and damp countries," says Lancereaux (*Distribution Géographique de la Phthise Pulmonaire*, 1877). On the other hand, the climatic influences unfavourable to the development of phthisis are associated with dryness of soil and rarefaction of the atmosphere. Elevation above the sea-level is regarded by many observers as of the utmost importance in the prevention of phthisis. M. Jourdanet maintains that the disease almost ceases to appear at the half-distance between the sea-level and the snow-line for any given latitude (quoted by Lancereaux, p. 30). It is, again, well pointed out by Walshe that "there exists in each of the earth's zones spots enjoying a more or less complete immunity from phthisis, the exemption of which is inexplicable on any acknowledged meteorological principle" (*Dis. of Lungs*, p. 466). And again: "The real potential conditions of climate, both antagonistic and attractive, with the exception to a certain extent of barometric pressure, remain unrevealed" (p. 467).

Those who, with Dr. Andrew (Lecture iii, *BRITISH MEDICAL JOURNAL*, April 19th, 1884, pp. 751, 755), find a resemblance between phthisis and ague, endeavour to explain the prevalence of the former disease in certain low-lying, ill-drained situations, by the existence there of conditions favourable to the well being of the tubercle-bacillus. As I shall point out presently, however, the bacillus of tubercle can be said to flourish, that is, to grow and multiply, under no conditions of Nature, however favourable, outside the animal body; and, on the other hand, its tenacity of life is so great as to place it apparently beyond the reach of adverse local influences. With regard to high altitudes, we do not yet know how far the scarcity of phthisis is attributable to sparsity of population; how far to climatic influences proper; how far to conditions unfavourable to the activity, or even presence of the bacillus. The answer to these questions may, however, soon be found; for the experiment of massing the population in these regions is being made; the germs are there in increasing abundance; and, whether they will flourish in such localities can be easily proved by inoculation and by culture-experiments there conducted. Cases of active phthisis continue to be active in elevated regions; and it is emphatically stated by Walshe that "tubercular deposition is by no means very uncommon in certain other organs of natives of the highest habitable regions" (p. 465). Amongst these and other more disputed conditions in connection with climate, it cannot escape observation that phthisis is nourished under those influences favourable to the prevalence of catarrhal and inflammatory affections of the air-passages—more especially bronchitis and pleurisy—that is, on cold and damp soils, with variable temperature; and, as we know, at those seasons of the year, the spring and autumn, when these conditions prevail. In high situations, especially in dry situations, catarrhs are much less prevalent, respiratory conditions are more robust and active, and the processes of nutrition go on more vigorously in those to whom such climates are favourable.

An examination of both climatic and industrial conditions predisposing to phthisis, cannot fail to convince any one how largely they are mixed up with conditions that fall under the heading of social relations. Phthisis is essentially a scourge of what we call civilisation.

¹ It has been claimed for the late Mr. Middleton of Salisbury, that he first drew attention to the effect of subsoil drainage in diminishing phthisis-mortality, in his paper read before the British Association at Bath in 1864. Mr. Middleton appears, however, to have regarded the prevalence of phthisis as largely due to "humidity and nastiness of atmosphere produced by the canals and cesspools," and its diminished prevalence to improved drainage and sewerage works. Buchanan pointed out that the lessened death-rate was attributable to the diminished wetness of subsoil—apart from improved sewerage-works—effected by drainage, and to have fairly established this doctrine. (See correspondence in *Lancet*, 1869, and *Salisbury Times*, June 10th, 1884.)

Its rarity amongst nomadic tribes and savage populations in all climates; its prevalence in littoral districts; in river sites, and in industrial as compared with agricultural localities; in townships and cities—all point to social rather than climatic influences, as predominant in the cultivation of the disease. All the depressing conditions of life—*anxiety, mental strain, disappointments, bad sanitation, overcrowding, debauchery*—are concentrated at the centres of civilisation. The weakly are helped to live; rickets, scrofula, syphilis, catarrhs prevail; the general tone of health is depressed; recovery from acute specific disease—*measles, whooping-cough, and from inflammatory chest-diseases*—is less complete; germs, putrefactive and other, are so rife, that special precautions against them are necessary to secure the healing of wounds. All that we know about phthisis would lead us to expect its prevalence under such conditions. This unhappy concomitance of phthisis with those states of life most favourable to the culture of art, science, and the graces of humanity will, I am persuaded, not always continue. I have much hope that the new science of sanitation, that has so tardily developed into dimensions of now rapidly increasing scope and importance, under the wise guidance of Farr, Simon, and Buchanan, will gradually cleanse our social atmosphere; compel more elbow-room in social life; investigate and supervise the conditions of labour, and especially of female handiwork; seeking to combine the interests of humanity with those of trade; and that phthisis, amongst many other diseases, will lessen.

The effect of certain industries, and especially of those attended with the inhalation of dust, in causing phthisis amongst other chest-diseases, has long been well known, and was most exhaustively illustrated by Dr. Greenhow in Public Health Reports for 1860 and 1861. Mining, flax-industries, pottery work, tool grinding, lace-work, etc., were all examined into, and, all allowances being made for social causes above alluded to, that the inhalation of dust remained as the positive cause of the undue prevalence of phthisis in the industrial localities, was shown by the fact "that the high death-rate from lung-disease belonged, according to the occupation, to men or to women of the district; that it sometimes was nearly twice as high for the employed sex as for the unemployed sex; and that it only extended to both sexes when both were engaged in the occupation." (Simon, Report, page 34, 1860, 1861.)

There are others to follow me in this discussion much better informed than I am, and with much larger experience of this particular form of phthisis: I will only refer to two or three points which have struck me as noticeable in cases of phthisis attributable to dusty employment, namely:

1. That many such cases are not distinguished by any special characters from ordinary subacute double phthisis.
2. In other cases, the disease is typically one-sided, densely fibroid, with degeneration, and very chronic course.
3. That all special features become lost in the general havoc of the later stages of the disease.
4. If the parent of the patient has followed the same occupation, he has apparently suffered, not from phthisis, but rather from bronchitic or asthmatic chest-troubles which have disabled him much later in life.

It would seem then that, in some cases, mechanical irritation calls forth the disease to which the person is otherwise liable. Again, the remarkable one-sidedness of the disease during its long characteristic periods is worthy of note, beside the fact that the bronchitic phenomena of dust inhalations are strictly bilateral. This one-sidedness is strictly in accord with the type of the more chronic forms of phthisis which one used to regard as of catarrhal origin. We want more information as to the relative frequency of these several lesions called forth by exposure to a dusty atmosphere. What lies behind the dust in the causation of a given form of lesion, and at what stage does it cease to be of mechanical origin, and become specific?

Underlying all the external causes of pulmonary phthisis, but most strikingly illustrated in mechanically induced cases, is the, I think, too often forgotten fact of the complex structure of the lungs themselves, which renders them so peculiarly receptive of poisonous and irritant matters; so vulnerable by reason of that extreme delicacy of texture and vascularity, with regard to both blood-vessels and lymphatics, essential for function; so retentive of disease products, whether auto-genetic or of foreign source; and lastly, their ceaseless movement, and the ever changing samples of the surrounding air, be it foul or fresh, dust-laden or germ-laden, with which they are permeated.

In view of these facts connected with structure, it is further important to remember how readily, on the one hand, exudative lesions are absorbed, whether from the lungs or elsewhere, in contrast with the comparative difficulty with which corpuscular and epithelial proliferations are removed. These later proliferations occur mostly on sur-

faces, and can with readiness be removed by mechanical effort. We snort away the products of nasal catarrh; we cough out those of bronchial catarrh; but, as we approach the bronchioles, we find greater and greater difficulty in getting behind the products; and, on the infolded loculated surface of the lung, such products tend to lodge, to inspissate and decay, and to become the nidus for putrefactive or other germs brought to them by the ceaseless bellows action of respiration. If we admit the occurrence of such lodgments—and can we deny their frequent presence—are we to regard them as constituting prodromal or incipient phthisis, or shall we allow them no nominal relationship to the disease of which they at least, in the majority of instances, determine the commencement and seat?

The now well known observation of Buhl (1857), that an outbreak of tuberculosis was almost always attributable to the previous existence of caseous matter somewhere in the body, foreshadowed the experimental proof by Villemin (1865) that such matters, when inoculated into healthy animals, produced tuberculosis; and contained, therefore, a specific poison. Similarly, Villemin's observations paved the way for the still more refined discovery of Koch (1882) of the specific agent, the bacillus tuberculosis. I need not allude now to the experiments, confirmatory and otherwise, called forth by the important discovery of Villemin. His conclusions were, after due questioning, accepted; but it was found that other substances than tubercle, introduced in the same way, led to tuberculosis; and the only one of the several able experimenters who has not since admitted that his experiments productive of tuberculosis, with indifferent substances, were fallacious, is Dr. Burdon Sanderson, who, at the time, however, included this important reservation in summing up his results; namely: "As regards the question of a *specific contagium* of tubercle, we think it very important to note that this is not as yet disproved by the facts of traumatic tuberculosis. It still remains open to inquiry whether or not injuries which are of such a character that air is completely excluded from contact with the injured part, are capable of originating a tuberculous process."² Dr. Burdon Sanderson had found that a certain proportion of animals (guinea-pigs) inoculated with pyæmic matter, and which survived the direct—that is, pyæmic—effects of inoculation, if examined several months later, presented morbid appearances in lungs, liver, and spleen, in which all the characters of a tuberculous process were to be recognised; and further, that, in one out of two guinea-pigs in which sections of cotton had been introduced by means of a clean needle, death resulted within three months, the lesions consisting of local abscesses discharging by sinuses, and of lung, liver, and spleen tuberculosis.

Whilst one must accept the conclusions flowing from the convincing experiments recently made by Koch, Cohnheim, Salomonsen, and others abroad, and by Watson Cheyne and Dawson Williams in this country—namely, that, with due precautions against contamination, only tuberculous matter is, when inoculated, productive of tuberculosis—it is still difficult to throw over altogether the experiments of Wilson Fox and Sanderson. Their results were valid under the conditions laid down by Villemin, and from them we learned that local inflammatory (irritative) lesions, continued for a certain time, are apt to lead to local, and ultimately to general, tuberculosis. How the specific poison of tubercle gained access, when it did so, and whether from within or from without, it would be difficult to say; but one cannot but be reminded of the very similar conditions brought about by local lesions such as eczema, intestinal affections, etc., leading to gland-infiltrations in scrofulous subjects, and sometimes later to tuberculosis. It might be expected that, under the purest health-conditions, indifferent or protected substances would yield negative results when inoculated. Carbolicised sutures, sponge-grafting in pleuritic cavities, are clinical instances in point.

Koch has recently restated the more fully developed results of memorable researches. His conclusion is that, "In all the morbid processes which, on account of their course as well as of the characteristic microscopical structure and the infectious properties of their products, must be considered true tuberculosis, there always occur in the tuberculous centres rod-like formations (tubercle-bacilli) which can be demonstrated with the aid of particular methods of colouring; this holds true both with the tuberculosis of man and of different species of animals."³ Koch affirms these bacilli to be the specific cause of tubercle, for, when separated from their native soil, and cultivated on neutral matter through many generations, their power of setting up tuberculosis when inoculated is unimpaired. The tubercle-bacillus is a minute immobile rod-shaped vegetable spore-containing fungus,

² *Anatomical and Experimental Investigations*, etc., 1868-69, republished in the *Practitioner*, December 1882, p. 425.

³ *Die Etiologie der Tuberculose*—Dr. Struck's *Mittheilungen aus dem Kaiserlichen Gesundheitsamte*, p. 45.

only capable of growth and multiplication under culture in blood-serum or animal broth, at a constant temperature of 30°; of comparatively slow growth, and unable to continue its development in decomposing fluids in the presence of the more rapidly growing putrefactive bacteria. All the conditions essential for the development of the bacillus are alone found naturally in the animal body. The bacillus is, however, of very tenacious vitality. It will preserve its virulence for six weeks or longer in decomposing sputum, for six months or longer in the dry state. When inoculated, whether from pure culture or in tuberculous matter, it produces tuberculosis; intimately diffused in water, whether as sputum or from pure culture, and dispersed through the atmosphere in the form of spray, animals exposed to its inhalation die of tuberculosis. Hence Koch concludes that contagion is the only means by which tuberculosis is acquired, and he affirms that "the bacilli precede in time and place all the pathological changes peculiar to tuberculosis; and that their number, appearance, and disappearance stand in direct relation to the course of the tuberculosis" (*loc. cit.* p. 46).

It is important here to observe that, since the application of experimental methods to the elucidation of the pathology of phthisis, all kinds of phthisis and tuberculosis have been lumped together as tuberculosis; as being alike the product of one virus, the tubercle-bacillus. So that with these observers, a disease is not phthisis until it is tuberculosis.

Koch points out that one-seventh of mankind die of phthisis, after for weeks or months ejecting sputa containing organisms which, although of somewhat tardy development, have great tenacity of life, and when inhaled may take root, and cause phthisis. Villemin, Budd of Clifton, Thomson of Melbourne, and others, have anticipated Koch's conclusions in so far that they insisted that phthisis was a contagious malady (a view held in ancient times, and by many in the first half of the last century).⁴ Koch points out the why and the how.

If phthisis be a zymotic disease, it must be, under certain conditions, communicable and even contagious. Are the conditions for the reception of the germ those which have been hitherto regarded as constituting incipient consumption? If so, then a readjustment of nomenclature is only needed to place in accord clinical experience and experimental observations. Or, on the other hand, is the disease, apart from experiment, communicable directly (with no intermediate lesion) as enteric fever, ague, syphilis, etc., are communicated? If this view be maintained, I do not see the way of reconciliation between clinical experience and the teachings of experimental research.

If phthisis be a disease alone produced by contagion, and afflicting one-seventh of our population, a disease lasting from six months to five or seven years or longer, during most of which time patients are distributing wholesale the seeds of their malady, how is it, under these circumstances, that any one escapes, or, at least, that the disease does not become perceptibly more rife? For, be it observed, phthisis differs from all other zymotic diseases, even from syphilis and ague, with which it has often been compared, in the fact that, once attacked, the patient is ever increasingly liable to fresh lesions, this liability not being limited to the individual, but extending beyond himself to his progeny, and with such increasing force that that progeny shall be attacked at an earlier period of life than himself. There is no such thing, then, as acclimatisation to, or "protection" from, tubercle. Moreover, in about half the cases, the disease is not hereditary; it cannot, therefore, be said that the mortality is self-regulated, that is, kept within bounds by family extinction.

Yet, in spite of these adverse conditions, the death-rate from consumption has, in the last recorded quinquennium, 1875-79, declined at every age, with some insignificant exceptions, to the extent of about 18 per cent., or a total rescue of nearly 11,000 lives.⁵ My observation would lead me to think, too, that the disease is less severe, at least in its complications, and particularly in the more rare occurrence of intractable intestinal complication.

It is encouraging to note that even Koch admits certain conditions of a pathological kind as favourable to the effective adhesion of the

micro-organism of phthisis, namely, denuded epithelium of the air-passages, especially as arising in measles; "stagnant secretions within the lung," whose occurrence is favoured by pleuritic adhesions; deformity of thorax, and the like, which impede lung-movement. These saving clauses, inadequate as they stand, and in regard to the apparently little importance attached to them, yet give promise of a better agreement between the findings of clinical observation and those of experimental inquiry.

Clinical physicians cannot be unmindful of the debt they owe to the patient research of experimental pathologists, without the aid of which we should still be groping in the dark respecting a very essential part of the nature of phthisis as of some other fell diseases. I fail, however, to find any support in clinical experience, of a view which completely subordinates constitutional and local conditions to the chance inhalation of specific germs, which declares that the bacilli "precede, in time and place," everything beyond mere denudation, stagnation, or pleuritic adhesion, and hampered breathing. We must have the soil much more specially receptive than these stated conditions imply. It may be wearisome to repeat again the statement of consumptive mortality amongst the officials of our great chest hospitals, which has been so carefully ascertained by Cotton, and brought up to a recent period, with great care, by my colleague, Dr. Theodore Williams;⁶ which has been, and still more recently, referred to, with the valuable addition of records of another hospital staff, by Dr. Andrew. Suffice it to say that, among the former and present officials—nurses, sisters, matrons, secretaries, porters, servants, physicians and their assistants, the mortality, calculated over many years, has been rather below than above the average. Taking into account the in-door lives; the average age of susceptibility to the disease; the average hereditariness; the time (six months to many years) spent more or less in near contact with phthisical persons, in all stages of the disease, gathered in a sub-tropical temperature; and, up to a recent period, the absence of any special precautions with regard to sputa, disinfection, etc., I say, take all these circumstances into consideration, we have had in our very midst an experiment of the utmost importance to the question before us, extending over many years, and under conditions more favourable for the purpose than we are likely to meet with again.

It is true, that the wards of the hospitals were well ventilated; but perhaps not better so than the admirably constructed death-traps provided by Professor Koch for his animals submitted to bacillus spray experiments. The out-patient rooms of the old building at Brompton were, however, far from being efficiently ventilated; yet I have watched cases of emphysema, of chronic bronchitis, of asthma, of chest-rickets, of cardiac disease, of anæmia with spurious hæmoptysis, and the like, for months, some of them, at intervals, for years, and they have not become phthisical. Now, these people have been, for two or three hours at a stretch, exposed to the "virulent" atmosphere of a crowded waiting-room, nine-tenths of the occupants of which have been phthisical. With a courage, perhaps begotten of ignorance, I have myself sat for three or four hours twice a week, for eight years, practically in the midst of these people; taking my luncheon in the same room, and washing down the bacilli with my coffee. I have more than once observed in the breath of a colleague, after such a long out-patient day, the very peculiar odour of cavity-stage phthisis, showing how thoroughly he must have taken in any poisonous emanations; yet he is well, and so am I! Now, I repeat, you may go to the uttermost parts of the earth, and you will not find the conditions so well arranged for testing practically the contagion-theory of phthisis, as we meet with that disease in practical life. Dr. Andrew, in his able Lumen Lectures, has carefully gone through the question of geographical and racial distribution of phthisis, with special regard to the question of its spread by contagion, and his conclusion is negative. He mentions the statement made by Dr. W. Budd, that phthisis was introduced into North America by Columbus and his European followers by contagion, but points out that Dr. Rush, upon whose work Dr. Budd's statement was founded, comments upon phthisis being unknown amongst the natives there at the time he was writing, 1789, three centuries after the appearance of Europeans; and Rush attributes the disease, even amongst the whites, to the results of advancing civilisation. Dr. Andrew also casts doubt upon the introduction of phthisis into the South Sea Islands by Europeans, although the altered conditions of life introduced with civilisation would tend to foster such a 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

⁴ Dr. Bowditch gives a brief history of this subject in a pamphlet, *Is Consumption ever Contagious?* published in Boston, 1864.

⁵ The death-rate has declined at every age, with some insignificant exceptions, in both sexes; the decline being for males 14, for females 22, per cent. For both sexes, the decline was greatest (28 per cent.) between the ages of 15 and 20, when deaths may be considered in an economic sense, the greatest loss. . . . The decline has been not much less considerable among both males and females at all other ages below 25, varying from 18 to 26 per cent. The mortality of males above 35 has diminished very little, but that of females has fallen between 14 and 24 per cent. until the age of 75, after which there is a trifling rise. The lives saved amounted to 3,966 males, 6,806 females; not less than 2,885 of the former, and 4,233 of the latter, were of ages 15-35, that is, among the most useful of the community.—Longstaff, "On the Decline in the English Death-rate." *Journal of Statistical Society*, June 1884, p. 220.

⁶ BRITISH MEDICAL JOURNAL, September 30th, 1882.

certain modes of life, enjoy, even though, like the North American Indians in Dr. Rush's time, they are in contact with phthisical communities; such a reference will make 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" (*loc. cit.*, p. 695).

It would seem that in the circumstances of human life it can but rarely happen that the virus is so concentrated as directly to set up the disease in the manner in which it can be set up by the artificial conditions of experiment. It would be rash to say that in no conditions of human association can the degree of concentration be reached which might induce tuberculosis in a healthy person. I have never myself seen an example of the poison thus directly operative. Other observers think they have met with instances. The results of the Investigation Committee inquiries on this point are as yet highly inconclusive; but the matter is still under investigation by them, and we may look presently for a most complete and valuable report. Many striking instances of apparent communication from husband to wife have been reported, but how many thousand instances to the contrary have been left untold? The passage of bacilli with the spermatic fluid is admittedly impossible. It is difficult to imagine how three wives should perish from the exhalations of a man with dormant phthisis. Dr. Andrew well points out that during a considerable period of this chronic malady the husband is still about, and requires no close nursing, so that "the danger of personal contagion is fairly equal between the two sexes for a very long period, whichever may show the first symptoms of the disease." Dr. Andrew thinks that the contraction of syphilis helps to turn the scale so heavily against the wife. I should myself hesitate to accept this argument.

How far can we still say that phthisis is a constitutional disease? By the constitution of a man I mean his build, the integrity or otherwise of the tissues of which each part of his body is made up, and the wholesomeness or otherwise of the juices with which they are bathed; the sum of his vital force, his cell-quickenings power, which shall bear the call of judicious expenditure for a long or but a brief period of life. The constitution of a man is born with him, and nourished with his years of growth, after which period any depravation is a result of disease or of wilful damage. Constitutional disease, or tendency to disease, is, I take it, a badness of the original "bricks and mortar" of which the man is built which renders him in one or more points vulnerable in the vicissitudes of life.

Hereditariness and the illnesses of children, have necessarily much to say with regard to original and acquired defects of constitution. The vices, evil circumstances, and diseases of adult life to its impairment and wilful damage.

Inheritance is a means by which an acquired feature, whether good or bad, in a man becomes a constitutional factor in a certain proportion of his offspring. There are some points in the heredity of phthisis which have been recently worked out from the Brompton Hospital records by my colleague, Dr. Reginald Thompson (*The Different Aspects of Family Phthisis in Relation to Heredity and Life-Assurance*, by R. E. Thompson, M.D., 1884), which have an important bearing upon the question of the constitutional nature of phthisis. In brief, Dr. Thompson (p. 131 *et seq.*), in his able statistical exposition, shows that heredity precipitates the date of attack in phthisis by from five to ten years, and increases its acuteness and mortality. That in females, *paternal* heredity, whilst precipitating period of attack, also increases resistance, rendering cases less severe and of longer duration than in acquired cases for the same sex. In males, *maternal* heredity on the other hand, *lessens* resistance, increasing the acuteness and fatality compared with acquired cases. Atavism, when added to immediate heredity, has cumulative influence, anticipating period of attack to extinction of the family. Dr. Thompson distinguishes three modes of inheritance, namely,—1, by "maternal contagion;" for example, small-pox, in which the transmission is only from the mother, the development of disease rapid, the disease of infant protection against future attacks; atavism impossible. This form of heredity is obviously quite different from that of phthisis. 2, "Parental infection;" for example, syphilis, in which transmission is from either parent, the disease in child *consequent* only upon declared disease of parent, protective against future attack, atavism unknown. Again different from phthisis, especially as regards protectiveness and atavism. 3, A form of heredity to which Dr. Thompson does not attach any name, but which one might speak of as trophic or morphological inheritance, for example, insanity, monstrosities, etc. Here the transmission is from either parent; the disease may be consequent upon, or in anticipation of, declared disease in the parent; the disease in the offspring is not protective but predisposing to future attacks; atavism is a frequent and important characteristic. Dr. Thompson points out

that the inheritance of phthisis is identical with that of insanity in every respect, and points to the probability that phthisical inheritance is a condition of diseased structure, not dependent upon, but prone to, tubercle. Animal poisons, such as syphilis, and diseases such as phthisis, much contrasting in this, that whereas the syphilitic poison is so diminished by inheritance as to be completely extinguished in the third generation, the phthisis and insanity heredity gather force with each succeeding generation to the extinction of the family (*op. cit.*, p. 143).

With regard to what may be called acquired constitutional phthisis, the condition known as scrofula, which is specially characterised by a proneness to gland inflammation, affords an accepted, and, in accordance with the observations of Buhl and Villemain, a potent cause of phthisis. Must these glands become first invaded by the bacillus organism before they become infective? Probably there are plenty of germs inside and outside of us for the purpose, but how are these gland-processes set going? Is it not through the agency of measles, whooping cough, scarlet fever, enteric fever, and their attendant lesions; or of eczema, carious teeth, and the like sources of irritation?

We must, I think, allow that phthisis commences frequently with acute catarrhal symptoms, with local inflammatory symptoms and signs, or with apex "congestions" arising insiduously under evil conditions; and that these lesions may clear up, or may go on to ulcerative destruction and phthisis. These conditions give us the signs of incipient phthisis, and they determine the seat of the lesion should it become confirmed. They appear to me to be at least generally attributable to constitutional or acquired delicacy, *plus* an exciting cause, be it cold, or dust, or hæmorrhage. Engrafted upon these primary lesions comes the bacillus parasite, imparting to the lesion its infectiveness, its invasiveness—in a word, its malignancy.

Again, the lesions of phthisis are by no means to be all explained by the pranks and requirements of the bacillus, although very probably the miliary lesions, so frequent in phthisis, are so accounted for. I have had the opportunity, through the kindness of my colleague, Dr. Percy Kidd, Pathologist to the Brompton Hospital, of examining many sections prepared by him, and I have been much struck by the haphazard and, apparently, purposeless way in which the bacilli are scattered about the lesions of phthisis, the only point approaching uniformity being their tendency to collect round microscopic and larger cavities, as though they needed air for their free development.

We have, then, in phthisis a first, let us call it a pre-tubercular, stage corresponding with the catarrhal stage of Niemeyer, recognisable by signs, determining the seat of further lesion, and of inflammatory nature. We again, in the later stages of the disease, see Addison's dictum illustrated—namely, that "inflammation constitutes the great instrument of destruction in every form of phthisis."

Koch and his predecessors show us how the lesions of phthisis tend to be inveterate; why they are infective; how, under certain possible conditions, they may even become contagious. Our means of diagnosis have been added to by their researches, and new methods of treatment and prophylaxis suggested. Let us not think, however, that we can run away from the bacillus organisms. They always are, always have been, and probably always will be with us. We may diminish their numbers by millions, but millions will still remain; and, even if we could exterminate the bacillus, but failed to give heed to the culture of healthy families, the maintenance of healthy conditions of life, and the timely treatment and thorough cure of local chest inflammations, and of the sources of gland irritation, it is my belief that consumption would remain with us, and another bacillus would be evolved.

The PRESIDENT (Dr. J. W. T. SMITH) said that, on this question on the contagiousness of phthisis, the last idea that would have been suggested to him by his professional experience was this of the contagiousness of this disease. He had never met a single case which, in his view, would have justified that conclusion.

Dr. WARD COUSINS (Portsmouth) said that the discovery of Koch's bacilli in connection with morbid changes called tubercular, had exercised a remarkable influence on the application of antiseptic inhalation. The questions, however, which surrounded the recent doctrines were absolutely settled; still, this modern treatment appeared to be anticipating their final demonstration. There was not one particle of evidence to show that any kind of antiseptic vapour, however inhaled as a drug, could destroy micro-organisms lodged in the substance of the lung. The revival of the doctrine of the contagion of phthisis, too, had stimulated the adoption of antiseptic inhalation. Some considered that the evidence obtained by the inoculation and cultivation of bacilli, gave great force to the old theory, and so they trust, by a com-

plex instrument, to filter or sterilise the infused air. No respirators could keep out germs, and no respirator could be persistently employed by the patient. If any cover over the mouth and nose could keep germs out, surely this instrument ought to be worn, not by patients labouring under phthisis, but by those exposed to the danger of breathing an infected atmosphere, loaded with material thrown off by those actually labouring under the disease. Close inhalers were actually injurious, because they fostered the prejudice against fresh air and good ventilation. Patients would put on these, and then sit all day in a close and stuffy atmosphere. Antiseptic inhalation did actual harm if it interfered with the hygienic surroundings which were essential to the successful treatment of the disease. Dr. Cousins believed that antiseptic inhalation was not destructive to floating germs nor to micro-organisms lodged in the substance of the pulmonary tissue; still, it was very useful in all forms of chest-disease, as well as in phthisis. It did good by promoting expectoration and exciting more healthy secretion, and it was no doubt absorbed by the moisture of the mucous surfaces. In tubercular disease, it helped the expulsion of those materials which were the result of inflammation and degeneration, and which were ever liable, by absorption and diffusion, to create other centres of local mischief. It also promoted full inspiration and expiration; and this was a great element in the successful treatment of chronic disease. The chest was badly expanded in phthisis, the air-passages became plugged with mucus, and the balance between inspiration and expiration was lost; but inhalation promoted the activity of the pulmonary functions, and helped to maintain the normal state of those portions of the lung not invaded by the tubercular changes. Dr. Rameidge, fifty years ago, introduced inflation as a local treatment for phthisis, and his method was now resuscitated, with the single addition of antiseptic vapour. All who adopted antiseptic treatment were united in the opinion that the treatment must be extended over a long period; but there was a great difference of opinion about the inhaler to be used. His opinion was that inhalation should be open, not closed; that it ought to be a voluntary method, promoting full and forced inspiration, and that the instrument ought to be as simple and light as possible. Closed inhalers irritated the skin of the face, and sometimes occasioned dyspnoea, and caused oppression and headache. Dr. Ward Cousins exhibited the lightest inhaler yet introduced. It consisted of a little receptacle made of vulcanite, which could be used both for oral and nasal inhalation. It was suspended with a wire over the nose, did not require to be removed for coughing and expelling the sputa, and weighed only 72 grains.

Dr. GAIRDNER (Glasgow) said he had a serious difficulty in now addressing the Section on this subject, more especially as, two or three years ago, he had had an opportunity, before Koch's great discovery, of expressing his views at considerable length; and he did not feel he had yet attained to that clearness of view that would enable him to say anything, at present, of more authority than any other man. The very thoughtful address of Dr. Powell almost added to his feeling of disablement, because the idea of his taking up any of the enormously diversified topics of that address, within the time at his disposal, seemed to be quite out of the question. Referring to the well known views on this subject of Dr. H. Mac Cormac of Belfast, he (Dr. Gairdner) had never been able to adopt them; indeed, he could not conceive it possible that anyone, in the present state of pathological science, could adopt them to the full extent; he had always given Dr. Mac Cormac the credit of having, as it were, struck a vein of thought that it was very important to have well before their minds; his view being that re-breathed air was the sole cause of phthisis, and that tubercle was a sort of carbonaceous material accumulated in consequence of that. Although, in its essential part, Dr. Mac Cormac's view was only a pushing to the extreme of views advanced before, yet it was of enormous importance to insist upon, and, if possible, strike home, the idea that the dissemination of tubercle had, in all probability, more connection with impurity of the atmosphere than with any one or other working causes. This was shown in a multitude of ways, as in the proclivity to tubercle of prisoners in gaols under the old system; of animals living in close confinement, and closely in air very frequently re-breathed; and, generally, in circumstances where there was a massing together of human beings under conditions of imperfect ventilation. Sanitary authorities, while they said that the conditions of the soil and a great number of other conditions entered into the causation of phthisis to a very considerable extent, were almost unanimous in saying that the disease, on a large scale, was propagated under the conditions which propagated the epidemic and endemic diseases generally, more than under those accidental conditions to which it was commonly attributed. Phthisis had a great deal more to do with imperfect ventilation and respiration, in the practical sense, and

on the large scale, than it had to do with cold, or east winds, or northerly, or any winds they liked to name, or any particular season. These were merely trifles, as compared with the other and larger cause. But the same lesson was derived from the national statistics of phthisis, and to that comparative diminution in districts where, although there might be a good deal of imperfect ventilation, it was true there was very much less of the massing together of human beings. What was necessary to be done was to bring all this into relation with Koch's view; for he believed in the bacillus which Koch had discovered, and thought that Koch had announced and presented his discovery with remarkable and judicious reserve as regarded its ultimate consequences in pathology, saying he would not anticipate these. But there was another aspect of the subject, and a not unimportant one. There were certain circumstances in which re-breathed air would not produce phthisis, while at the same time producing the most disastrous results, and utterly destroying the lung itself. What was that condition which was known as the colliers' or coalminers' lung? It was not so often seen now, though he used to see it very frequently in the pathological department of the Edinburgh Infirmary. It had now become a rarity, and he did not think he had seen more than two or three cases in the last four or five years. And this rarity could be attributed to nothing else, in his opinion, but the much greater care which was now taken in regard to the ventilation of mines than was formerly the case. Now, the essence of this disease consisted in the inhalation of carbon, the very material out of which Dr. Mac Cormac thought tubercle was manufactured. He did not deny that miners' lung might coexist with phthisis, and, if a miner had phthisis, or was predisposed to it, that he might have his condition advanced and its result accelerated by living under the horrible conditions which used to obtain. But the miner who was not phthisically disposed went on to the end of the chapter without the least evidence of tubercle arising in the lung. He became dropsical, or had dilated heart, it might be; he spat up lung-substance in connection with carbonaceous material—stuff like Indian ink more than anything else—and his lung was like a sponge, out of which they could squeeze masses of carbon, and the lung be absolutely ruined; and yet would not have phthisis. And, therefore, it was perfectly clear there was another factor in the production of the disease than impure air. There was here a predisposing element, but they must have another element. On looking at the hereditary aspect of the thing, it was just the same story over again. He could not imagine a practical physician, who had argued over this subject with himself for a series of years, could doubt the hereditary character of phthisis—that was to say, that there was something which was transmitted from the parent to the child that gave a family disposition and constitution, making the individuals of the family more liable than others to undergo phthisis. It was not known what that something was, and it was impossible to suppose it was communicated through, or entered into, the ovum, or was directly communicated in any way from father to child. But the constitutional peculiarity, whatever it was, that entered into this family, made them a soil on which, in course of time, the bacillus would grow. And, although the case of phthisis was a very extreme instance, no doubt, of deferred action of the peculiarity, and a very difficult one to understand, and which it would probably take long years yet before they had thoroughly worked it out, yet they were not without analogies in this respect. What was gout? It was a family and hereditary disease, and yet men in the affected family might pass forty years of their lives without any manifestation of it. How was the disposition kept up, and how did it come to declare itself after so long a period? But it was not, perhaps, a case very much in point, because they did not know that gout was a bacillus or germ-disease. But in respect to a disease which unquestionably was this, something was known which might guide the mind as to the causative relation of phthisis. Diphtheria was most unquestionably a germ disease, at least very few doubted that; and yet to understand its incidence as regards individuals of the human race under apparently identical circumstances, was most extremely difficult, and not only as regarded individuals, but as regarded families. His own experience would hardly have reached so far, for he saw few cases of diphtheria; but one of his surgical colleagues, who had perhaps operated oftener in cases of diphtheria in children than any other man in his town, told him he had over and over again been struck by the fact that, one child in a family having diphtheria, not only would other children get it, and in circumstances where there was no prospect whatever of infection being communicated from one to the other, but there was a family history of uncles and aunts and cousins being liable to diphtheria. His own earlier and later experience, together, had taught him the same lesson, although in a less pointed way. He had happened to be very intimate with one family, where the children were taken off

with diphtheria—he thought to the extent of three or four in succession, at intervals of years, preserving their health in the intervals of time perfectly well. First one infant died, and then there was a lapse of two or three years, then another child died, and then there was another lapse of two or three years, and then another died, and so on; and that in the face of the fact that there had been changes of residence in the interval; so that the whole of the external conditions of the family were different. They could not call that anything less than a family proclivity—or something in that family, which of course must have been transmitted hereditarily, and yet would not create the disease unless the germs were there as well. Let them apply those conditions to the much more chronic conditions of phthisis, and they were able to see a little at all events in the direction of a solution of the mystery. Considerations like that, and many others which he had not time to enter upon, had always prevented his accepting the views current for a great number of years in Germany, which made the whole thing consist in catarrhal inflammations of the lungs, and attempted to deprive phthisis, as it were, of its specific character. The views of Niemeyer had always seemed to him to be a perfectly extravagant statement of the case, and one which could not commend itself to anyone who viewed the case largely from the point of view of the clinical observer. The immunity of certain persons under exactly these conditions, of children who would go on taking catarrh after catarrh, and not become phthisical, who would get emphysema of the lung, or dilated heart, and get bad coughs, but who did not become tubercular, negated that view. Something there was over and above all this of a more specific nature. He had taught that against Niemeyer, before the advent of Koch and his bacillus, and certainly the bacillus view seemed to add what was necessary to supplement the objection. But how the whole thing was to be brought into relation with Koch's discovery, and the two things to be brought into a harmonious system of doctrine, he confessed he did not see clearly at present.

Dr. Ross submitted a paper by Dr. Henry Bennet, which Dr. Bennet had forwarded, as he was unable to be present through indisposition (see page 704).

Dr. MYRTLE (Harrogate) said he was struck with the remarks of Dr. Gairdner as regarded the influence of the air upon the lungs of the colliers. He was a fellow-student of the Professor's at Edinburgh, and well remembered the frightful ravages worked among the stone-workers in Edinburgh through the stone-dust, and that these men, who fell victims to consumption readily, were nursed by their wives and daughters, often in a box-bed, and yet he had failed to find a single case where the disease was communicated from the sick to the healthy. As regarded the inhalation of coal-dust, he attributed the improvement in the condition called coalminers' lung to the fact that the mines were now in a more favourable sanitary condition. The inhalation of coal-dust produced a certain deposit in the lung, but, strange to say, never any tubercular mischief. So well known among colliery-surgeons was this condition, that the late Dr. Hamilton, of Falkirk, father of the present Professor Hamilton, of Aberdeen, thought that the inhaled carbon was a cure for phthisis, and he invented a machine for inflating the lung with an artificial carbon—practically lamp-black. Dr. Hamilton had tried his experiment upon him (Dr. Myrtle), and, by means of a pair of bellows, blew into his lungs an amount of lamp-black that had blackened them till now. The remark which Dr. Powell made as regarded the progression of the disease in father and son—namely, that the father became affected with bronchitis, and the son, following the same occupation, with true phthisis, he (Dr. Myrtle) had seen borne out by several examples, among sculptors principally; and he had himself, he believed, saved several young men from phthisis by causing them to give up an occupation which was the inheritance of the family; with us, as with many other nations, the son very frequently following the father's occupation. Coming to consider the question of the inhalation of dust as inducing tubercular disease, he dared say they would all remember that Professor Simpson was very much struck when walking through the cotton-mills of Lancashire and Yorkshire to find the girls so liable to bronchitis, and frequently to phthisis; whereas the worsted spinners, working under apparently the same conditions, were a healthy, robust body of people, little liable to this affection or phthisis. On closer examination, he jumped to the conclusion that the immunity of the worsted workers was due to their inhaling particles of oil, and he thenceforward set to work, and cured his cases of phthisis by inunction with oil. He (Dr. Myrtle) lived on a very high table-land, where consumption was exceedingly rare, and those who were affected with it managed, somehow or other, to exist, and for a long period of years. On the other side of the table-land, within a quarter of a mile of where he lived, there was a declivity leading to a low portion of ground, inhabited by perhaps two hundred or three hundred families. This spot afforded a greater num-

ber of cases of consumption than occurred among the ten thousand inhabitants of the high tableland. He was not able to explain it, but there was the fact. Dr. Ward Cousins had shown them a new means of inhalation for the distension of the lungs. When he (Dr. Myrtle) had joined his late partner, he said he should have died of consumption had he not taken it into his head to begin to take snuff, which had caused him to sneeze most tremendously. From then he had gone on snuffing and sneezing until he was then fifty-seven years of age. He had attributed his cure from his consumptive tendencies to his forcibly dilating his lungs in the act of sneezing. The question of the bacillus, he must confess, he approached with great difficulty. It was a new theory, a new fact to him, and, like the President, he scarcely felt at present ready to accept the doctrine of the bacillus as the cause of tubercular phthisis. He thought they would require to wait a little longer, and learn a little more from the researches of those who were engaged in this branch of pathology, before quite accepting this view. If the bacillus were floating about the atmosphere, as it was represented to be, they had Dr. Powell, as he had himself remarked, living in an atmosphere of them, and yet he lived to tell the tale.

Dr. BARR (Liverpool) said there had been an incidental remark in this address regarding the prevalence of phthisis in prisons. He was medical officer of Skipton prison, in which acute phthisis was tolerably permanent when he first became connected with it, and it had, in fact, been the bugbear of the institution. To him, at any rate, it had proved a bugbear. Whilst they were troubled with acute phthisis, it was one of the healthiest prisons in England, the death-rate never exceeding an average of 6 per 1,000, while it was usually about 3 per cent; so that, if they could get rid of this acute phthisis, it would become almost a sanatorium. Dr. Powell had alluded to dust as a causation of phthisis. He had tried to get rid of this in connection with their prison, finding it gave rise to a great deal of catarrh. But a great number of the phthisis cases did not commence with catarrh; they began almost invariably with a rise in temperature, but, after a few weeks, they soon began to manifest the true nature of the case. The temperature was of the kind they got in typhoid fever, with rapid breathing, and, after a little, expectoration; this sometimes containing bacilli, always towards the end of the case, while sometimes, towards the commencement, it was exceedingly difficult to detect any. However, it was a very intractable form of the disease, the majority dying of it. When he went to the prison first, it was, as now, treated with hot air, and the entrance for external air consisted simply of a very small opening. His idea was to get that very greatly enlarged. In one wing, especially where chest-affections were very prevalent, including phthisis, he had the ventilation increased, and in that wing these affections diminished at once. The Commissioners, certainly, had carried out all his suggestions, so far, with the greatest willingness; and would, no doubt, greatly improve matters still further, as he saw his way to make suggestions. However, they had had two cases this year, and this was sufficient indication for him to go on, and try and get rid of this disease effectually. Another point was as to the hereditary tendency to this disease. In some of these cases there was no such tendency whatever to be traced, and the men who were fit for the very hardest work in the prison, were the most ready to get affected and die from it. He remembered one case in which the disease commenced in the dry form with no expectoration, until there was a regular breaking down of the lung-substance. He thought he might find some original focus for the disease, and could find none, but the man had suffered from hydrocele. On section, he found one of the testicles was an irregular caseous mass; but, in many cases, they could not find such foci; that being so, the cause must be introduced from without. In regard to inhalation, he was quite at one with Dr. Ward Cousins. One of these inhalers, he thought, was rather dangerous if there was any tendency to hæmoptysis, because he believed that the strong inflations which these called forth were very apt to give rise to this.

Dr. SNOW (Bournemouth) said that for many years he had tried antiseptics for the treatment of phthisis, and he had not yet found any which had the slightest influence on the life of the bacillus. After several months they would find the bacillus as lively and as active as ever. At the same time, he was inclined to think that the antiseptic-treatment had considerable value to the patient; patients all told them they liked it, as it aided expectoration and made it less offensive, and very much diminished the laryngeal irritability. In regard to the contagiousness of phthisis, as seen in chest or ordinary hospitals, he said he was connected with two hospitals, one a sanatorium for consumptives, in which there were sixty patients, and, the other, a small cottage-hospital for general cases. Not a single case had occurred in the staff of the consumptive hospital; while, within the last two years, in the staff of the cottage-hospital, where no case of ordi-

nary phthisis was admitted, one of the nurses and one servant had died from the disease. Within the last few years, he had noticed that phthisis was certainly more prevalent among widows of men who had died of phthisis than among the other population; but he dared say there were causes accounting for this, without saying the contagion produced it.

Dr. BERNARD O'CONNOR (London) said that, as to the existence of the bacillus tuberculosis there could be no doubt; but if he were asked if the bacillus were the cause of phthisis, he would unhesitatingly reply, "not necessarily so, any more than any minute irritant might be supposed to be an indirect cause." He believed that bacilli might be derived from each of two persons; the one apparently healthy, the other consumed by tuberculosis; the former he regarded as an unfavourable and the latter as a favourable ground for the further propagation of these micro-organisms. In the first case there was a barren soil, in the second case a marked receptivity on the part of the patient for these particles; and it was this element of receptivity that physicians attempted to imply in the phrase "tubercular diathesis." Dr. Powell had considered the question of the communicability of phthisis, and he had adduced the cases of the nurses and attendants at the Brompton Consumption Hospital, as evidence against the entertaining the idea of such transmission. He (Dr. O'Connor) had, however, been struck by many of the statements forwarded to the Collective Investigation Committee respecting this subject, and his own personal observation has led him to the consideration of evidence which he could not wholly ignore. He had several times contemplated cases of phthisis apparently transmitted from the slowly dying and presently dead husband to the wife. He had never met with a case of supposed transmission from the wife to the husband, or to any other member of the family. He could only explain this, in part, by the attendance of the wives on their husbands. He had frequently observed that cases of undoubtedly hereditary phthisis seemed to run a much longer course than instances of the same disease occurring in those whose family history was not quite so definite. He thought that all must confirm Dr. Powell's statement that abdominal symptoms in phthisis were now very much less marked, and of less frequent occurrence than seemed to have been the case formerly. With regard to the effects of antiseptic inhalations of different substances, he could not agree with all that had fallen from Dr. Ward Cousins. Antiseptic inhalation, whether by means of Coghill's, or Mackenzie's, or of any similar instrument, continued day and night for weeks and weeks, had, in some cases in his hands, been followed by marvellous improvement in the general bodily condition; the cough had been eased, lessened, and, in some instances, stopped; hæmoptysis had disappeared; nocturnal perspiration had been, after a time, almost forgotten; the appetite had been regained, and the weight, in the vast majority of cases, had been considerably augmented. He had generally found that patients strongly objected to inhalers. At the same time, he never lost sight of the importance of driving the patient into the open air wherever he might live, of making him respire deeply, and of limiting himself, if he could do so, to sixteen or eighteen respirations in the minute. At the same time, he was directed to give up alcohol in all its forms, and to drink milk freely. Professor Gairdner had acquitted cold winds of any share in the causation of phthisis. He might have given them the verdict, "Not proven." He (Dr. O'Connor) thought that the cold winds (and more especially the north-east wind) had an indirect effect in the direction of favouring the advance of phthisis, by causing ignorant persons to keep their windows sealed during the prevalence of these winds. The majority of dwelling-houses were constructed on most faulty principles; and, unless a house were well ventilated by means of Tobin's tubes, or of some such contrivance, it was absolutely necessary that the windows should be kept open at the top, so as to allow the escape at the upper part of all the deleterious particles arising from the respiratory tract, the skin, etc., of the occupants of the room, as well as of the results of the incomplete decomposition of coal-gas, and the large quantity of sewer-gas which entered most dwelling-houses. Professor Gairdner stated that, from the miner's condition, it was plain that re-breathed air was not causative of phthisis. He thought that Dr. Gairdner meant to say that it was not the carbon existing in previously breathed air that was chargeable with the offence, but something else; otherwise his statement would almost presuppose that re-breathed air contained nothing but carbon. As regarded Niemeyer's views in connection with catarrhal pneumonia, he thought that the whole of his teaching might be summed up by a single quotation, "Some phthisical patients may become tuberculous."

Dr. POWELL, in reply, said he wished to say a word with reference to one point, and that was thoroughly to endorse all that had been

said as to the best cure or the best treatment for phthisis. He should attach no special importance to the treatment of phthisis by inhalations. He thought that it was perfectly certain that inhalations of such strength as could be used had no earthly effect in killing the bacilli, but had much the same sort of influence on them as a London fog had upon us. The bacilli did not like them, and did not flourish so well with them as under other conditions; but they still flourished quite too well for the patient's welfare.

ON THE CONTAGION OF PHTHISIS.

Read in the Section of Medicine.

By J. HENRY BENNET, M.D.

WHEN the question of the contagion of phthisis was submitted collectively to the profession, I did not sign the paper sent to me, not feeling able to answer it either affirmatively or negatively. At the same time I then felt, and still feel, that my opportunities for professional observation during the last quarter of a century have been so great and so exceptional that my experience must be worth recording. About this time last year I published, in a Paris medical journal (*La Semaine Médicale*, July 3rd, 1883), a communication on this subject, which I believe exercised in France some little influence in stemming the present current of professional opinion, and I now reproduce the facts therein contained.

There is no ignoring the fact that the doctrine of the contagion of phthisis is rapidly gaining ground with the junior members of the profession, especially abroad, and that its general adoption would lead to much that is distressing to the patients, to their relatives, and to society in general.

If it were true that all or nearly all the cases of phthisis observed have their origin in contagion, as asserted by some prominent writers, such as M. Debove, the Paris hospital-physician, the fact must be accepted, however disastrous in its social bearings. It is asserted that, in a person having a constitution hereditarily predisposed to consumption, or prepared by anæmia from whatever cause, exposure to the breath and to the emanations of a consumptive individual leads to the inoculation, to the development of phthisis, and that most, if not all, cases of phthisis thus originate, that is, in contagion.

If this statement be correct, we cannot avoid the inevitable sequela. We must adopt the same preventive precautions with our patients as we do with those suffering from scarlatina or small-pox. Phthisis, however, is so much more universal a disease, causing, as it does, the mortality of from one-seventh to one-tenth of those who die in large northern towns, that the result of such a pathological discovery would be infinitely more trying to society. Indeed, social relations would be all but disorganised, a consumptive person would be considered like a leper in olden times, one to be separated from his family, to be isolated, shut up. He must be refused admission into hospitals or asylums. He would have to live months, years, in a tent. His clothes should be destroyed, and whether he dies or recovers, the home which he has inhabited should be burnt. It would be perhaps a charity to mankind to kill him at once, like an animal attacked with rinderpest; for, as the duration of the ordinary forms of phthisis may extend over years, during that period every time he breathed he would be filling the atmosphere with the germs of disease, wafted by the atmosphere to be scattered far and near. Such is the logical sequela of the doctrine of the contagion of phthisis carried to an extreme, as it is now being carried by many, especially in Germany and in France.

Fortunately, clinical experience shows that this doctrine is not the expression of facts; that, if phthisis be communicable from one individual to another—contagious, in a word—it is only so to a very limited degree, and under very exceptional circumstances. The history of the Consumption Hospital at Brompton, since its foundation, as brought forward by Dr. Theodore Williams, at the Worcester meeting, is in itself conclusive. Although the hospital has always contained large numbers of consumptives, in every stage of the disease, the mortality from that malady among the large staff of nurses, attendants, medical men, etc., has never been greater, scarcely as great, as that of the population of London in general.

My own personal experience corroborates these data. I went to Mentone in 1859, twenty-five years ago, in an advanced stage of phthisis, recovered in a very unhopèd-for way, and have practised there ever since. I was the first, or nearly the first, to draw the attention of the profession, at home and abroad, to the special adaptability of the winter climate of the Genoese Riviera to the treatment of chronic chest-disease, owing to its dry, sunny, equable character,